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ROYAL SOCIETY OF LONDON.

From March 6, 1862, *to June* 18, 1863 *inclusive.*

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ERRATA.

Pages 20 & 21, in the description of figures 3, 4, 5, 6, and 7 of Mr. Woollcombe's paper, *for* "actual size" *read* "$\frac{5}{8}$ of actual size."

Page 20, line 6 from bottom, *for* "spherical" *read* "spheroid."

„ 25, „ 15 „ „ *insert* ":—" after "discharge."

„ 25, „ 13 „ „ *for* "horizontal. The range," *read* "horizontal, the range."

„ 512, line 10, *for* c for 1300 *read* c for 13000.

„ 515, lines 2, 4, & 5, *for* 6842·5, $\bar{1}81{\cdot}0$, and 6838·1 *read* 6843·4, $\bar{1}82{\cdot}0$, and 6839·0.

„ 515, last line, *for* $\frac{B-b+c}{B+b}$ *read* $\frac{B-b}{B+b}+c$.

„ 516, line 6 from bottom, *for* even *read* ever.

Note to the Communication of Mr. Huggins and Dr. Miller, of Feb. 26, 1863:— "The first figure gives the spectrum of Aldebaran, the second that of Sirius, the third that of a Orionis, and the fourth the Solar spectrum—the left hand of the figure representing the least refrangible extremity."

OBITUARY.

Page iii, line 15, *dele* from "liable" to the end of the sentence.

OBITUARY NOTICES OF FELLOWS DECEASED

Between 30th Nov. 1861 and 30th Nov. 1862.

Peter Barlow, Esq., was born at Norwich in October 1776, and died at Charlton in Kent on the 1st of March 1862. After going through an ordinary school education, he chose mathematics as a special study, and obtained the place of Mathematical Master, and subsequently that of Professor, in the Royal Military Academy at Woolwich. During the earlier part of his career he gave his attention chiefly to pure mathematics; and his first original work was 'On the Theory of Numbers,' which appeared in 1811. In that work he shows an acquaintance with the writings of foreign mathematicians to an extent at that time unusual. In 1814 he published a Mathematical and Philosophical Dictionary; also his well-known Mathematical Tables, which, besides other cognate matters of use and interest, gives the factors, squares, cubes, square roots, cube roots, and reciprocals of all numbers up to 10,000. This work having passed out of print, and a decided opinion being entertained by some discriminating authorities of the practical usefulness of its chief contents, the tables, so far as indicated above, the factors excepted, were republished in stereotype under the sanction of the Useful Knowledge Society in 1840.

In 1817 Mr. Barlow published a work on the Strength of Materials, based on extended experimental inquiries carried on by himself. In this way he was brought into friendly relation with the leading engineers and architects of his time, was much consulted in reference to important works of construction, and served on more than one Government Commission on great engineering questions. He next turned his attention to the subject of magnetism in general, and especially to the deviations of the compass-needle caused by local attraction, and the best means of correcting it. His researches form the subject of his 'Essay on Magnetic Attractions,' published in 1820, and of seven papers communicated to the Royal Society from 1822 to 1833. These labours of Mr. Barlow were so highly esteemed, and the method he devised for correcting compass-errors, although confessedly not perfect, was at the time deemed of so great practical value, that in 1825 he received the Copley Medal from the Royal Society "for his Various Communications on the Subjects of Magnetism."

Mr. Barlow also applied himself to the improvement of achromatic object-glasses, and in 1827 communicated a paper on that subject to the Royal Society. In the further course of his inquiries he was led to try the effect of substituting, for the flint-glass lens of the usual achromatic combination, a concave lens formed of transparent fluid enclosed in a glass capsule having surfaces of appropriate curvature. The idea of employing fluid lenses, it may be observed, was not new: it had occurred to Newton and David Gregory, and had even been practically applied by Dr. Blair of Edinburgh. Mr. Barlow made choice of sulphuret of carbon as the fluid, which, with a refractive power about equal, has a dispersive power more than double that of flint-glass. He demonstrated the practicability of his method by constructing two telescopes on that principle, with a result sufficiently promising to obtain for him the support of the Board of Longitude in the further prosecution of his experiments. The Council of the Royal Society also, sensible of the important scientific bearing of Mr. Barlow's proposal, engaged Mr. Dollond to construct a fluid-lens telescope under Mr. Barlow's superintendence, and submitted the instrument to be practically tested by competent judges in order to decide on the expediency of constructing a telescope of much larger dimensions on the same principle. The trial instrument is described by Mr. Barlow in the Philosophical Transactions for 1833; and Reports on its performance, by Sir J. Herschel, Mr. Airy, and Captain (now Admiral) Smyth, will be found in the Proceedings for December of the following year. From the trials made, it appeared that Mr. Barlow's principle might be advantageously applied to the construction of a great refracting telescope to be employed in the observation of nebulæ and for certain other astronomical purposes; but the project seems not to have been further proceeded with.

Besides publishing the memoirs and treatises mentioned above, Mr. Barlow during his active career was a large contributor to the 'Encyclopædia Metropolitana.' He retired from the duties of his Professorship in 1847. In May 1823 he was elected into the Royal Society. He was one of the original Fellows of the Astronomical Society, and a member of several of the leading Societies in Europe and America.

Jean-Baptiste Biot, the last of that powerful school of science which grew up during the first French revolution, cannot here be the subject of a detailed scientific biography. The wide extent of his labours would alone render this difficult; and when it is added that a large part of this extent contains matters in which the position of Biot and of others could not be discriminated in few words, difficulty merges in practical impossibility. And this is rendered still more obvious when we state that we do not so much refer to actual points of disagreement commenced and continuing, as to matters in which anything short of a minute and cautious handling would probably create new discussions which had better find a natural origin in the statements of professed historians. Of these matters some are of very old date, and may therefore be said to have passed into history; while this very circumstance makes it more desirable to dwell especially upon the personal life of one who was born under Louis XV. and lived to the age of eighty-eight in the full enjoyment of high faculties. Of this personal life we are able to give some account from documents on which we can rely.

Biot was born at Paris, April 21, 1774. His father, Joseph Biot, was an *employé* at the Treasury, whose ancestors had been farmers in Lorraine. The son, after a classical education at the college Louis-le-Grand, and some instruction in mathematics from Mauduit, was placed, against his wish, with a merchant at Havre, who employed him in copying letters by the thousand. Disgusted with this occupation, he volunteered for the army as soon as the legal age of eighteen was attained, and served as an artilleryman in the army of the North at the battle of Hondschoote in 1793. Declining the promotion offered on condition of permanently engaging himself, he remained a few months, at the end of which a severe illness made him desirous of returning to his parents. The military authorities were very slow about the dismissal of volunteers who were likely to be useful, so Biot took his departure for Paris, with nothing but his serjeant's certificate, in September 1793. Walking feebly along the road, he was overtaken by a smartly dressed person in a cabriolet, who invited him into his carriage, and entered into conversation with him. Finding that he was going to Paris, the stranger pointed out the danger of his purpose, a recent ordinance having made it death for soldiers to approach the capital. Biot

persisted, and his companion then offered to take him all the way. This stranger, whoever he was, gained their free passage through military posts, and sent patrols about their business by a mere whisper. At Compiègne, the young volunteer was summoned before a revolutionary committee, then and there sitting, on the evidence of his uniform. But his examination had hardly commenced when his companion entered the room in violent anger and addressed strong reproaches to the Committee, which were answered by humble apologies. When they arrived at Paris, Biot desired to know the name and address of his protector, and was answered *St. Just,* Rue de la Michodière, Hotel X———. The story, one would suppose, ends here with full explanation of all that had taken place. But when Biot, after an illness of several weeks, presented himself at the address given, he was told that no such person had ever lived there. In later years Biot made many efforts to clear up the mystery, but never could get beyond a doubt. So far the notes from which we write. We add that it is notorious that the formidable leader of the revolution was on his way to Paris about the time in question, having been commissioner to the army of the North; and he was the *élégant* which Biot describes his friend to have been; but this was, of course, known to Biot. It may be surmised that the person really was the colleague of Robespierre, who, knowing that the power he had shown would necessitate the inference that he was very high in the state, and render his detection easy, chose to give his real name, but also to hint that further acquaintance would be inconvenient, by giving a wrong address. He was guillotined in July 1794, so that Biot, enfeebled by illness, probably had no opportunity of seeing him in public.

Biot was admitted into the school of *Ponts et Chaussées,* and into the Polytechnic School at its opening. He formed the acquaintance of Poisson, and the two became the favourite pupils of Monge. But Biot, Malus, and some others who had smelt powder, took part in the insurrection of October 1795, the suppression of which by grape-shot was the first very notorious achievement of Napoleon Bonaparte. Biot found refuge at Melun; but the names of the insurgent students were known, and they would have been expelled from the Polytechnic School if Monge had not interfered by the declaration that they were among his best pupils, and that if they

were dismissed he would retire with them. Monge was more than once the protector of the school. The Emperor, when he gained this title, felt strongly that the students were his enemies, and seems to have meditated their dispersion. "We had work enough," said Monge to him, "to make them republicans; give us a little time to form them into monarchists: you yourself must agree that you have turned that corner rather sharply." Napoleon did nothing: and he lived to call the school the goose which laid him the golden eggs.

Biot's next step in life was to a chair of mathematics at Beauvais. At this place he gained the acquaintance and correspondence of Laplace by an offer to correct the sheets of the 'Mécanique Céleste.' He has given, in the Journal des Savans, an anecdote which is very honourable to Laplace. While at Beauvais he married the sister of his friend Brisson, whose family resided there. Neither had any money, either in possession or reversion; so that all except lawyers will share Biot's wonder when he found that the notary had contrived a contract of marriage twelve pages long. Madame Biot had been very well educated, and the little stories and dramas which she wrote for her children were celebrated in her circle. She learnt German in order that her husband, at the desire of Berthollet, might publish a French edition of Fischer's work on physics; but the actual translation, watched of course by her husband, was her own. The first edition was published in 1805. In 1799 Biot was appointed an examiner of the Polytechnic School; in 1800 he was removed to Paris as Professor of Physics at the Collége de France, and was made an associate of the Institute, of which he became a member in 1803. The other dates which we ought to give are as follows. He was appointed, with Arago, to the continuation of the measure of the meridian, in August 1806; with Mathieu, to determine the pendulum at Bordeaux, August 1808. He became editor of the Journal des Savants, May 1816. He went to Scotland and the Shetland Islands for the measurement of the pendulum in 1817; to Dunkirk, with Arago, to act in concert with an English commission for the determination of the latitude, in 1818; to Illyria and the Balearic Islands, for the pendulum, and to Spain for the repetition of measures connected with the great survey, in 1824–25. He was made a Foreign Member of this Society in

1815, and obtained the Copley Medal in 1840. He died at Paris, February 3, 1862.

The other dates, &c. of his life will be found, given by M. Lefort (the son of his daughter's daughter), in the 'Nouvelles Annales de Mathématiques,' 2nd series, vol. i. The list of his writings, associated and separate, is under 477 heads; and this list, says the collector, is certainly incomplete. Of his separate works should be especially mentioned the 'Astronomie Physique,' 1st ed., 1805; 2nd, in 3 vols., 1810–11; 3rd, in 5 vols., 1841–57; the 'Traité de Physique Expérimentale,' 4 vols., 1816; the 'Précis' of the same, 1st ed., 1817; 3rd, in 2 vols., 1824; 'Recueil d'Observations Géodésiques' (vol. iv. of the 'Base du Système Métrique'), 4to, 1821; the edition (in conjunction with M. Lefort) of the 'Commercium Epistolicum,' &c., with additions, 4to, 1856. The works on Indian and Chinese astronomy can hardly be given apart, without the writings on the same subject in the journals.

How completely Biot was devoted to his occupations sufficiently appears. The indomitable energy of his character was associated with a strong feeling of personal independence. With the pride of a republican he refused, before his election to the Academy of Sciences, to pay the usual visits of ceremony to his future colleagues. This he afterwards regretted; and, as a kind of expiation, he made it a rule, until at last his friends insisted that he should spare his extreme old age the fatigue, to pay a visit to every new member of the Academy, so soon as his election was made certain.

In 1803, his son Edward was born. This son, after a respectable career in science and engineering, took a dislike to such pursuits, and applied himself to literature, and especially to the study of Chinese. He died in 1850, a member of the Academy of Inscriptions, in which he found himself the colleague of his father; for it should be noted, as a thing which is, we believe, unique, that Biot died a member of three of the four academies, being also elected to the Académie Française in 1856. The mother survived her son two years; and these losses were the great misfortunes of the father's life.

The name of Arago will always be associated with that of Biot. Arago in his early youth (he was but twenty-four years old when he gained his place at the Institute, after his return from captivity

at Algiers) had distinguished himself to an extent which induced Biot to make it almost a condition that the young man should be appointed his assistant, before he would undertake the conduct of the survey. When a place in the astronomical section of the Institute became vacant, Poisson was thought of as a successor to Lalande, with every chance of success. Biot protested, and urged strongly to both Lagrange and Laplace, that the astronomer ought to be a person conversant with astronomy, and that Poisson's future chair ought to be one of geometry. Lagrange gave way at once—"Vous avez raison," he said, "c'est la lunette qui fait l'astronome." Laplace was harder to convince, but yielded at last.

In 1809 Biot obtained those apartments in the Collége de France which he occupied with hardly any intermission until his death. We have heard it said that he never left Paris for one single night during fifty years: this is probably not literally true, but is certainly very near it. In the same year (April) an imperial decree named him professor of astronomy in the new University then founded. Biot had not been an Imperialist; and the appointment was a free testimony to his merit. In 1804 he had endeavoured to prevent the Institute from expressing an opinion in favour of the new *régime*, on the ground that a scientific body should not meddle with politics: this opinion he always maintained. The police were well aware that he had assisted Benjamin Constant, Andrieu, and perhaps other frequenters of the house of Madame de Staël, in the composition of a satirical piece which had great success in such private circulation as could be safely given. Fouché had charged Laplace to tell his young friend to be a little less witty and a little more prudent. Biot, as might be expected, obtained no very great patronage from the Emperor. He had a turn for dry satire, which, under very effective restraint, is visible in his controversial writings; and he had the mode of delivering a sarcasm which tells. In 1800, Roederer, then high in the direction of public instruction, paid a visit to the Collége de France, and, surrounded by the professors, read them a lecture on their functions, recommended practice in preference to theory, and pointed out geometry and algebra as not good for much. "Cependant," quietly remarked Biot, "la géométrie a du bon pour l'arpentage," to which the other was unfortunate enough to assent in a manner which showed he did not understand

the answer. The amusement which this excited led Laplace to tell the story to the First Consul, among whose few objects of reverence the mathematics stood very high. Roederer accordingly had to encounter one of those *bourrasques* by which Napoleon is so well known. "You are a pretty ignoramus not to know that mathematics is the root of human knowledge. The young man served you right when he turned you into ridicule; and you could not even see what he was at."

If such anecdotes appear to be unusual in our notices, it may be remembered that these accompaniments would, in most cases, be of too recent a character. We insert nothing but what is more than half a century old, and we proceed to a few words on Biot's scientific life.

Over and above separate works, fifteen in number, the scientific life of Biot is recorded in 60 articles of the Journal Philomathique, 119 of the Comptes Rendus, 3 of the Journal of the Polytechnic School, 8 of the Connaissance des Tems, 41 of the Annales de Chimie, &c., 22 of the Memoirs of the Academy, 1 of the Savans Etrangers, 83 of the Journal des Savans; and of accounts and criticisms, 37 in the Moniteur Universel, 35 in the Mercure de France, 1 in the Journal des Débats, 5 in the Journal des Mines, with 23 articles in the Biographie Universelle, 9 in the Mémoires, &c. d'Arcueil, 1 in the Academy of Inscriptions, 2 in the Revue Britannique, 6 in the Revue ou décade Philosophique, &c., and 8 in the Nouvelles Annales du Muséum d'Histoire Naturelle. In this large mass of results the author appears as an observer and experimenter, as a critic and historian, and as a teacher and elementary writer.

As an astronomical and geodetical observer, Biot has long had his place in history; to discuss that place would require the discussion of critics, historians, and subsequent observers. As an experimenter, we cannot undertake to describe that long train of which Professor Forbes, in his elaborate sixth dissertation of the 'Encyclopædia Britannica,' says "the number and variety of his experiments and writings almost baffles enumeration." There is no part of physics into which he did not carry his researches; but of all he was most devoted to optics. Here the point which has been most signalized by historical writers is the effect of the rotatory action of fluids, to

which he attended for forty years. All acknowledge the sagacity, perseverance, and honesty which are conspicuous in this prominent part of Biot's life, as in others.

As a critic and historian, Biot's field of labour was even wider than that of his life as an experimenter and observer. Had he published nothing whatever except his papers on Egyptian, Hindoo, and Chinese astronomy, he would have been known as an inquirer the amount of whose labours was fully equal to that of several whose reputation is entirely founded upon oriental astronomy. Had he produced nothing except the long series of articles on contemporary science and history of science which adorns the Journal des Savans, he would have been remarkable as the most continuous and varied scientific critic of his time. And in all these articles there is a close and discriminating production of the whole subject, relieved by legitimate satire, and by a tone of occasional pleasantry which is the true vehicle of certain parts of good criticism. Three volumes of 'Mélanges Scientifiques et Littéraires' were published in 1858; but it may be hoped that this will be superseded by a more complete reprint.

It is natural that a notice in these pages should make allusion to Biot's part in a controversy which, more than any other, concerns this Society: we mean the never-ending question of Newton and his opponents. From the time when the life of Newton appeared in the 'Biographie Universelle,' its author was what we may here call the chief of the opposite party. His views were strong, and ably supported; his mode of opposition was fair and downright. Biot was one of those disputants who cannot fail to forward sound conclusion, take which side they may.

As an elementary writer, this country is under especial obligations to Biot. In 1816, just after the termination of the long struggle which had isolated Great Britain from the continent, he produced those treatises on physics, full and abridged, which laid all the recent physical improvements before those who could not have sought them in scattered organs of announcement. Very many of those whose youth belongs to this period will remember Biot's 'Traité' and especially his 'Précis,' as the first sources of their acquaintance with modern experimental methods and results. The treatise on astronomy, not so much known in this country, filled up a void

which had been left open in the large mathematical work of Delambre.

It is not often that a death at the age of eighty-eight leaves a blank in the scientific world; but this must be said of Biot. To the end of his long life he was in perpetual activity. A volume on Indian and Chinese astronomy appeared in 1861, closing the list which began with "Elements of Arithmetic," prefixed to Clairaut's 'Algebra,' in 1797.

William Borrer, Esq., the eldest of the three sons of William Borrer, Esq., of Parkyns-manor, Hurstpierpoint, was born at Henfield in Sussex on the 13th of June 1781. He passed his long life in the country, discharging the duties incident to a landed proprietor and county magistrate, and earning the respect and attachment of his neighbourhood for his well-considered acts of local beneficence. Amidst his rural occupations Mr. Borrer found ample scope for the pursuit of botany, to which he was enthusiastically devoted, and earned for himself a considerable reputation among British botanists for his extensive and accurate knowledge of indigenous plants. To the great repertory of that species of knowledge, the 'English Botany,' and especially to the Supplement of that work, he contributed valuable materials; and, in association with his friend Mr. Dawson Turner, commenced a 'History of British Lichens,' which, however, was stopped in its progress by the death of the printer and other untoward circumstances. After lying dormant for a quarter of a century, the fragment of this work actually printed was brought out by Mr. Turner for private circulation, and mainly, as he expresses himself, that it might serve as a monument of Mr. Borrer's industry, ability, and profound knowledge of the family of plants to which it refers.

Mr. Borrer was elected into the Royal Society in 1835. He was also a Fellow of the Linnean Society and of the Wernerian Natural History Society of Edinburgh. He died on the 10th of January 1862.

The life of our late President, Sir Benjamin Collins Brodie, Baronet, Serjeant-Surgeon to the Queen, has not been ended long enough to allow even those who are best acquainted with it, fully or, perhaps, correctly to estimate its precise value.

There is no profession where a man may in his lifetime be so distinguished, and leave behind so slight record of his life, as the profession of Medicine or of Surgery. With the death of the man there perishes in such case a vast amount of personal skill and observation, which, being unwritten, and indeed not capable of being written, can be amassed again only by the combination of similar talent, opportunity, and industry in another individual. Nor is even this always possible. There are epochs in human knowledge as in human affairs; and a man may so turn to account the peculiar circumstances of his epoch as to attain not only just celebrity, but a certain masterly power which he could not have attained without such a combination of events.

Such considerations must be present to our minds if we would form a correct estimate of Sir Benjamin Brodie. He furnishes a rare instance of a man who, having in early life had no particular advantages on the one hand, nor any great drawbacks on the other, obtained the highest place in a learned profession, as well as the greatest honour which English Science can bestow on a scientific man—the Presidency of the Royal Society.

A brief record of his progress, considered not only as that of an adept in science and a master in the noblest of arts, but as a man, will be well worthy a place in the 'Proceedings' of the Royal Society—a Society which is not only directly concerned in advancing human knowledge for its own sake, but indirectly also in interesting the most complete minds in the advancement of that knowledge. It must set a special value on the example of one who proved to demonstration, by a long and admirable career, that devotion to purely scientific pursuits, and a deep interest in all that concerns scientific progress, may coexist with eminent professional skill, with a philanthropic spirit, and an enlarged religious mind.

Benjamin Collins Brodie was born in the year 1783, at Winterslow, in the county of Wilts. His father was Rector of the parish. He had three brothers and two sisters, being himself the fourth child. His father was a man of energy and ability, and brought both to bear on the education of his children, in whose well-being he took the deepest interest. He instructed them himself; by his own example he trained up his children in habits of industry; above all he taught them in many ways from their earliest years to think and

act for themselves. Sir Benjamin would often state the great advantage he had derived from being called upon at the age of sixteen to join in managing a volunteer corps at the period of the first anticipated French invasion in 1798. The difficulties of communication, and the whole condition of the country, made such a task more arduous, and therefore more instructive, than a young man would find it at this day.

But this occupation did not distract him from those studies which there has been a tendency of late years to decry. In after life he often looked back with satisfaction to the labour he had bestowed on committing to memory passages of the Greek and Latin authors, and of our own chief poets; he would tell how, in long professional journeys, before the days of railroads, he had been cheered by the recital of them; and he would point out how he believed the imaginative faculty, so essential to any great artist, be his art what it may, had been disciplined as well as fostered by early industry in ordinary classical studies.

Being naturally, or from a sense of duty, a studious boy, he, of his own accord, amassed, in leisure hours spent in his father's library, a great variety of knowledge; and he even then acquired a taste for those psychological speculations which shed a genial glow over his later days, when the labour of life was over and when his mind dwelt with serene delight on the contemplation of those higher qualities which are the peculiar property of man, and which are strengthened or impaired according to the use made of them by each possessor.

Thus prepared, in the autumn of 1801 our future President entered such a school of medicine as sixty years ago London afforded. He had no special predilection for either medicine or physical science. The arrangements of his family, rather than an active choice, led him to adopt his father's suggestions as to his future profession. He had already acquired a taste for work as such, he knew that he had to strive for his own maintenance, and forthwith betook himself with rational zeal to the selected study. Had his lot engaged him in the study of some other subject-matter than medicine, that other subject would assuredly have been equally mastered by the same steady grasp, and elucidated by the same clear mind.

It is not easy for one conversant only with the existing appliances for medical instruction to appreciate the circumstances of a lad who

was sent to walk the hospitals in 1801. At present the work of a student is regulated, or over-regulated—is divided into many subjects, so many lectures being assigned to each. There is the first year's work and the second year's work, under an arrangement so systematic that the young student, whatever be his capacities or his previous training, is allowed only to join the stream, and therein is hurried on too fast if slow of apprehension, or wearied by useless attendances if quick beyond the average. The freest scope was then given to the able and well-disposed—too free by far for the careless or dissolute. Brodie, nurtured hitherto only by his father's polished care, the companion of the Rector's walks, the popular subaltern of a volunteer corps in a county far from the metropolis, reaches London, and is thrown at once upon opportunities so ruinous to some, so good for him. He was not without good advice as to his future course. Dr. Denman, Dr. Baillie, and Sir Richard Croft, eminent and admirable men, were connected with his family by marriage. He was sent to Abernethy's lectures on anatomy. If in these lectures details were absent, the deficiency was compensated for tenfold by the genius and heart of the man. Abernethy gave to his pupils what the living teacher can best give, a living interest in their work. The Student was fascinated. He determined on following the profession recommended by the example of that popular Surgeon; for he found that he could not be a Physician without a university degree; and this had not been provided for him.

Brodie's character and training must have made parts of his early medical studies irksome, and some of his companions distasteful to him—though in this he only shared the lot of other right-minded youths who come from virtuous homes. Nor can it be doubted that the shock which he must have experienced sixty years ago at the low education and unformed habits of mind of some of his class companions, helped to implant in him that strong, it may be said parental concern in all that affects the best interests of medical students, which characterized him up to the last days of his long and active life. It is, however, true that, devoted, as his subsequent career proves, to his purely professional studies, he was not dependent on the medical school for his companions. His literary tastes did not desert him. He studied in this year the writings of various masters in mental philosophy—of Dugald Stewart, Berkeley,

Locke. He belonged to a literary debating Society, of which Lord Campbell, then young, was also a member; and as evidence of the subjects there occupying his mind, it may be stated that he read essays on the advantages to be derived from metaphysical inquiries, and on the supposed modern discoveries which are to be found forestalled in Pliny's 'Natural History.' This comprehensive view of the subjects, both proper and useful in the formation of a large professional mind, was never altered. In the most active period of his life he is known to have examined, with care and interest, the scientific papers in the early volumes of the 'Transactions' of this Society; and still later to have increased his acquaintance with the older medical and surgical writers.

In this year and the following he dissected at Wilson's School of Anatomy, worked at pharmacy in the open shop of an apothecary, and did not enter St. George's Hospital till 1803.

At the Hospital the youth immediately ripened into the man. Though he would even then look wistfully at literary pursuits, and kept up constant intercourse with literary men, he here first learned to apply the mental instrument, which hitherto he had only whetted, to the material on which for half a century it was henceforward to work. He first watched his teachers as they played before him the solemn and weighty game of Therapeutics, "life being the stakes;" and then, unceasingly in the wards, he studied by himself with avidity the accidents and injuries to the human frame which he had pledged himself henceforward to strive to alleviate or avert. He wrote full notes of what he observed. They who know his terse mode of expression, know how clearly he thought, how exactly, how simply he recorded what is essential, and how he discarded everything that is irrelevant.

Though so intent on clinical study, his well-poised mind did not relax its hold on scientific work. He seized now the opportunity of teaching anatomy, and continued for many years to employ his powers in this manner. He attended few lectures; there were, happily for him, few to attend. He read few professional books; there were not many worth study. He dissected, observed, recorded, taught. He worked at anatomy for and with Sir Everard Home, not only as it bore on surgical practice, but as a science, pursuing it (as Hunter had done) into higher physiological questions, and into the

comparison of organ and function throughout the animal series. Here he received much help from Mr. Clift, the faithful and excellent Conservator of Hunter's preparations.

It was about 1806 that his connexion with the Fellows of the Royal Society may be said to have begun. Through Home he made the acquaintance of Sir Joseph Banks, and (as the few who remember the liberality and kindness of that illustrious and useful man will well understand) met in his society the galaxy of scientific persons who early in this century flocked daily to his residence in Soho Square. In after days he used to refer to the advantage he had derived from his early acquaintance with Davy, Hatchett, Wollaston, Brown (the botanist), Dryander, Dr. Young, and others. He had been always shy, and was still nervous; modest, yet not without ambition: as he listened to the discourse of these men, and admired the consummate fitness of Sir Joseph Banks for his high station, he probably little thought that he would himself be called upon at a future day to occupy the same distinguished post.

By these several means he thus early gained a complete appreciation of what was needed to understand the nature of the diseases which oppress mankind, of the relative importance of clinical observation, pure science, and philosophical culture, in procuring alleviation of physical suffering. One can imagine the sadness, the almost bitterness of spirit which a man so disciplined in youth must have felt when, full of years and rich in vast experience, he felt himself called upon to leave as one of his last legacies to his countrymen his manly answer* to some of the ill-grounded fallacies which fashion supports under the guise of medicine improved and reformed.

In 1808, at the age of not quite twenty-five, he became Assistant Surgeon to St. George's Hospital; and he continued in the direct service of the hospital for full thirty years. The absence of one of his seniors at once threw the charge of many in-patients on his hands. On these he bestowed the most assiduous care, and lectured on the most important cases. He had declined on a former occasion to give lectures on surgery, because he could have only given second-hand, or book knowledge. But he now was able to draw his pictures from the life; he began not only to lecture in the hospital

* Frazer's Magazine, September 1861. Consult also Quarterly Review, December 1842.

clinically, but, with Mr. Wilson, in Windmill Street, to teach systematic surgery; and he continued to do so for nearly twenty years. His mode of instruction was then more peculiar than it would be now. His object was, as Abernethy's had been, not to gain applause, but to teach the pupils. He found this could not be done by reading out his lectures; and he soon accustomed himself, as all great teachers do, to pour forth his knowledge, previously arranged and digested it is true, but flowing at the moment spontaneously and unconstrained.

At this time (1810) his Anatomical Lectures became more frequent; and he was engaged in preparing his work for the day following till three or four in the morning. When not so engaged, he sought instruction and recreation with his literary and other friends; and, strange as it may seem to those who do not see the bearing of scientific study on practical work, he continued his researches on philosophical anatomy with Clift and Home. He also undertook some original physiological investigations, the results of which were communicated to the Royal Society, he having been elected a Fellow in 1810. The object of these communications deserves consideration. It has been remarked that it is an instinct of genius not only to suggest what work can, but what work cannot be attempted with advantage,—in other words, that intellectual efforts are not made by great minds except in cases when they will succeed. Brodie's physiological researches "On the Influence of the Brain on the Action of the Heart, and on the Generation of Animal Heat," were made in 1811, fifty years before his death. His mind was keenly alive, as we have seen, to the value of purely scientific research, without any regard to the immediate utility (so called) of the inquiry. On the other hand, the relief of suffering and the prevention of disease were the cynosure of his life. The four important essays which appear in our Transactions—viz., the one just alluded to, with its sequel (published in 1812), and two on the "Mode in which Death is produced by certain Poisons" (one printed in 1811 and the other in 1812)—all give a clue to the large views of the young surgeon. He goes straight to the cardinal points to be noted in the apparatus for the maintenance of life, and inquires into some of the crucial instances in which death is rapidly produced. The mutual relation between the nervous system, which is the organic differentia of

animals, and the circulatory apparatus which gives the basic conditions of nutrition in the higher animals, occupy his mind in the first two Essays. In the second he was aiming at the solution of one part of the question of questions for physiologists, how poisons operate on vital organs so as to produce death. These, his first papers, were composed with great clearness, and based on well-devised experiments. They do not exhaust subjects then only opening; for they have both been carried forward by various able inquirers in the half century that has now elapsed since he wrote them. But Brodie proved the correctness of the opinion advanced by Bichat and Cruickshank, that the cessation of the heart's action depends, not directly on abstraction of the influence of the brain, but on the cessation of respiration; and he showed that, to some extent (though to what extent and in what way is not even now certain), the maintenance of animal heat is under the influence of the nervous system. The most important of his experiments on poisons were those which he made with the Woorara. They showed that the poison is first absorbed through the blood-vessels, and so acts on the nervous system; that in consequence of the paralysis thence ensuing, the respiratory organs cease to act; and that the failure of respiration leads to cessation of the heart's action, and suppression of the circulation. Accordingly it was further shown that if respiration be artificially performed, the heart will continue to act, and the blood to circulate; and that if the process be carried on for a sufficient length of time, in some instances the brain will recover, and life will be maintained without perceptible impairment.

Brodie appears also to have been the first to show that the Antiar poison of Java operates by primarily arresting the action of the heart, and that it thus offers a remarkable contrast to the class of poisons to which the Woorara belongs.

The Copley Medal was awarded to Mr. Brodie for the former set of observations in the year 1811.

It is noteworthy that at this period he was an active member of two small societies, to one of which, the Animal Chemistry Club, Humphry Davy belonged, while of the other, the Society for the Promotion of Medical and Chirurgical Knowledge, John Hunter had been one of the original members. He began about this period to be a frequent visitor at Holland House; and he has often been heard to

speak of the benefit he had derived from the society of the great statesmen, literary men, artists, and other eminent persons whom he there met.

In the year 1813 he delivered the Croonian Lecture on the effect of the Nerves on the Heart and on the involuntary Muscles. With characteristic modesty and his usual thoroughness of purpose, he requested that, as the subject required further inquiry, it might not be printed. His mind was still bent on the significance of the higher vital phenomena in animals; for in the following year (1814) another paper was printed in our Transactions, "On the Influence of the Nerves of the Eighth Pair on the Secretions of the Stomach."

In 1816 he instituted experiments on animals to determine the effect of the bile on chylification. His conclusion, that it is essential to that process, has been since disputed; but it is still probable that the earlier investigator was as near to the truth as his critics. He afterwards lectured on Comparative Anatomy for four years at the College of Surgeons, from 1819 to 1823, bringing to bear the stores of knowledge he had previously acquired when working with Mr. Clift and Sir Everard Home.

With these last lectures his active physiological studies must be considered to have closed. But so trained was he in scientific pursuits, that his eager interest in anatomical and physiological questions, and in the philosophy of his art, never left him.

Henceforward not only his duties at St. George's Hospital, and to private patients, but also his inclination led him to devote his whole powers of investigation to those alterations in the living body which constitute disease. The unsatisfactory state of surgical knowledge, and therefore of practice, in regard to affections of the joints, attracted his attention, and he at once hit on a principle which was a guiding maxim to him in after life. Finding how little could be made out of a disease by dissection of the parts where organic alteration was far advanced, he used to examine the joints of those who had died of other diseases, in order to find the first traces of future injury. At the end of two years' assiduous inquiry, for which he had great opportunities, he felt at liberty to communicate to the Medico-Chirurgical Society some observations on the question*. To these he added in the following year †. These papers form the basis of

* Med. Chir. Trans. vol. iv. † Ibid. vol. v.

his invaluable work on Diseases of the Joints. His nice discrimination of the tissues affected, and of the exact value of pain in the joints as evidence of organic disease, has altogether altered the practice in such cases, and has tended greatly to reduce the number of amputations. What a reflection for a man at the close of life!

To pursue further the details of his active professional life belongs to the purely Medical biographer. Here it need only be said that he published, besides the work on Diseases of the Joints, three other surgical volumes, and contributed numerous papers to the 'Transactions of the Royal Medical and Chirurgical Society,' and that he did not cease to lecture on surgery at St. George's Hospital, with more or less frequency, till 1843, his instruction being sought with avidity, for it was simple and drawn from the life. None who heard him can forget the graphic, yet artless, manner in which, sitting at his ease, he used to describe minutely what he had himself seen and done under circumstances of difficulty, and what under like circumstances he would again do, or would avoid. His instructions were illustrated by valuable pathological dissections which during many years he had amassed, and which he gave during his lifetime to his Hospital.

The threefold character of scientific man, author, and surgeon, thus early formed, was maintained till his sight failed shortly before his death. A few words must be said on his qualities in each respect.

As a scientific man his several works were marked by distinctness of purpose, adaptation of means to end, and rigid determination to conclude no more than observation completely justified. His relations to other scientific men may be best understood by recalling the just, courteous, and candid manner in which he conducted the business of the various societies whenever he was called upon to preside, and the lucidity with which he kept the main points before a meeting. He always advocated and supported open discussion, and in this way did good service to the Royal Society.

As an author, he was not voluminous; nor did he speak much in public. He discarded all arts of style, aiming solely at precision and brevity: he wrote as he spoke, only when duty called, or when there was something which he believed he could write or say well. He was well versed in the literature of his profession and of those sciences which interested him; but he had not much love for books as instructors in his calling, because he knew that observation and

reflection were of more service than reading for the formation of the scientific mind, and original knowledge more valuable than that which is secondhand. He himself used books and so advised younger men to use them rather to gain the knowledge of what had been done, and as an aid towards actual observation and reflection, than thereby to educate themselves. His belief that observation, practice, and thought are the chiefest means for self-training in science partly accounts for the brevity of his published works, and greatly enhances their value.

As a surgeon, he was remarkable from early life for the scrupulous care which he bestowed on the investigation of the cases entrusted to him. This obtained for him in a few years rare quickness as well as precision in the formation of his opinion. When Sir Astley Cooper's practice declined, he was for many years extensively called upon to act as an operator. He excelled in that department of his art; for he had every requisite for success—knowledge, coolness, and the quick imagination which prepares for almost all possible emergencies that can occur, and suggests at once expedients when any come unforeseen. He did not, however, give the highest place to this part of his professional duties; for, in an occupation in which intellectual power and practical skill are combined, he valued those parts the most in which the most intellectual power is evoked. At the same time he was ready and ingenious in mechanical contrivances, and had the neatness and the method so requisite for a good surgeon. It was characteristic of his mind, that, among a few valuable lectures on some important subjects which he collected into a volume, he has given a place to one on Corns and Bunions—showing that in his judgment a small evil which can produce great annoyance requires as much consideration in its turn as more serious disorders. In truth, as the great aim of his life was to prevent or to cure disease, that which was curable, though trifling, would in one sense attract his notice more than that which was already irremediable. At the same time his difficulty in coming to the conclusion that there was nothing to be done, in even the gravest case, was a marked feature in his hopeful mind.

But the character of Brodie can be only properly considered as a whole. Neither as scientific man, nor as surgeon, nor as author was he so remarkable as he appears when viewed as he was—a complete man necessarily engaged in various callings. It was impossible to see him acting in any capacity without instinctively feeling that

there he would do his duty, and do it well. Nor could he be imagined in a false position. A gentleman, according to his own definition of that word, "he did to others that which he would desire to be done to him, respecting them as he respected himself." Simple in his manners, he gained confidence at once; accustomed to mix with the poorest in the hospital and with the noblest in their private abodes, he sympathized with the better qualities of each—valued all, and despised nothing but moral meanness. Though as a boy he was retiring and modest, he was happy in the company of older persons, and, as he grew older, loved in his turn to help the young. "I hear you are ill," he wrote once in the zenith of his life to a hospital student of whom he did not then know much; "no one will take better care of you than I; come to my country house till you are well;" and the student stayed there two months. He was thought by some reserved—he was modest; by others hasty—he valued time, and could not give to trifles that which belonged to real suffering; he was sometimes thought impatient, when his quick glance had already told him more than the patient could either describe or understand. Unconscious of self, of strong common sense, confident of his ground or not entering thereon, seeing in every direction, modest, just, sympathetic, he lived for one great end—the lessening of disease. For this object no labour was too great, no patience too long, no science too difficult. He felt indeed (to use his own words on the day of his election as President) "his happiness to be in a life of *Exertion*." As a professional man he valued science because it so often points the way to that which is practically useful to man; but as a scientific man his one object was the Truth, which he pursued for its own sake wholly irrespective of any other reward which might or might not follow on discovery. He had not the common faults of common men, for he had not their objects, nor their instinct for ease, nor their prejudices: though he became rich, he had not unduly sought riches; though he was greatly distinguished, he had not desired fame; he was beloved, not having courted popularity. What he was himself, that he allowed other men to be, till he found them otherwise. He saw weak points in his profession, but he saw them as the débris from the mountains of knowledge and of wisdom, of benevolence and of self-denial, of old traditional skill ever growing and always purifying,—those eternal structures on which are founded

true Surgery and Medicine. If ever he was bitter in society, it was when they were undervalued; if ever sarcastic, it was when the ignorant dared assume to judge them.

A light is thus thrown on his even career of uniform progress. Training his powers from youth upwards, by linguistic and literary studies, by scientific pursuits, by the diligent practice of his art, by mixing with men, he brought to bear on the multifarious questions which come before a great master of healing, a mind alike accustomed to acquire and to communicate, a temper made gentle by considerate kindness, a tact that became all but unerring from his perfect integrity. He saw that every material science conduces to the well-being of man; he would countenance all, and yet be distracted by none. He knew the value of worldly influence, of rank, of station when rightly used; he sought none, deferred excessively to none; but he respected all who, having them, used them wisely, and accepted what came to himself unasked, gave his own freely to all who needed, and sought help from no one but for public ends.

A few words only may be added on the inner life of his later days. Those who knew him only as a man of business would little suspect the playful humour which sparkled by his fireside—the fund of anecdote, the harmless wit—the simple pleasures of his country walk. Some, who knew these, might not have imagined another and deeper current which flowed unheard when neither the care of his patients nor his literary pursuits or memories engaged his mind. He who from his early professional life sat down every night, his work ended, his notes entered, his next day ordered, to ask what could have been better done today and what case otherwise managed, was not one to reach threescore years and ten without a keen onward gaze on the entire destiny of man. Yet he who realized in his profession the answer of Trophilus the Ephesian to the question, Who is a perfect physician?—"he who distinguishes between what can and what cannot be done"—such a man would not dogmatize on what cannot be known, nor would he, so humble, attempt to scan the Infinite. But his nature yearned for some better thing to come; and yearning, it became satisfied. He had for many years thought and conversed among his friends on facts he had noted in relation to our mental organization. In the year 1854, he published anonymously a volume on Psychological Inquiries. This was

followed by a second, with his name, in 1862. These volumes contain little that is actually new to professed psychologists; but they are the conclusions of one who had thought and worked—variously, consistently, practically. Living not in the closet, but hearing the opinions of every party and of every kind of men—liberal in all his views—without prejudice, and ever open to conviction, yet tinged with a general dislike to change as such,—he tells in these volumes what he had concluded concerning the mind of man—its laws, its discipline, its future state. They therefore who value such a character will prize these writings for qualities other than the novelties they may contain. It will be remembered that the scientific inquiries of his early life related to the influence of the nervous system on certain parts of the animal economy. To the ordinary physiologist this may be a purely material question; to him it was not so. In middle life he said to a friend, speaking of his lectures on the Comparative Anatomy of the Brain, "the complexity of the mechanism of the higher brains is enough to make one giddy to think of it." A fortnight before his death he talked to the same person of this mysterious link between our consciousness and our visible material organization, descanting with keen interest on the relations between mind and body, and the mutual reactions of one on the other. As he then lay on his sofa almost for the last time, in great pain, having scarce for many months seen the outer world which had been so much to him, and to which he had been so much, he spoke freely of our ignorance as to many things which it would be a joy to know—of the existence of evil—of the too little attention which philosophers had paid to the terrible nature of physical pain—of the future state. So gathering up the teachings of his useful life, and still, as ever, looking forward, he waited its close. Not many days after this he breathed his last, at Broome Park, on October 21, 1862, in possession of the full calm power of his disciplined mind to within a few hours of his death.

Such was our late President. They who knew and honoured him may excuse, while they accept, a delineation too feeble for so complete a man. In the quality of his mind he was not unlike the most eminent of his contemporaries, Arthur Duke of Wellington. Those who did not know him, and who do not appreciate the power requisite to make such a master in medicine as he was, may

be surprised at the comparison. Yet our great soldier might have accepted the illustration without dissatisfaction. Whatever art Brodie undertook, if he has been correctly drawn, he would have entirely mastered. The self-discipline of the strongest man can effect no more. The care with which the two men compassed every detail and surveyed every bearing of a large question, the quiet good sense, the steadiness of purpose, the readiness of wide professional knowledge in critical emergencies, were in each mind alike. The public and his profession esteemed Brodie as the first in his art. He advised three successive Sovereigns, and from one had the only other mark of esteem which a Sovereign can bestow—a Title. He was made a Corresponding Member of the French Institute in 1844, and received the Honorary Diploma in Civil Law from the University of Oxford in 1855. He was elected the President of the Royal Society in 1858, and became the first President of the Medical Council under the Act for regulating the Education and Registration of the Medical Profession; but he resigned both offices in two years, on account of the advancing failure of his eyes.

It remains to be recorded that in 1816 Mr. Brodie married the daughter of Serjeant Sellon, and survived her two years. By her he had issue two sons and a daughter. Of these, the eldest, the present possessor of the Baronetcy, like his father, received early in life one of the Medals of the Royal Society, and is now the distinguished Professor of Chemistry in the University of Oxford.

Francesco Carlini was born in Milan on the 7th of January 1783. His father, Carlo Giuseppe Carlini, a native of Cremona, was one of the librarians of the Brera, and was eminent as a Bibliographer. He died in 1789. His ancestors came, it is said, from Linz in Austria.

At an early age Francesco Carlini manifested a taste for astronomy, and was in the habit of making calculations for Oriani, Reggio, and Cesaris, the astronomers of the Brera. In 1799 he was admitted to the Observatory as a pupil, was appointed one of the commissicners of weights and weasures for the kingdom of Italy soon afterwards, and in 1804 was promoted to the rank of supernumerary astronomer. In 1803 he made observations of Pallas in opposition, and continued his observations of this and the other small planets up to 1816. Between 1814 and 1818 he constructed

tables of the equation of the centre, and the reduction to the ecliptic, of all the small planets then discovered. In 1804 he undertook the sole charge of calculating the Effemeridi Astronomiche, pronounced by Lindenau and Bohnenberger, in 1816, to be the best ephemeris in existence at that time. From 1802 to 1807 he took a share in the geodesic operations required for the construction of a map of Lombardy. These were carried on from 1788 to 1807 by the astronomers of the Brera, and afterwards by the French engineers.

A careful examination of the solar tables of Delambre, made in order to ascertain their fitness for use in calculating the Effemeridi, having revealed some serious errors, Carlini was induced to undertake a revision of them. Retaining the constants which Delambre had deduced from the observations of Bradley and Maskelyne, he recalculated the tables by a method of his own. In 1832 he published a new edition of the tables, based upon newer and more accurate elements. These tables were used till very recently for the calculation of the sun's place in the most celebrated Ephemerides.

Laplace, dissatisfied with the semiempirical basis on which the lunar theory rested, even after the publication of the 'Mécanique Céleste,' suggested to the Institute, as the subject of the prize for 1820, the formation of lunar tables, by theory alone, as exact as those which up to that time had been constructed by theory and observation combined. As far back as in 1813 Plana and Carlini had resolved to construct a complete theory of the moon, subjecting all the inequalities to the laws of geometry, and had made considerable progress in their task, when the announcement of the programme of the Institute induced them to compete for the prize by sending in the results they had already obtained. By the decision of a commission, consisting of Laplace, Burkhardt, and Poisson, the prize was divided between Plana and Carlini, and Damoiseau. The principle on which their joint memoir rested, and which rendered it superior to all researches of earlier and many of later date, was this: —never to take from observation any constants that were not indispensably necessary for the solution of the problem. They adhered most rigorously to this condition, without which the analytical solution cannot be perfect. In the development of the various expressions in series, they retained the literal notation, giving an algebraical, not a numerical solution. This memoir was not published

at the time: afterwards Plana resumed it alone, developed it more completely, and published it in 1832, under the title "Théorie du Mouvement de la Lune," in three volumes quarto, a work which forms an epoch in the history of astronomy. In 1820 they published a joint memoir in reply to some objections to their theory raised by Laplace—and another on the lunar equation, having for its argument twice the difference between the longitude of the node and that of the perigee. Carlini next undertook the construction of lunar tables. These have been used up to the present time in computing the places of the moon for the Effemeridi di Milano. He afterwards investigated various points of the lunar theory, especially the remarkable inequality of the moon's mean motion, indicated by a comparison of ancient and modern observations, and produced, as Hansen proved in 1847, by the disturbing action of Venus. Of his "Algoritmo del calcolo delle perturbazioni lunari," intended as the commencement of a complete lunar theory, only the first chapter appeared in the 5th volume of the memoirs of the Istituto Lombardo.

Carlini's principal contributions to practical astronomy are:—"Tables of astronomical refraction" (1807); "Computation of occultation of stars by the moon" (1808); "Tables for the reduction of circummeridian altitudes," (1809); "Tables for calculating the coefficient of the square of the time in the precession of stars" (1819). He invented a method of finding the time and latitude by means of a telescope provided with a level and a micrometer, and applied it in Spain, where he went by order of the Government to observe the eclipse of the sun on the 18th of July, 1860.

Between 1821 and 1827 Carlini assisted in the measurement of the Italian portion of an arc of longitude extending from Bordeaux to the Adriatic. He made the requisite astronomical observations on the summit of Mont Colombier; determined the length of the seconds' pendulum on Mont Cenis; observed from Parma the gunpowder signals fired on Monte Cimone, and from Milan those fired on the summit of Monte Baldo. He also determined anew, with the aid of better instruments and improved methods of calculation, the latitudes of Mondovi and Andrate, the extremities of the arc measured by Beccaria. He found the sum of the deviations of the plumb-line to be about 48″, and thus confirmed the accuracy of Beccaria's observations.

On the death of Angelo de Cesaris in 1832, Carlini was appointed Director of the Observatory of the Brera. He was a good analyst, understood five modern languages, was the author of various scientific biographies, and was one of the editors of the 'Biblioteca Italiana' from 1826 to 1840.

The list of his writings appended to the notice of his life read before the Istituto Lombardo contains one hundred and forty-four titles. Most of these, exclusive of separate works, are to be found in the appendices to the 'Effemeridi astronomiche di Milano' from 1805 to 1863, the 'Memorie dell' Istituto Lombardo-Veneto,' the Memorie, Atti, and Giornale dell' Istituto Lombardo, the 'Biblioteca Italiana,' the 'Memorie della Società Italiana,' the 'Monatliche Correspondenz' and 'Correspondance Astronomique' of von Zach, and the 'Zeitschrift für Astronomie' of von Lindenau and Bohnenberger.

Carlini presided for many years over the Istituto Lombardo-Veneto, afterwards the Istituto Lombardo, and was a member of the principal Italian Academies. He was foreign member of the Institute, of the Astronomical Society, and of the Academies of Vienna, Göttingen, and Berlin. The date of his election as foreign member of the Royal Society is 1832.

In 1832 Carlini married Gabriella Sabatelli, the daughter of an eminent artist. She survives him. He was amiable in private life, and of spotless character. He remained free from infirmity, either bodily or mental, up to the age of nearly eighty years, and was able to compute the elements of the second comet of 1862 only a month before his death, which occurred, after a short but painful illness, at Crodo in Ossola, on the 29th of August 1862. The foregoing particulars of his life and labours are extracted from a biographical notice read before the R. Istituto Lombardo, on the 8th December 1862, by his successor as Director of the Observatory of the Brera, G. V. Schiaparelli.

James Ormiston M'William, M.D., was a native of Dalkeith in Scotland, and was educated in Edinburgh, where he obtained his medical degree from the University, and the diploma of Licentiate from the Royal College of Surgeons. In 1829 he entered the Navy, and, after serving for a time on a foreign station, attained the rank

of Surgeon, and was appointed to H.M.S. 'Scout,' stationed on the west coast of Africa. On that unhealthy station, so trying to young naval surgeons, he devoted himself most earnestly to his duties, and obtained the distinction of the Gold Medal instituted by the late Sir Gilbert Blane for special merit in the medical service of the navy.

On returning to England, after the ship's term of service had expired, Dr. M'William took advantage of his temporary liberation from duty to improve himself in professional and scientific knowledge, and more especially to make himself well acquainted with the principles and practical methods of ventilation, heating, and other arrangements for the preservation of health. This species of knowledge, together with the information he had acquired on the coast of Africa, peculiarly qualified him to superintend the fitting up of the three ships which were destined to proceed on a voyage up the Niger. His sound judgment, intrepid character, and medical experience of a tropical African climate, had already led to his being appointed chief medical officer of the Expedition.

This Expedition was undertaken by benevolent individuals, supported by a considerable Government grant, to plant an English colony in Central Africa, and to promote agriculture and honest trade among the natives, in the hope thereby to reclaim them from trafficking in slaves. The chief incidents of the ill-fated voyage are well known. In the summer of 1841 the three ships proceeded to the African coast, under the command of Captain Trotter, and, ascending one of the mouths of the Niger, reached the confluence of that river with the Chadda, 270 miles from the sea; but in the mean time malignant fever had broken out in the ships, and in a short time had prostrated both officers and crews to such an extent that first one of the vessels, and soon afterwards another, had to be sent back freighted with the sick of the expedition. The remaining ship, the 'Albert,' of which Dr. M'William was surgeon, continued her voyage fifty or sixty miles higher up the river, in the hope of reaching some less unhealthy region; but the sickness did not abate, some of the ship's company had died, and many more, including the commander and most of the officers, were lying helpless under the malady; so that Captain Trotter, confirmed as he was in his judgment by Dr. M'William, determined to abandon the enterprise, and

the 'Albert' followed her consorts back to the sea. By this time only two officers, Dr. M'William and Dr. Stanger, and one of the European seamen were left able for service. In this plight the return voyage was commenced, and, the one remaining sailor having also fallen ill, the navigation of the ship was left to the two medical officers, Stanger acting as engineer and M'William as steersman; and by their good management and gallant perseverance she reached the sea in safety.

After his return home, in 1842, Dr. M'William published the 'Medical History of the Niger Expedition,' which was well received; and some time afterwards he was sent on a special mission to the Cape de Verde Islands, to inquire into the origin of the yellow fever which had broken out at Boa Vista soon after the arrival of the 'Eclair.' After a most laborious investigation, Dr. M'William gave in to the authorities an able Report, in which he clearly established that the disease was communicable by infection, and had been imported by the 'Eclair.' This report was printed by order of Parliament.

In rather tardy recognition of his public services, Dr. M'William was in 1846 appointed Medical Officer to the Custom House, and in 1858 was made a Companion of the Civil Order of the Bath. He was elected a Fellow of the Royal Society in 1848, and a Fellow of the Royal College of Physicians in 1861. He was a most active member and promoter of the Epidemiological Society, of which he was Secretary, and to which he contributed many valuable papers. As a naval medical officer he was highly esteemed by his brethren, not only for his professional merit, but for his unceasing efforts to secure for them, and particularly for the junior members of the service, a rank and position more in accordance with the social standing of the medical profession in civil life.

Dr. M'William's active and useful career was terminated on the 4th of May 1862, in consequence of an injury of the brain, caused by a fall down a steep stair a few days before.

Rear-Admiral Sir James Clark Ross was born in 1800, and died in April 1862. The incidents in the life of this great navigator and excellent man would doubtless furnish abundant matter for an interesting narrative; but here we must be contented with little more

than a bare indication of dates. In 1812 James Ross entered as a midshipman on board the 'Briseis,' commanded by his uncle, Captain (afterwards Sir John) Ross. In 1818 he accompanied his uncle in his first polar voyage, and between 1819 and 1827 he returned four times to the same seas under Parry. Again he accompanied his uncle in his Arctic voyages performed between 1829 and 1833. He conducted the scientific observations in these last-named expeditions; and it was while thus employed that he determined the situation of the North Magnetic Pole, in latitude 70° 5′ 17″ and west longitude 96° 45′ 48″. In 1834 he was raised to the rank of Captain, and in 1835 was sent out to Baffin's Bay to succour ice-bound whalers.

Captain James Ross's skill and experience as a magnetic observer led to his being employed, between 1836 and 1838, in a series of determinations of the magnetic declination and dip, and the intensity of the magnetic force over Great Britain and Ireland.

In 1839 he set out on his memorable Antarctic Expedition; and after making three voyages within the Antarctic Circle, reaching 78° 10′ of south latitude, greatly widening the known geography of the south polar regions, and gathering a rich harvest of observations in magnetism and other branches of terrestrial physics, he returned home after an absence of four years. In 1848 he sailed on the last of his many arduous voyages. It is well known that the direct purpose of this voyage—the discovery and relief of Franklin and his fellow sufferers—was unhappily not accomplished, but its Commander did not fail to render it profitable to science.

Sir James Ross received his knighthood in 1844. King Louis Philippe nominated him to the Legion of Honour. He was elected a Fellow of the Linnean Society in 1824, and of the Royal Society in 1848. He was also a Corresponding Member of the French Academy of Sciences, and belonged to various other foreign societies of note.

To this brief notice of the leading events of Sir James Ross's professional life, it will be incumbent on us to add a larger comment on his labours and achievements as a man of science; but as these have not yet been fully made known in their proper place, it is deemed advisable that such reference to them as it will be requisite to make should be postponed for the present. The great work

which especially deserves to have its merits prominently set forth is his magnetic survey of the Antarctic regions. This is justly held to be the greatest work of the kind ever performed, and it was undertaken at the special desire of the Royal Society and the British Association. An account of the magnetic observations made by Sir James Ross in the first and second of the three Antarctic voyages was published in the 'Philosophical Transactions' by his friend General Sabine; and it is hoped that the labours of the third voyage and the coordination of the three years' work will be laid before the Society in the next session. The completeness with which the great and hazardous enterprise was carried out renders a full exposition of what was accomplished all the more requisite for a just appreciation of the merits of its gallant and skilful conductor; and the propriety of waiting until this has been given will, it is conceived, be generally recognized.

Edward Stanley, Esq., Surgeon Extraordinary to the Queen, and Surgeon to St. Bartholomew's Hospital, died suddenly, in one of the wards of that Institution, on the 24th of May 1862, at the age of seventy.

Mr. Stanley, after receiving his general education at Merchant Taylors' School, was, in 1808, apprenticed to Mr. Ramsden, at that time one of the Surgeons of St. Bartholomew's Hospital, on whose death he was transferred to Mr. Abernethy. To this great master the pupil soon recommended himself by his earnestness in study, and especially by his devotion to anatomy and pathology; and through their joint labours was created the Hospital Museum of Morbid Anatomy, to which Mr. Stanley especially contributed many preparations illustrative of diseases of the bones and joints. This important collection they afterwards liberally presented, as a gift, to the Hospital. Mr. Stanley's acquirements in anatomy, and his well-directed zeal in its pursuit, led to his appointment as Demonstrator, and, after holding that office for some years, he joined Mr. Abernethy in 1826 in lecturing on anatomy and physiology, and continued to discharge this duty after the death of his eminent colleague, until 1843. In the mean time he had been, in 1816, elected Assistant Surgeon, and twenty-two years later, Surgeon to the Hospital; and he continued in that office until 1861, when,

although in good health and full possession of his faculties, he considerately retired to make way for his juniors, and accepted the title of Honorary Consulting Surgeon offered him as an acknowlegment of his past services and a mark of respect for his honourable motives in resigning his duties. In 1858 he was appointed Surgeon Extraordinary to the Queen.

Mr. Stanley entered the Royal College of Surgeons in 1816; he became an active and industrious member of the Council and Court of Examiners, served the office of Hunterian Lecturer, and was twice elected President. He was of old standing among the Fellows of the Royal Medical and Chirurgical Society, and successively filled the offices of Secretary, Treasurer, and President. His election into the Royal Society was in 1840.

Mr. Stanley's principal work is his 'Treatise on Diseases of the Bones,' which appeared in 1849. While Demonstrator of Anatomy he published a 'Manual of Practical Anatomy,' which in its time was extensively used in the Anatomical Schools of this country; and in 1829 an 'Account of the Mode of performing the Lateral Operation of Lithotomy.' He also contributed twelve papers to the 'Medico-Chirurgical Transactions.'

Mr. James Walker was born in 1781, at Falkirk, where he passed his childhood and received the rudiments of his education. He was afterwards sent to Glasgow, where he studied at the University and obtained distinction in Natural Philosophy and Mathematics. From that University he, in later years, received the honorary degree of Doctor of Laws.

In the year 1800 he came to London, and commenced the profession of Civil Engineer, under his uncle, Mr. Ralph Walker, at that time engaged in the construction of the West India Docks. He continued assistant to his uncle during the progress of that undertaking and during the completion of the East India Docks. In 1803 he was appointed Engineer of the Commercial and East India Roads, undertaken in order to open a more direct communication with the Docks and the eastern parts of London. These works were successfully carried out under his direction, and now form the great route to the extensive suburbs which have risen between London and Blackwall.

The successful completion of the East and West India Docks, and of the communications leading to these important dépôts, obtained for Mr. Walker the appointment of Engineer for the Commercial Docks, which, with all improvements and enlargements up to the present time, have been executed from the designs and under the direction of himself and his partner, Mr. Burges.

The satisfactory manner in which these works were accomplished led to other appointments under the public Boards, who, from that time to the day of his death, consulted Mr. Walker on every engineering work of importance. His numerous and important labours as a Civil Engineer, for the last forty years, are well known. Among the great works with which he was connected, we may indicate, as especially worthy of note, Vauxhall Bridge, the Victoria Bridge over the Clyde at Glasgow, the improvements of the river Clyde (which from the death of Mr. Telford up to a recent date were under Mr. Walker's direction), the great repairs of the Caledonian and Crinan Canals, the drainage of the Middle Level by a cut of thirty miles in length, the coffer-dams and river-wall of the new Houses of Parliament, the Netherton Tunnel and other important works carried out by the Birmingham Canal Navigation Company, the Pier and Harbour of Granton, the improvements of the Harbour at Belfast, and the Harbour works at Dover. All these and many others, such as the designs and execution of the Harbours of Refuge at Alderney, Dover, and Harwich, the Tyne Piers, and the completion of the Plymouth Breakwater, were under the direction and superintendence of Mr. Walker. To them may be added, what is perhaps the most lasting monument of his skill, the various Lighthouses of the Corporation of the Trinity House. The Bishop Rock Lighthouse, on the Scilly Islands, the erection of which was attended with peculiar difficulties successfully overcome by Mr. Walker, will rank with the foremost of the great structures of that class reared by Smeaton and by Stevenson.

Mr. Walker's sound judgment and high character as an Engineer caused him to be frequently consulted by the Corporation of the City of London on the various engineering works under their jurisdiction; among which may be named the City Sewers, the Navigation of the Thames, and the Thames Embankment. The

last-mentioned work was laid out by Mr. Walker, and his design has now been adopted under the designation of "Walker's line."

Thus, for more than half a century, did Mr. Walker indefatigably exercise his professional skill in works of public utility in all parts of the kingdom—works which have had no mean influence on the social and commercial progress of the country.

Mr. Walker was elected into the Royal Society in 1828; he cooperated with Mr. Telford in establishing the Institution of Civil Engineers, and succeeded him as President; and on the institution of the University of London, he was nominated in the Charter among the original members of the Senate.

In private life he was much respected; he died on the 8th of October 1862.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

March 6, 1862.

Major-General SABINE, President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Alexander Armstrong, M.D.
Henry Foster Baxter, Esq.
George Bentham, Esq.
Sir Charles Tilston Bright.
William Brinton, M.D.
Henry William Bristow, Esq.
Samuel Brown, Esq.
Alexander Ross Clarke, Capt. R.E.
Edward William Cooke, Esq.
William White Cooper, Esq.
Joseph Cubitt, Esq.
Henry Duncan Preston Cunningham, Esq., R.N.
John W. Dawson, Esq.
Alexander John Ellis, Esq.
Frederick John Owen Evans, Esq., R.N.
John Evans, Esq.
Henry Fawcett, Esq.
Frederick Field, Esq.
George Gore, Esq.
Robert Philips Greg, Esq.
Robert Harley, Esq.
John Braxton Hicks, M.D.
William Charles Hood, M.D.
The Very Rev. Walter Farquhar Hook, D.D., Dean of Chichester.
Edmund C. Johnson, Esq.
Waller Augustus Lewis, M.B.
Edward Joseph Lowe, Esq.
Charles Watkins Merrifield, Esq.
Gavin Milroy, M.D.
Sir Joseph F. Olliffe, M.D.
George Wareing Ormerod, Esq.
Frederick William Pavy, M.D.
William Pengelly, Esq.

Charles Bland Radcliffe, M.D.
George Rolleston, M.D.
Edward Romilly, Esq.
Henry E. Roscoe, Esq.
William Henry Leighton Russell, Esq.
Samuel James Augustus Salter, Esq.
Charles William Siemens, Esq.
Maxwell Simpson, M.B.
Balfour Stewart, Esq.
Thomas Tate, Esq.
Thomas Pridgin Teale, Esq.
Sir James Emerson Tennent.
Isaac Todhunter, Esq.
Charles Tomlinson, Esq.
C. Greville Williams, Esq.
Charles Wye Williams, Esq.
Frederick Marow Eardley Wilmot, Lieut.-Col. R.A.

The following communications were read:—

I. "Notes of Researches on the Poly-Ammonias."—No. XX. On the Colouring Matters produced from Aniline. By A. W. Hofmann, Ph.D., LL.D., F.R.S. &c. Received February 20, 1862.

In a note on the Action of Tetrachloride of Carbon on Aniline submitted to the Royal Society on the 17th of June, 1858, I have described a crystalline basic derivative of aniline formed by the coalescence of three molecules of ammonia, viz. carbotriphenyltriamine,

$$C_{19}H_{17}N_3=\left.\begin{array}{c}C^{iv}\\(C_6H_5)_3\\H_2\end{array}\right\}N_3,$$

the formation of which is accompanied by that of a colouring matter of a magnificent crimson colour.

It may be useful to quote here the passage* of the paper referred to, in which the formation of the colouring matter is mentioned. "On submitting a mixture of $3\frac{1}{2}$ parts by weight of aniline and 1 part of bichloride of carbon, both in the anhydrous state, for about thirty hours to a temperature of 170° C., the liquid will be found to be converted into a black mass, either soft and viscid, or hard and brittle, according to time and temperature.

"This black mass, which adheres firmly to the tubes in which the reaction has been accomplished, is a mixture of several bodies. On

* Proceedings of the Royal Society, vol. ix. p. 284.

exhausting with water, a portion dissolves, while a more or less solid resin remains behind.

"The aqueous solution yields, on addition of potassa, an oily precipitate containing a considerable portion of unchanged aniline; on boiling this precipitate with dilute potassa in a retort, the aniline distils over, whilst a viscid oil remains behind, which gradually solidifies with a crystalline structure. Washing with cold alcohol and two or three crystallizations from boiling alcohol render this body perfectly white and pure, a very soluble substance of a magnificent crimson colour remaining in solution.

"The portion of the black mass which is insoluble in water dissolves almost entirely in dilute hydrochloric acid, from which it is reprecipitated by the alkalies in the form of an amorphous pink or dingy precipitate soluble in alcohol with a rich crimson colour. The greater portion of this body consists of the same colouring principle which accompanies the white crystalline substance."

The action of tetrachloride of carbon on aniline yields only a comparatively small quantity of the crimson pigment; the temperature of the exposure, and the relative proportions in which the two substances act upon one another, have the greatest influence upon the results of the reaction. The white crystalline base, and the base dissolving with a crimson colour, are by no means the only products; other bases, most of them amorphous and accessible only in the form of platinum-salts, are produced, and complicate, owing to the similarity of their chemical characters, the purification of the new compound. Notwithstanding many efforts, I failed in obtaining the new colouring matter in a state fit for analysis, and for the time abandoned the inquiry.

Industry, however, was not long in discovering new and much more appropriate methods for the production of the crimson aniline dye. Certain metallic chlorides (tetrachloride of tin) and nitrates (mercurous nitrate), and numerous oxidizing agents are capable of converting aniline into the crimson colouring matter. It was M. Verguin who first prepared this colour upon a large scale by the action of tetrachloride of tin on aniline. Since that time the production of the aniline-crimson has become an important industry, which, in the hands of Messrs. Simpson, Maule, and Nicholson in this country, of Messrs. Renard frères in France, has rapidly attained

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to colossal proportions. The interest attached to the subject is sufficiently evident by a glance at the periodical literature of the day. The journals of applied chemistry teem with the descriptions of processes for the production of the aniline-crimson, for which the names fuchsine, magenta, and others more fanciful have been proposed. Even the action of tetrachloride of carbon on aniline, little promising as it appeared at first, has been used upon the large scale; and interesting papers upon the industrial production of the colour by this process have been published by M. Charles Dolfus Galline*, by Messrs. Monnet et Dury†, and lastly by M. Lauth‡, who have proved that aniline-crimson, prepared upon the large scale by means of tetrachloride of carbon, may be applied in dyeing with exactly the same result as the colouring matter produced by other processes. It is not the object of this Note to enter into a detailed account of the development of this new industry, which has been admirably traced by M. E. Kopp in a series of interesting articles published in the 'Répertoire de Chimie Appliquée;' but I thought it right to quote the above authorities in order to show that the basic colouring matter obtained by me in 1858, while studying the action of tetrachloride of carbon upon aniline, is identical with the aniline-crimson which is now by various processes manufactured upon an enormous scale.

A substance possessing such remarkable properties as aniline-crimson, and accessible, moreover, as a commercial product, could not fail to attract the attention of scientific inquirers. The subject has been examined in succession by M. Guignet §, M. Béchamp ||, M. Wilm ¶, Messrs. Persoz, De Luynes et Salvetat**, M. Schneider††, and more recently by M. Emile Kopp ‡‡ and M. Bolley §§. The conclusions, however, at which these experimentalists have arrived are far from concordant. I attribute this discrepancy in the results obtained by such able observers to the extreme difficulty of procuring the colouring matter in a state of purity, and to the circumstance

* Répertoire de Chimie Appliquée, 1861, p. 11. † Ibid. p. 12.
‡ Ibid. p. 416. § Bull. Soc. Chim. Séance du 23 Déc. 1859.
|| Annales de Chim. et de Phys. [3] tome lix. p. 396.
¶ Bull. Soc. Chim. Séance du 27 Juillet 1861.
** Comptes Rendus, li. 538. †† Ibid. li. 1087.
‡‡ Annales de Chim. et de Phys. tome xii. p. 222.
§§ Dingler's Journal, clx. 57.

that the slightest contamination with other bodies is capable of altogether masking the properties of this remarkable compound.

The red colouring matter of aniline and its saline compounds have been obtained for the first time in the state of purity by my friend and former pupil Mr. Edward Chambers Nicholson, a chemical manufacturer equally distinguished for his scientific attainments as for the skill and indomitable energy with which in many instances he has succeeded in rendering the results of purely scientific inquiries available for the purposes of life.

Mr. Nicholson has, with the utmost liberality, placed at my disposal not only a very considerable supply of the beautiful compounds which he produces, but also the vast and precise information which in his protracted experiments upon this subject he has accumulated. It is entirely through the kindness of Mr. Nicholson that I was enabled to resume the study of these remarkable bodies, a short account of the composition and of the chemical nature of which I beg leave to submit to the Royal Society.

Mr. Nicholson designates the pure base of the red colouring matter by the name of Roseine, which appears very appropriate, since this substance, which furnishes such splendidly coloured solutions, is absolutely white in the solid condition. Nevertheless, since the compound in question appears to be the prototype of a number of similar substances obtainable by similar processes from the homologues, and probably also from the analogues, of aniline, it may be useful to commemorate the origin of the compound in its name, and I accordingly propose the term *Rosaniline* for the designation of the new substance.

Rosaniline.—The material from which the base may be most conveniently obtained is the acetate which in practice is generally used for dyeing. This acetate Mr. Nicholson produces on the large scale in a state of perfect purity. The boiling solution of this salt, when supersaturated with a large excess of ammonia, furnishes a rose-red somewhat crystalline precipitate, which constitutes the base in a tolerably pure state. The colourless liquid filtered off while boiling from this precipitate deposits, on cooling, perfectly white needles and plates, which are the rosaniline in a state of absolute purity. Unfortunately the solubility of rosaniline in boiling ammonia, and even in boiling water, is extremely limited, so that only a

very small proportion of the compound is obtained in the perfectly white condition. The base is somewhat more soluble in alcohol, the solution having a deep-red colour; it is insoluble in ether. Exposed to the action of the atmosphere, rosaniline turns rapidly pink, and ultimately dark red. No perceptible alteration of weight is observed during this change. At the temperature of 100° the base rapidly loses a minute quantity of water of interposition; it may then be heated to 130° without further losing weight. At a higher temperature rosaniline is decomposed with evolution of an oily liquid containing much aniline, a quantity of charcoal remaining behind.

The combustion of rosaniline has led to the formula

$$C_{20}H_{21}N_3O = C_{20}H_{19}N_3, H_2O,$$

which has been corroborated by the examination of numerous well-characterized salts and derivatives.

Rosaniline is a well-defined powerful base, which forms several series of salts, nearly all remarkable for the facility and beauty with which they crystallize. The proportions in which this substance unites with acids characterize it as a triacid triamine. Like several other triamines which I have examined, it will probably be found to produce three classes of salts, viz.,

$$C_{20}H_{19}N_3, \; HCl,$$
$$C_{20}H_{19}N_3, 2HCl, \text{ and}$$
$$C_{20}H_{19}N_3, 3HCl.$$

Up to the present moment, however, I have been able to obtain only representatives of the first and the third of these classes. The predilections of rosaniline are essentially monacid. The salts with one equivalent of acid are wonderfully stable compounds. I have recrystallized them four and five times without producing the slightest alteration in their composition. The salts with three equivalents of acid present comparatively little stability, being, in fact, decomposed by the action of water or by exposure to 100°.

By a glance at the formula given, it is obvious that the white crystals of the base itself, which were submitted to examination, are a hydrate,—the saline compounds of rosaniline, as might have been expected from many of the processes of their formation, containing no oxygen. The salts of rosaniline may be obtained by two different processes: either by the direct action of the respective acids, or

by submitting the ammonium-compounds of the several acids to ebullition with an excess of the free base. Both processes yield the salts equally pure and of exactly the same composition. The salts with one equivalent of acid exhibit for the most part, in reflected light, the splendid metal-lustrous green of the wings of the rose-beetle; in transmitted light the crystals are red, becoming opaque when they acquire certain dimensions. The solutions of these salts in water or alcohol possess the magnificent crimson colour for which rosaniline has become so justly celebrated. The salts with three equivalents of acid, on the other hand, are yellowish brown, both in the solid state and in solution. They are much more soluble in water and alcohol than the monacid salts, which for the most part are comparatively sparingly soluble. Both classes of rosaniline-salts crystallize readily, more especially the monacid compounds; some of them Mr. Nicholson has obtained in perfectly well-formed crystals, which are at present in the hands of Quintino Sella for crystallometrical examination.

Chlorides.—These substances, and more especially the monacid salt, were of particular use in fixing the formula of rosaniline. Prepared either by the action of hydrochloric acid, or by means of chloride of ammonium, the salt is deposited from the boiling solution in well-defined rhombic plates, frequently united in stellar forms. The chloride is difficultly soluble in water, more soluble in alcohol, insoluble in ether.

The salt retains a minute quantity of water at 100°, but becomes anhydrous at 130°. At this temperature it contains

$$C_{20} H_{19} N_3, H Cl.$$

The salt, like most of the rosaniline-salts, is very hygroscopic, a character which must not be lost sight of in the analysis of these compounds.

The monacid chloride dissolves more readily in moderately strong hydrochloric acid than in water. If this solution, gently warmed, be mixed with very concentrated hydrochloric acid, it solidifies, on cooling, into a network of beautiful brown-red needles, which have to be washed with concentrated hydrochloric acid and dried *in vacuo* over sulphuric acid and lime, since water decomposes them with reproduction of the monacid compound. The salt obtained by the

action of concentrated hydrochloric acid is the compound with three equivalents of acid,

$$C_{20}\,H_{19}\,N_3,\ 3H\,Cl.$$

Exposed to 100° this salt gradually loses acid, the brown crystals becoming indigo-blue; and if the exposure be continued until the weight becomes constant, the original green salt with one equivalent of acid is reproduced, which was identified by analysis.

The two chlorides combine with *dichloride of platinum*. The compounds thus produced, being uncrystallizable, are not easily obtained in a state of purity. From platinum-determinations, which have given only approximative results, I infer that they contain respectively,

$$C_{20}\,H_{19}\,N_3,\ \ H\,Cl,\ \ Pt\,Cl_2 \text{ and}$$
$$C_{20}\,H_{19}\,N_3,\ 3\,H\,Cl,\ 3\,Pt\,Cl_2.$$

The *Hydrobromate of Rosaniline* resembles in every respect the hydrochlorate; it is even more difficultly soluble than the latter; it contains

$$C_{20}\,H_{19}\,N_3,\ HBr.$$

Hydriodate of Rosaniline.—Green, very difficultly soluble needles of the composition

$$C_{20}\,H_{19}\,N_3,\ HI.$$

Sulphate of Rosaniline is readily obtained by dissolving the free base in boiling dilute sulphuric acid. On cooling, the salt is deposited in green metal-lustrous crystals, which by one recrystallization become perfectly pure. At 130°, at which temperature it loses a small quantity of water, the formula of the salt is

$$\left.\begin{matrix} C_{20}\,H_{19}\,N_3,\ H \\ C_{20}\,H_{19}\,N_3,\ H \end{matrix}\right\}\,S\,O_4.$$

The acid sulphate crystallizes with difficulty. I have not analysed it.

Oxalate of Rosaniline.—The preparation and properties of this salt are similar to those of the sulphate. The salt retains at 100° one equivalent of water, and is at this temperature represented by the formula

$$\left.\begin{matrix} C_{20}\,H_{19}\,N_3,\ H \\ C_{20}\,H_{19}\,N_3,\ H \end{matrix}\right\}\,C_2\,O_4 + H_2\,O.$$

The water may be expelled at a higher heat; but the temperatures at which the water is lost and the salt commences to be decomposed are so close to each other, that it is not quite easy to obtain the salt

in the anhydrous state. I have not been able to procure an oxalate with a larger amount of acid.

Acetate of Rosaniline.—This is probably the finest salt of the series. Mr. Nicholson has obtained it in crystals an inch in diameter, which, on analysis, were found to be the pure monacid acetate, viz.,

$$C_{20} H_{19} N_3, H C_2 H_3 O_2.$$

The acetate is one of the more soluble salts, both in water and in alcohol; it cannot be conveniently recrystallized.

The *Formiate of Rosaniline* is similar to the acetate.

Of the remainder of the salts of this base I may mention the *Chromate*, which is obtained by adding chromate of potassium to the solution of the acetate in the form of a brick-red precipitate, becoming a green, crystalline, almost insoluble powder on ebullition with water.

The *Trinitrophenate* also deserves to be noticed; it crystallizes in beautiful reddish needles, likewise very difficultly soluble in water, which contain

$$C_{26} H_{22} N_6 O_7 = C_{20} H_{19} N_3, H C_6 H_2 (NO_2)_3 O.$$

Multiplied and varied though the analytical results may be which support the formulæ of rosaniline and its compounds, it appeared desirable to seek additional experimental evidence for the expressions derived from simple analysis. With this view I have studied the products of decomposition of rosaniline. They are both numerous and interesting. I must limit myself to-day to quote one or two compounds which claim some attention, not only because they unmistakeably confirm the formula which I have proposed, but also on account of the light which they throw upon the nature of the class of substances to which rosaniline belongs.

Action of reducing agents upon Rosaniline.—This action appeared to promise the simplest mode of controlling the formula of the new base. My anticipations have not been disappointed. Rosaniline is readily attacked by nascent hydrogen or sulphuretted hydrogen. A solution of the base in hydrochloric acid, when left in contact with metallic zinc, is rapidly decolorized. The liquid thus produced contains, together with chloride of zinc, the hydrochlorate of a new triamine, which, perfectly colourless as it is in the free state and in its saline compounds, I propose to designate by the term *Leucaniline*. The separation of the new compound from the zinc being tedious and troublesome, I prefer to prepare it by the action of sulphide of

ammonium. A salt of rosaniline, when digested for some time with sulphide of ammonium, furnishes a yellow, half-fused, scarcely crystalline, brittle compound, which constitutes the leucaniline in a state approaching purity. It is, however, by no means necessary to employ for the preparation of this compound a rosaniline-salt in the pure state. In most cases I have produced the leucaniline from the commercial article sold under the name of fuchsine or magenta.

To purify the product thus obtained, the yellow resinous mass is powdered, washed with water to remove the sulphide of ammonium, and dissolved in dilute hydrochloric acid, when sulphur, together with impurities, are left behind. The dark-brown solution thus obtained yields, with concentrated hydrochloric acid, a copious crystalline precipitate, which, according to the degree of purity of the commercial colouring matter, is either brown or yellow. Washing with concentrated hydrochloric acid, in which the precipitate is insoluble, effects a considerable purification; but in most cases it is necessary to repeat the process of dissolving in dilute and reprecipitating by concentrated hydrochloric acid once or twice. If the solution, before the last addition of concentrated acid, be heated to ebullition, the liquid remains clear, and the new chloride crystallizes out only on cooling. The crystals are beautiful, white, rectangular plates, which are, however, always very small. Recrystallization from water, in which they are extremely soluble, renders them perfectly pure. Or they may be dissolved in alcohol and precipitated by ether, in which they are quite insoluble.

The chloride thus purified yields, on addition of ammonia, the leucaniline as a dazzling white powder, which assumes the faintest rosy tint when left for some time in contact with the atmosphere of the laboratory. It is scarcely soluble in cold, very little soluble in boiling water, from which it is deposited, on cooling, in the form of small crystals. It is very soluble in alcohol, and, although less so, in ether. I have not been able to get it in good crystals from these solvents. The best solvent appears to be a solution of the chloride above described, in which leucaniline is freely soluble, and from which, on cooling, it is deposited in the form of interlaced needles, which are frequently united to spherical aggregations. Leucaniline may be dried *in vacuo* over sulphuric acid without changing its colour. When slightly heated it becomes red, and at 100° it fuses

to a deep-red liquid which, on cooling, solidifies to an indistinctly crystalline mass of lighter colour. Leucaniline is anhydrous. The analysis of the substance, dried *in vacuo*, and at 100°, has given results which correspond to the formula

$$C_{20}\,H_{21}\,N_3.$$

This formula has been verified by the examination of the chloride already mentioned, of a splendidly crystallized platinum-salt, and, lastly, of the nitrate, which may be likewise obtained in good crystals.

Hydrochlorate of Leucaniline.—The preparation of this compound has been mentioned. It is triacid, and retains, when dried *in vacuo*, one equivalent of water, its formula being

$$C_{20}\,H_{21}\,N_3,\,3\,HCl,\,H_2\,O.$$

The salt cannot be dried at 100°, at all events in air; but the water may be expelled, although with great difficulty, by exposing the salt for a considerable length of time to 100° in a current of hydrogen. I have endeavoured to convert this compound into a salt with less acid by boiling the solution with an excess of leucaniline, but without result. The boiling solution deposits, on cooling, beautiful crystals of the base, the triacid salt remaining in solution.

Platinum-salt of Leucaniline.—On adding dichloride of platinum to a moderately concentrated, gently warmed solution of the chloride, a splendid bright orange-yellow platinum-salt separates, on cooling, in well-formed prisms, generally aggregated to triangular stars. The salt is difficultly soluble in cold water; boiling water decomposes it. At 100° this salt retains one equivalent of water, which can be expelled, although with difficulty, at higher temperatures. Numerous analyses of this beautiful compound have established the formula

$$C_{20}\,H_{21}\,N_3,\,3\,HCl,\,3\,PtCl_2+H_2\,O.$$

Nitrate of Leucaniline.—Well-formed white needles, soluble in water and alcohol, insoluble in ether. The salt is rather difficultly soluble in nitric acid. When dried *in vacuo* this salt contains

$$C_{20}\,H_{21}\,N_3,\,3\,H\,N\,O_3+H_2\,O.$$

I have not succeeded in separating the water of crystallization, the salt being decomposed at 100°.

The salts of leucaniline are, in general, well crystallized. They are all very soluble in water, and precipitated from the aqueous solu-

tion by the addition of the respective acids. The sulphate crystallizes readily. I have submitted leucaniline to the action of disulphide of carbon, chloride of benzoyl, and several other agents. In all these cases new compounds are generated, some of them remarkable for their crystallizing power. The study of these substances does not belong to the present inquiry; I shall return to them in a future communication, in which I propose to examine the constitution of rosaniline and leucaniline, and their derivation from aniline. The object of the present Note was only to fix the composition of the two new bases, and their mutual relations to each other. This relation, as will be obvious by a glance at their formulæ, is of the simplest kind. In the anhydrous condition the two substances respectively contain,

Rosaniline .... $C_{20} H_{19} N_3$.
Leucaniline................ $C_{20} H_{21} N_3$.

Leucaniline differs from rosaniline simply by containing two equivalents of hydrogen more. The two bases hold to each other the relation which obtains between blue and white indigo:

Blue indigo.............. $C_{16} H_{10} N_2 O_2$.
White indigo .... $C_{16} H_{12} N_2 O_2$.

Leucaniline, as might have been expected, is readily reconverted into the red colouring matter by oxidizing agents. The reaction succeeds with peroxide of barium, perchloride of iron, or chromate of potassium. On gently heating the colourless solution of the chloride with one of these reagents, the liquid at once reassumes the splendid colour of the rosaniline-salts. An excess of the oxidizing agents is, however, to be avoided, lest the action should go too far and the regenerated compound be converted into further products of oxidation. Both rosaniline and leucaniline, when submitted to protracted ebullition with highly oxygenated compounds, yield a brown amorphous powder, the composition of which remains at present unknown.

The two bases which I have described in the preceding paper are the prototypes of two series of homologous colouring matters which cannot fail to be obtained from the homologues of aniline. Toluidine appears to yield perfectly similar bases. I have not, in the present Note, examined into the nature of the reaction by which aniline is transformed into rosaniline; in most of the processes which give rise

to this substance, it is accompanied by several other bases, the study of which is not yet completed. Nor am I at present in a position to offer any definite opinion regarding the constitution of the new compounds, tempting though it appears to venture on speculations. It is in the hope of rendering the formulæ of the new bases more transparent that I have commenced to examine some of the products of decomposition. Their study is likewise far from being completed; but I may mention, even now, that both rosaniline and leucaniline, when in nitric solution, are powerfully acted upon by nitrous acid, new bases being thus generated, the platinum-salts of which are remarkable for their fulminating properties. A splendid crystalline base also deserves to be mentioned, which, associated with aniline, appears among the products of distillation of rosaniline.

The results obtained in the further prosecution of these studies I propose to lay before the Royal Society in a future communication.

II. "On the Integration of Simultaneous Differential Equations." By GEORGE BOOLE, Esq. Received March 4, 1862.

It is well known that a system of $n-1$ simultaneous differential equations of the first order connecting n variables always admits of $n-1$ integrals, each of which is the form $P=c$, i. e. each of them is expressible by a function of the variables equated to an arbitrary constant.

But when the number of the variables exceeds by more than by unity the number of the differential equations, no existing theory assigns the number of theoretically possible integrals, or guides us to their discovery.

Yet cases such as this occur in problems of the greatest importance. The solution of partial differential equations of the second order by Monge's method depends ultimately on the solution of a system of *three* ordinary differential equations of the first order between *five* variables.

I wish here briefly to indicate the results of a theory which enables us in all such cases, 1st, to assign *à priori* the number of possible integrals; 2ndly, to reduce the determination of the integrals to the solution of a system of differential equations equal in number to the

number of the integrals, and capable of expression in the form of exact differentials.

I will confine my observations to the case of $n-2$ differential equations connecting n variables. The general theory will be seen in the particular one.

1. The solution of $n-2$ differential equations of the first order connecting n variables may be reduced to the solution of a system of 2 linear partial differential equations. To deduce these, let $P=c$ be any integral of the given system, and suppose $x_1, x_2 \ldots x_n$ the variables, then from

$$\frac{dP}{dx_1}dx_1+\frac{dP}{dx_2}dx_2 \ldots + \frac{dP}{dx_n}dx_n=0$$

eliminate by means of the given system $n-2$ of the differentials, and equate to 0 the coefficients of the two remaining and independent ones.

2. Let the two partial differential equations thus formed be

$$A_1\frac{dP}{dx_1}+A_2\frac{dP}{dx_2}\ldots+A_n\frac{dP}{dx_n}=0 \ldots\ldots\ldots\ldots \text{(I.)}$$

$$B_1\frac{dP}{dx_1}+B_2\frac{dP}{dx_2}\ldots+B_n\frac{dP}{dx_n}=0\,; \ldots\ldots\ldots\ldots \text{(II.)}$$

then representing

$$A_1\frac{d}{dx_1}+A_2\frac{d}{dx_2}\ldots+A_n\frac{d}{dx_n} \text{ by } \Delta_1,$$

$$B_1\frac{d}{dx_1}+B_2\frac{d}{dx_2}\ldots+B_n\frac{d}{dx_n} \text{ by } \Delta_2,$$

the equations become

$$\Delta_1 P=0, \qquad \Delta_2 P=0. \ldots\ldots\ldots\ldots\ldots (1)$$

Form now the equation $\Delta_1\,\Delta_2\,P-\Delta_2\,\Delta_1\,P=0$, or as it is permitted to express it,

$$(\Delta_1\,\Delta_2-\Delta_2\,\Delta_1)\,P=0. \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

This will also prove a linear partial differential equation of the first order; and if from it by means of (I.) and (II.) we eliminate $\frac{dP}{dx_{n-1}}$ and $\frac{dP}{dx_n}$, we shall obtain an equation of the form

$$C_1\frac{dP}{dx_1}+C_2\frac{dP}{dx_2}\ldots+C_{n-2}\frac{dP}{dx_{n-2}}=0\ldots\ldots\ldots \text{(III.)}$$

This we shall represent by $\Delta_3\,P=0$. The equations (I.) and (II.)

may be so prepared as to lead to this equation *directly*. To effect this, it suffices to eliminate from one of these equations $\frac{dP}{dx_n}$, from the other $\frac{dP}{dx_{n-1}}$, and to reduce in each the coefficient of the one which remains to unity, and then apply the theorem (2).

3. Between (I.) and (III.) and between (II.) and (III.) the same process may be applied as between (I.) and (II.). The effect of this is to give new partial differential equations; in fact, to generate a system which will be *complete* when the further application of the method gives rise to no new equations, but only to identities, or to repetitions, or combinations of the equations already obtained. And though any equation of the system may be combined with any other, according to the theorem, in order to form a new one, yet it may be shown that the system will be complete when no new equation arises from the combination of any with the original ones (I.), (II.).

4. Suppose that in this way m partial differential equations have been obtained, including those two into which the given system of ordinary differential eqtions was transformed. Then that system of ordinary differential equations will admit of exactly $n-m$ integrals, *i. e.* the number of integrals will be equal to the number of the variables diminished by the number of partial differential equations.

5. To determine these integrals, let the complete system of partial differential equations be represented by

$$\Delta_1 P=0,\ \Delta_2 P=0, \ldots \Delta_m P=0;$$

then multiplying the second by λ_2, the third by λ_3, &c., and adding, we have

$$\Delta_1 P+\lambda_2 \Delta_2 P \ldots +\lambda_m \Delta_m P=0,$$

a single partial differential equation, which, $\lambda_2 \lambda_3 \ldots \lambda_m$ being regarded as indeterminate, will be equivalent to the *system* of equations from which it is formed. Represent this equation by

$$X_1 \frac{dP}{dx_1}+X_2 \frac{dP}{dx_2} \ldots +X_n \frac{dP}{dx_n}=0,$$

then its auxiliary system of ordinary differential equations will be

$$\frac{dx_1}{X_1}=\frac{dx_2}{X_2} \ldots =\frac{dx_n}{X_n}.$$

If from these $n-1$ equations we eliminate the $m-1$ quantities $\lambda_2 \lambda_3 \ldots \lambda_m$, we shall obtain $n-m$ differential equations. *These wil. be capable of expression as exact differential equations, and wil. give by integration the* n—m *integrals before mentioned.*

The method above described admits of important applications. It enables us to assign beforehand the conditions of success in the application of Monge's and of similar methods to the integration of partial differential equations of the second order, and even to determine the nature of the theoretically possible integral where its actual exhibition in a finite form is impossible. It also enables us to investigate by a new and perfectly rigorous method the conditions of integrability of ordinary differential expressions.

I subjoin a single result of the former of these applications. It is known that the equations of the possible envelopes of any surface

$$z=\phi(x, y, a, b, c),$$

in which three parameters, a, b, c, vary in subjection to two conditions,

$$f_1(a, b, c)=0, \qquad f_2(a, b, c)=0,$$

will satisfy a partial differential equation of the form

$$\mathrm{R}r+\mathrm{S}s+\mathrm{T}t+s^2-rt=\mathrm{V}.$$

The application of the above method shows that, in order that this equation may admit of an integral of the above species, *i. e.* an integral interpretable by the envelope of a surface in which three parameters vary in subjection to two connecting relations, the following conditions are necessary and sufficient, viz.

$$\mathrm{S}^2+4\mathrm{RT}-4\mathrm{V}=0, \quad \ldots\ldots\ldots\ldots\ldots \quad (1)$$

$$\Delta\mathrm{R}+\Delta' m=0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

$$\Delta m+\Delta'\mathrm{T}=0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

in which m is one of the equal roots of

$$m^2-\mathrm{S}m+\mathrm{RT}-\mathrm{V}=0,$$

and

$$\Delta=\frac{d}{dx}+p\frac{d}{dz}-m\frac{d}{dq}+\mathrm{T}\frac{d}{dp},$$

$$\Delta'=\frac{d}{dy}+q\frac{d}{dz}-m\frac{d}{dp}+\mathrm{R}\frac{d}{dq}.$$

The first only of the above three conditions appears to have been assigned before (Ampère, Journal de l'École Polytechnique, Cahier xviii.).

March 13, 1862.

Major-General SABINE, President, in the Chair.

The following communication was read:—

"An Account of some Experiments with Eccentric Oblate Bodies and Disks as Projectiles." By R. W. WOOLLCOMBE, Esq. Communicated by Prof. STOKES, Sec. R.S. Received March 11, 1862.

It is known now that, especially in the larger calibres, the rifle principle has effected more for shells than for solid shot. A high initial velocity, it appears, cannot be attained with this principle and cylindro-ogival elongation; this slow initial motion is, however, but slowly lost; while in a spherical projectile, such as the 68-pound solid shot, the conditions are of a reverse kind.

The object of this paper is to place before the Royal Society an account of some experiments with models on a design which appears to me likely in large guns to effect, not only an initial velocity greater than that of spherical shot, but a terminal velocity better sustained than that of rifle projectiles.

Fig. 1.
Transverse section of eccentric disk, of actual dimensions.

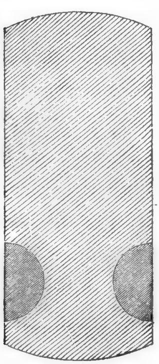
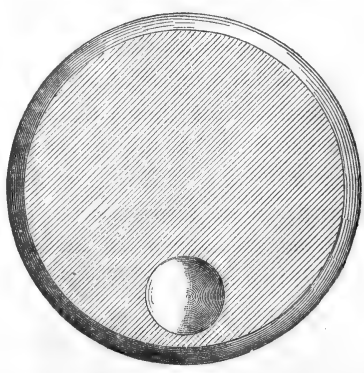


Fig. 2.
Side-view of disk, actual size; eccentric by a shallow and unfilled hole on each side



Average weight of disk in cast iron, $7\frac{3}{4}$ ozs., wrought iron, $8\frac{1}{4}$ ozs.

It is proposed to retain the circular periphery of a sphere only in the line of motion, and by cutting away the opposite sides of a sphere in parallel planes, say to half the radius, leave a disk, in that case with a zone of 60° (fig. 1).

I am informed by Professor Stokes, who kindly made the calculation, that such a disk (zone of 60°) is in volume or weight 1·45 times that of a sphere the sectional area of which is equal to the transverse sectional area of the disk. Could such a disk, fired from a gun of similar transverse section, be projected with sufficient cycloidal rotation to maintain it in one plane, assuming it fired in a vertical plane, the conditions appear favourable to dynamical effect at any elevation.

I find, however, that when *con*centric and homogeneous, a disk so fired from such a gun strikes a target, not in the vertical position as fired, but in any position, such as broadside on; and that it is necessary for the desired effect that the centre of the gravity of the disk should be slightly out of its geometrical centre, though not out of the equatorial plane, and placed in a certain position in the gun. I do not propose to employ eccentricity exactly as it has been employed in spheres, that is, to seek to gain range by the eccentricity as such, but chiefly to employ merely enough of it to secure due rotation, so as to make a disk, otherwise useless but at close quarters, a virtually elongated projectile, and dependent further for its effect on the more legitimate and substantial conditions of easily suppressed windage, rotation in aid and not at the expense of translation, facile displacement in the gun, and several other qualities, some of which are absent with spherical projectiles, and others incompatible with the rifle principle.

In the work entitled "Shells and Shell-guns*," by Commander Dahlgren, of the United States Navy, the history of the eccentric principle applied to spheres is treated at length, and by him traced back to the time of Robins, or for about 100 years; here, however, a further allusion would occupy too much space, though the history is an interesting one. In the fourth edition especially (or that preceding the last) of 'Naval Gunnery,' Sir Howard Douglas has given a more minute account than has Dahlgren of the experiments in England on this subject in 1850, 1851, and 1852, which were instituted at the suggestion of Sir Howard Douglas.

It is stated by him that it was by the experiments of General Paixhans at Metz, in 1841 and 1842, the fact was first established that the deviations of eccentric spherical projectiles could be made to occur at will, either in a lateral or longitudinal direction,—laterally,

* London, Trübner and Co., Paternoster Row, 1857.

by placing the shot with its centre of gravity to either side of the geometrical centre, to which side the deviation then occurred, and longitudinally, by placing the centre of gravity above or below; in the former position the range was increased, and when "below," the range was diminished relatively to the range of a concentric sphere of like dimensions, and of a weight approximately equal, but not necessarily exactly so. In these latter positions (*i. e.* in a vertical plane) there was found by General Paixhans to be also a relatively reduced amount of lateral deviation in comparison with that of common spherical projectiles; in shot the difference was as 8 compared to 13, and in shells as 2 to 16*. In the English experiments, however, of 1850, 1851, and 1852 it does not appear, from the published results, that lateral deviation was thus reduced, excepting at some or the longest ranges†. Of these the greatest was with a 68-pounder of 95 cwt., charge 12 lbs., elevation 24°, the shot being hollow and eccentric (but its weight and mode of eccentricity not mentioned); this shot ranged to 6500 yards‡, while the greatest range at the Deal experiments of A.D. 1839 with a 56-pounder gun and solid shot, 16 lbs. charge instead of 12 lbs., and 32° elevation instead of 24° only, was 5720 yards§.

The conclusions arrived at in England, France, and America from the results of experiment with eccentric spherical projectiles appear to be very similar, as regards the general inutility of the eccentric principle for any but certain exceptional occasions in warfare, such as the bombardment of a distant but very extended area. It has, however, been used in spherical projectiles in the Prussian field artillery‖; and Dahlgren states that when the centre of gravity, of a shell that has no more eccentricity than about $\frac{1}{54}$ of its weight added about the interior of the fuse-hole, is placed in the axis of the bore¶, or rather parallel thereto as regards the geometrical centre, the lateral deviations are nearly annulled**, and the longitudinal

* Naval Gunnery, ed. 4. p. 152. † Ib. p. 166. ‡ Ib. p. 168.

§ These two ranges are the greatest of any recorded in the 'Naval Gunnery,' with respectively eccentric and concentric spheres.

‖ Vide Taubert on the 'Use of Field Artillery,' translated from the German by Lieutenant Maxwell. London, J. Weale.

¶ Dahlgren says that such use of the eccentric principle is made in the U.S. Navy for shells.

** Dahlgren, p. 94.

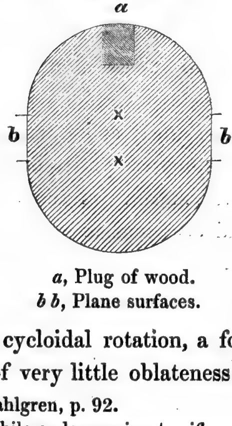
variations are much less than those of an ordinary shell not made purposely eccentric.

He states that, *con*centricity* being unattainable in shells, it is needless to inquire whether that or eccentricity is to be preferred, the real question being how best to deal with the eccentricity of all shells.

Of solid shot, Sir Howard Douglas remarks that not more than one out of a hundred, when floated in mercury, remained indifferent to the position in which they were placed in the mercury; while it was made manifest, by the experiment with eccentricity, that that quality was of all others by far the most fertile cause of deviation.

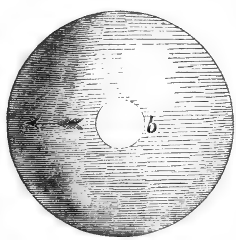
I now proceed to my own experiments†. My first idea (in 1854) was to employ, with the least amount of eccentricity sufficient to

Fig. 3.
Actual size of bore and transverse section of spherical shot (allowing for windage).



a, Plug of wood.
b b, Plane surfaces.

Fig. 4.
Side-view of spheroid.



effect cycloidal rotation, a form of projectile and section of bore of gun of very little oblateness. I procured two model mortars; one

* Dahlgren, p. 92.

† While endeavouring to rifle a small model mortar, and holding it obliquely, I was struck by the elliptical form of the muzzle presented by thus inclining the circular bore to one side; and happening at the time to be thinking of some of the details of the experiments with eccentrical spherical projectiles in the 'Naval Gunnery,' it occurred to me that if a gun was elliptically bored, but the bore straight and not helical, the long axis being in a plane perpendicular to the common axis of the trunnions, and the shot an oblate spherical *ec*centric (as already described), and properly placed in the bore, such would be an advantageous application of the eccentric principle, as the shot would rotate *ab initio* about its natural or shortest diameter, and the direction of such axis and of the plane of rotation could not alter within the gun, or be likely to alter through the air.

was bored at first to an ellipticity differing but little from a circle, not, however, a true ellipse, but two semicircular arcs, on centres a little separated, connected by straight lines at the periphery; and corresponding projectiles were made similarly differing from the true form generated by an ellipse about its minor axis. The difference, however, between the long and short axis of the figure of the shot was insufficient to obviate its getting crosswise in the bore, by means of the necessary windage to allow of free rolling in the bore.

The mortar was then re-bored to its present dimensions, by the kind aid, in lending instruments, of Mr. George Hoffman of Margate. Fig. 3 represents a section of the bore in its present state.

On this very small scale nothing, however, of any consequence could be ascertained in either force or accuracy, though a singular result appeared as to effect of relative position of centre of gravity; for in both models the longest ranges were afforded by a position of the centre of gravity which was the reverse of that giving the longest ranges in large guns.

Ranges.	Large guns.	Models.
Longest	Above	Below.
Second	Behind	Behind.
Third	In front	In front.
Shortest	Below	Above.

The reason seems to me, as regards the models, to be, that the powder has more time for complete ignition in such very short tubes when the shot is in stable equilibrium than when in the reverse position. The mortars were used as guns at low angles.

Fig. 5.
Side-view of disk of 5 ozs.
Actual size.

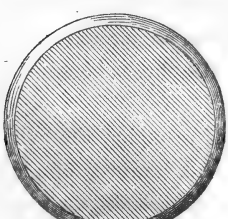


Fig. 6.
Section of disk.

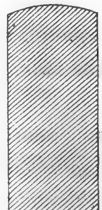
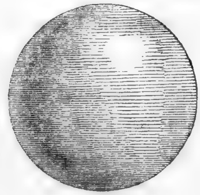


Fig. 7.
Sphere of 5 ozs.



I adapted one of them to project disks (figs. 5, 6) which were of about the same weight (5 ounces) as the spheres (fig. 7) for the other mortar.

I also made the mortars of the same length, namely small. They were fired from a moveable wooden platform, but each from the same bed or block of wood, which slid in a groove in the platform; the bed admitted of their being fired at the horizontal and at low elevations.

Recoil could be marked; the usual charge was 3 drachms of fine canister powder; the disk caused more recoil than the spherical shot, as in the former windage could be more effectually suppressed. Centre of gravity *below* in both gave more recoil than centre of gravity *above*. The eccentric disks and spheres were *usually* fired with centre of gravity *below*. The disk ranged to first graze about $\frac{1}{3}$, and at the extreme range about $\frac{3}{5}$ further than the range of the eccentric spheres; that is, as 4 to 3 to first graze, and at the extreme range (after grazing) as about 8 to 5. But there can be little doubt, from the light thrown on this point by my later experiments, that no sufficient rotation of a cycloidal kind could have been imparted to the disks from the mortar, the centre of gravity being below, but only from their striking the sand, as if the disk were bowled from the hand; the disks ricochetted to between 600 and 700 yards up to 3° elevation, above which angle there was no good ricochet. The mortars were about 10 or 12 inches above the level of the sand.

When a disk touched a rough place, though much oscillation was set up (as known by the noise it produced), this lasted only to the next one or two grazes; for at the end of the range, where the disk rolled before stopping, and the sand happened to be soft and dry, the track was continuous like that of a wheel, and in a line that was very straight.

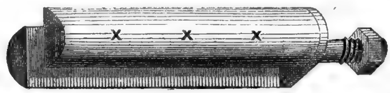
Some experiments with these mortars and disks were shown by me at Shoeburyness in 1855; but the ground there consists of mud with pools, and is not level enough for so small an apparatus. After a few comparative trials of the two mortars, the cheeks blew out from the disk mortar.

In 1859 I was afforded an opportunity of resuming the subject; but still, necessarily, with a *model* only; instead, however, of a length of 3 calibres only, as in the mortars, I had a disk-gun made of between 10 and 11 calibres, or about howitzer proportion; and instead of, as previously, a weight of disk of 5 ounces, the weight was about 8 ounces. Length of bore of gun 20 inches, long diameter of

bore $1\frac{7}{8}$ inch, and of disk nearly the same, and transverse diameter $\frac{3}{4}$ inch (see figs. 1, 2). Thus the disk was virtually a slice from a sphere, and across a zone of about 48° or 49°. Its long diameter was about $2\frac{1}{2}$ times the short diameter.

The gun was first bored to a cylinder, and the bore was then reduced to the proper shape by the insertion of two cheek pieces. I had difficulty in securing the cheeks of the gun, and found, after two failures with side-bolt fastenings, that it was requisite to secure the cheeks in the line of their length by attaching them to the head of a bolt (like the prongs of a tuning-fork to its handle), which passed through the breech end of the gun in the line of its axis, and was secured on the outside on its projecting screw end by a nut (see fig. 8).

Fig. 8.—Perspective view of cheeks.



The crosses show places for side-bolts to keep cheeks close to sides of cylindrical bore.

The gun was made so that the longer axis of the bore was, throughout the gun, perpendicular to the common axis of the trunnions. The gun weighed about 130 lbs. It was simply a cylindrical block with trunnions; I had, *in it*, to learn how to effectually secure the cheeks, and therefore had the gun made of a thickness otherwise unnecessary.

The first disks made were cast with the mould in a *horizontal* position; and several such disks were fired; but scarcely one of them had the centre of gravity in the equatorial plane; and I found that a disk that would not roll tolerably straight (as on a level table), had corresponding lateral deviation in the air when fired. Other disks were then cast with the mould in a *vertical* position, and these were much more symmetrical in respect of the sides.

The eccentricity was given at first by a hole through the disk, plugged with gutta percha, or with an alloy of lead and tin; experience, however, showed that plugs of any kind, though riveted, were often blown out, and could only be secured by being *screwed* in; and eventually I found no way better than to employ symmetrical shallow cavities* *un*filled, on each side of the disk, at about half the radius from the centre. The least amount of metal

* I am indebted to Mr. Braid, late R.A., of the Dep. Military Prison, Devonport, for his kindness in making these cavities in the disks by a lathe.

abstracted, which I found would effect rotation, was $\frac{1}{120}$ only of the disk's weight, and was removed from the sharp edge at the sides of the zone by filing at four points. Rotation was effected also in a homogeneous disk, and without any cavity, by making it slightly oval in periphery—in fact, as if it were a middle slice from a *very slightly* ovoid or egg-shaped body, instead of from a spherical one, the larger end or heaviest part being put uppermost in the bore. Excepting one other form, better described presently, these were all the forms I experimented with.

The gun being too small to effectually destroy a sabot, I commonly used a horse-shoe electro-magnet sliding in one end of a flat wooden rod of similar section to the bore of gun, with which to place the shot in any desired position. Having previously marked, by a spot of chalk, the face of the shot to be seen in front when in its place, I placed the shot as desired in the bore, and then by throwing in light with a mirror, I saw that the position was that wanted. Sometimes, if the shot turned in entering, it could not be again withdrawn but by firing; and thus such cases conveyed no meaning, unless the spot of chalk was not altogether out of sight, in which case the position and result were recorded.

In September 1859, by the kind permission of Captain Jerningham, R.N., in command, the gun was placed on board the 'Cambridge,' the gunnery ship at Devonport.

The first experiment was to ascertain the ranges due to centre of gravity above and below; and this would also show whether rotation occurred in one or the other position, or in both. Four disks were selected of within a few grains' weight of one another, average weight $7\frac{3}{4}$ ounces; three of the disks were to illustrate respectively *con*centricity, and the two opposite positions of centre of gravity "above" and "below." The charge was $1\frac{1}{2}$ ounce, and the elevation 5° in each case; the powder was that known as "Lawrence's No. 4, large-grain," and is a powder of great strength.

The *con*centric disk dropped at 550 yards; the *ec*centric with centre of gravity "below," at 500 yards; the *ec*centric with centre of gravity "above," at 1000 yards. The two first-named disks made much noise in passing through the air; the long-ranging disk, fired with centre of gravity *above*, made but little such noise.

The other eccentric disk was then fired with 2 ounces instead of the

$1\frac{1}{2}$-ounce charge, and at 10° instead of 5°, the centre of gravity being above as before. This shot was neither seen to drop in the water nor heard to make much noise. There was about 2000 yards of water then in the creek; and as the water was smooth, and many practised eyes were looking out, it was thought likely to have passed all the water and fallen on the mud. This view was much confirmed by three similar eccentric disks being similarly fired a few days after, *i. e.* with 2 ounces of powder and 10° elevation, and centre of gravity above. On this occasion Capt. Jerningham kindly sent out a boat near the 1500-yard range, and men were stationed about the ship to observe. The water was smooth, and, as before, there was about 2000 yards of it in the creek. Not one, however, of these three disks was seen to drop by any one, nor were they heard from the boat, so that there could be little doubt that rotation was established. A fourth eccentric disk was entered, but stuck in the bore, and was pushed down in a position unknown. This was also fired at 10°, and with the same charge as the preceding, and it was seen to drop at about 1000 yards, and was believed to have had no regular rotation. On Oct. 5th, 1859, the gun being on the lower deck, about 11 feet above the water, two eccentric disks were fired with centre of gravity above, the gun being laid horizontally, or what is called point-blank; charge as before, 2 ounces. The first graze of both of these shot was between 600 and 700 yards; and there could be no doubt of the range being due chiefly to velocity, and not to vertical deviation, the graze of the shot succeeding so immediately the discharge from the height of the gun from the water having been about 11 feet, and the gun horizontal. The range, if not due to vertical deviation, must have been due to a velocity of more than 2000 feet per second. In these early experiments with the model disk-gun, I had not the advantage (as at present) of having previously fired at timber, so as to have learned unmistakeably the effect on the position (as in striking in a vertical or oblique plane) of different amounts of lateral (or undesired) eccentricity; also I knew nothing of the injurious effect of sabots on the shot's rotation when the sabots were too substantial; consequently the majority of these earlier experiments were most uncertain, and could not be repeated at will.

I have, however, since, by firing at timber, learned the conditions which secure certain results; and these I will briefly state. Excepting when the centre of gravity is "above," or within a few degrees of

such position, a disk strikes a target not in a vertical plane as fired; but when the centre of gravity is "above," when the disk is free to roll and not merely slide in the bore, when the sabot, if any, is very light and destructible, as of card, when there is a sufficient charge of powder, and the disk is tolerably symmetrical laterally, and sufficiently eccentric longitudinally (but which eccentricity need not be an amount that causes a dip of more than 1° when the disk is floated in mercury), then the disk, if fired in a vertical plane, is certain to strike a target in that position up to the distances at which I have yet had the opportunity of trying it; and though such distance (from the land experiments here having been of necessity in a quarry *) has been only from twenty to thirty yards, yet, *as in the other* positions of the centre of gravity the disk turns over irregularly *within such distance*, it may be assumed that a rotation in a vertical plane is set up in the one position referred to, viz. "above," and in no other position of the centre of gravity. To this conclusion all these experiments appear to tend. It may by some be questioned whether this rotation is *as* a wheel, or the reverse way, by the advancement of the lower part of the shot. These experiments do not appear to support the conclusions of M. Magnus† (which have been so very widely adopted, as by Sir Howard Douglas in his fourth and subsequent edition), viz. that rotation occurs in both positions of "above" and "below," but in the latter only is *as* a wheel; while previously, and as expressed in his third edition, Sir H. Douglas entertained the opinion that the rotation was in that direction, or by the advancement of the upper hemisphere, when the centre of gravity was above. It is probable that rotation in a disk, in *either* direction, would keep its plane vertical when the projection had been in a vertical plane; so that if it strikes upright only when fired with the centre of gravity in one position (as when "above"), it seems a fair conclusion that with the other positions of the centre of gravity there can be no rotation imparted.

This I had reason to suspect, as regards the position of the centre of gravity "below," long before I had an opportunity of proving it with the disk-gun; for in 1854, the model-mortar experiments referred to

* Capt. Bent, R.A., of the Royal Laboratory, St. Budeaux, near Devonport, was so kind as to afford the ground for these land experiments, which otherwise I should have been unable to carry out.

† M. Magnus's Paper on Deviation of Projectiles in 'Taylor's Scientific Memoirs,' Nat. Phil. Part III. for May 1853.

appeared to indicate that such was probably the case, by demonstrating that not only were the vertical deviations from such models the reverse of those in large guns, while the lateral deviations were the same, but that it followed from this there must be a length and calibre from which, while the lateral deviations still remained constant, the range would be the same whether the centre of gravity of an eccentric sphere were put "above" or "below." What, then, becomes of the theory that the lateral and vertical deviations are due to the same proximate cause, *i. e.* eccentric rotation through the air, and that it is *by* the air, as assumed by M. Magnus, that both these deviations occur? The later disk experiments, *i. e.* from the gun, show that in three out of the four positions of the centre of gravity in one plane (a vertical plane) there is no decided rotation in such plane, or indeed regular rotation in any plane; yet these three positions in spheres all give different ranges.

Do these spheres rotate? or can they in such case rotate in a vertical plane with velocity enough to cause a vertical deviation, assuming that the mean length of a range admits of being increased in this manner, in opposition to gravitation?

Perhaps the approximate causes may be looked for (of the two kinds of deviation), the one more *within*, and the other chiefly *outside* the gun.

As regards the longer ranges due to centre of gravity "above," may not such increase be due to the fact of the nearer coincidence of that important point with the axis of the bore? In fact, may not the vertical variations in range be chiefly due to causes of a more directly dynamical nature than has been generally thought? while, respecting the lateral, M. Magnus's views, founded on his experiments with rotating bodies, appear not only incontrovertible of themselves, but the identity of such deviations in models with those of large guns offers no new fact on which exception could be taken or any new question raised, which cannot be said of longitudinal deviations.

To find whether a disk prevented from rotating in the bore, but still delivered at the muzzle with centre of gravity "above," would rotate in, and in such case by, the air, I made two disks (with a straight edge above and below), the disks being very eccentric by a transverse hole through the lower part.

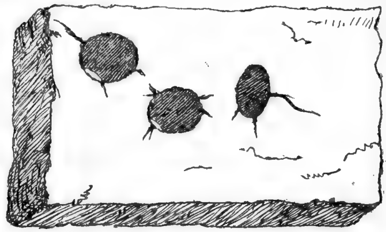
They both struck irregularly in any position, as seen by the wood of the target, which shows also the form of the disks.

I found that a homogeneous solid disk, formed slightly oval below, would strike as fired (in a vertical plane) when the centre of gravity was "above."

There are three ways in which, as I have found, disks may pass through the air (as seen by the target, and shown in figs. 9, 10, 11).

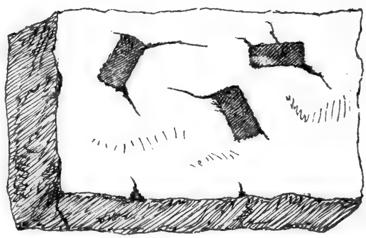
1st. *Con*centricity, or *Ec*-centricity, but with centre of gravity not "above," causes a disk to strike in any irregular position (fig. 9).

Fig. 9.



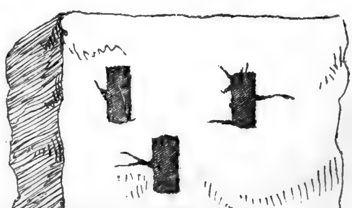
2ndly. When the centre of gravity is "above," but there is something within the bore to hamper but not arrest the rotation of the disk in the bore, such as too thick a sabot, or great fouling and insufficient windage, the disk strikes with the edge, but not upright as fired (fig. 10).

Fig. 10.



3rdly. Centre of gravity "above," with the attending conditions which have been before mentioned, gives a result as previously described and shown in fig. 11.

Fig. 11.



The difficulty of destroying "sabots" is much greater in models than large guns.

The penetration of disks when striking with the edge is great; with a 2-oz. charge, at twenty-five yards, the penetration has commonly been through three 4-inch planks of elm; and with half an ounce more powder and a wrought-iron disk, through three 6-inch beams of elm, the latter with the grain of the plank parallel to the plane of the disk, the former with it usually transverse. The difference of direction in grain of wood causes in such experiments about 2 inches difference in penetration.

I beg, finally, to sum up briefly the conclusions which appear to me to be deducible from these experiments.

1st. That the experiments with the model mortar, by giving the longest range and the shortest that are due to certain positions of the centre of gravity in a vertical plane in positions the reverse of those obtaining in large guns, while the other positions remain the same, as to their effect on the range, as in large guns, appear to render admissible the view that the causes of lateral and vertical deviations, which have hitherto mostly been assumed to be similar, may not be so.

2ndly. That from the above experiment it also results that there must be *a length* and calibre in which the range will be the same, whether the centre of gravity of an eccentric spherical projectile be placed above or below, while in the same gun all the other deviations due to the other positions will be similar to those obtaining in both large guns and small models.

3rdly. It appears (at least with the dimensions of gun and projectile here experimented with) that there is no decided rotation in any of the four positions in a vertical plane, excepting that of centre of gravity "above" the geometrical centre; and it may perhaps be fairly assumed that in the positions in which disks do not rotate, spheres (at all events, of like dimensions) cannot.

4thly. That if the results of these experiments with the model disk-gun may be viewed as indicative of similar results from large guns, then the above-mentioned phenomenon of rotation *in one position only* renders doubtful the previous conclusions on the direction of rotation, which have been based on a view of the rotation not being thus limited to one position.

5thly. It appears that to rotate outside the gun, it is requisite that the disk (and probably sphere also) must be free to rotate within it.

6thly. That rotation may be imparted sufficient to be permanent on one axis, but not in one plane—a matter of no consequence at close quarters; while, by certain means, rotation may approximately be secured in one plane when the projection has been in a vertical plane; this has been seen at least up to thirty yards at a target, and longer distances not yet tried at a target; but much within the above distance the other phenomena are seen to occur; and it may be assumed that if a disk will keep upright through several inches

of solid timber, it will also keep upright through the air, except in much wind, against the effect of which, however, the disk may probably be preserved by inclining the axis of the gun.

7thly and lastly. The before-mentioned results show that a disk-gun may in certain respects be viewed as a common gun, and in other respects as a gun for throwing an elongated projectile. The former characteristics, as of circular periphery in line of motion, ensure high initial velocity and small strain on the gun; and the latter, or virtual elongation, ensures the preservation of such velocity; for it is seen that the requirement of the tangent to the trajectory, so desirable respecting the proper axis of rotation of a rifle projectile, does not obtain in the disk; and it is also seen that while the rifle projectile can strike effectively but in the prolongation of one of its axes, and that becomes impracticable as elevation increases, the disk has no such limitation, and is not dependent on any one angle of elevation for preserving inviolate the conditions for which elongation is given to any projectile*.

* On March 20, in the week following that in which the above was read at the Royal Society, four disks were fired at Shoeburyness, in the presence of Col. Taylor, R.A., Commandant and Superintendent, and other officers. The weather was so wet, the tide also not admitting of the gun being loaded early in the day, that four disks only were fired at a target, first of oak, 9 inches thick, and afterwards with about 4 inches of deal behind it; gun twenty-five yards from target.

Experiment of March 20, 1862.

Powder.	Disk eccentric by between $\frac{1}{50}$ and $\frac{1}{60}$ abs. at $\frac{1}{2}$ Radius.	Position of centre of Gravity.	Remarks.
Ounces. $2\frac{1}{4}$	Between $7\frac{3}{4}$ and 8 ounces.	Behind, in axis of bore.	Hit broadside on, and went deep into, but not through the oak; no splintering or appearance on the far side of the oak.
$2\frac{1}{4}$	Ditto.	Above.	Passed through the oak into the sea. Hit vertically as fired.
$2\frac{1}{4}$	Ditto.	Behind, in axis of bore.	Hit vertically, and passed into the sea, through the oak and the deal backing (oak 9 inches, deal 4 inches).
$2\frac{1}{4}$	Ditto.	Above.	Hit vertically; splintered the oak on far side, but did not go through it. This I stated was a "low" shot (in windage), before firing.

March 20, 1862.

Major-General SABINE, President, in the Chair.

The following communication was read :—

"Suggestions for the Attainment of a Systematic Representation of the Physical Aspect of the Moon." By John PHILLIPS, M.A., LL.D., F.R.S., Reader in Geology in the University of Oxford. Received January 15, 1862.

I. SKETCH OF THE PROGRESS OF SELENOGRAPHY.

(a) *By Eye-draughts and Micrometry.*

1. Beginning with the labours of Hevelius (1647), maps of the moon, embracing the whole, and signalizing special parts, have been repeated by Riccioli (1651), Cassini (1680), Lalande (1787), T. Meyer (1748), Lambert-Schröter (1791), Lohrmann (1824), Beer and Mädler (1836).

2. The degree in which these laborious efforts may be regarded as meeting the wants of "Selenography," is about equal to that in which the maps of England of the last century satisfy the requirements of physical geography; and in the same proportion as the great one-inch Ordnance Map of 1862 is superior to the old Chart of 1800, so should be the new drawings of the features of the moon to the older delineations.

3. That such drawings are attainable by the patient employment of modern instruments, in hands capable of good sketching, is, I believe, not doubted by any competent observer with either achromatic or reflecting telescopes having equatorial mounting. If any one doubts, let him compare the Copernicus of Mädler with the Copernicus of Secchi; nay, I may venture to ask that my own Gassendi be placed side by side with that of any of the charts already named.

4. The results likely to be attained by such a series of careful drawings of special parts of the moon's surface, in one branch of scientific research, are recognized by Mr. Conybeare in his Report on Geology to the British Association in 1832. Indeed, it may be boldly affirmed that a competent theory of volcanic action can hardly

be regarded as having been adequately tested, much less completed, without a careful study of the magnificent volcanic surface of the moon, where for the most part the consolidated products of a long train of igneous eruptions are exhibited as clearly as in the celebrated region of Auvergne.

5. Considerations of this kind pressing upon Lord Rosse, Dr. Robinson, General Sabine, and other persons acquainted with the growing power of telescopes, and the necessity of organizing a system for the use of them on the moon, induced the British Association, assembled at Belfast in 1852, to constitute a Committee, consisting of the Earl of Rosse, the Rev. Dr. Robinson, and Professor Phillips, for the purpose of drawing up a Report on the physical character of the moon's surface as compared with that of the earth.

6. Acting as Secretary to this Committee, Professor Phillips forwarded invitations to fourteen selected observers, in Great Britain and Ireland, the United States, and several localities in Europe, known to be in possession of adequate instrumental power, or willing to provide it. To each observer a certain limited tract was offered, his peculiar work, but everyone was requested to add whatever information he might judge useful relating to other parts of the moon's surface.

7. The answers to these invitations were for the most part favourable as to good intentions; but in several cases want of adequate leisure, sometimes want of health, sometimes other causes were mentioned; and practically it was found that very few of the selected observers sent contributions which fulfilled the wishes of the Committee, even as preliminary surveys. The Secretary of the Committee, indeed, constructed an equatorial of large size for his own share of work, mounted it in the open air, made photographic and eye-drawings, and completed a sketch of his appointed region on the 19th of May, 1853, which sketch has been in the hands of the Royal Society. He thus established, to the satisfaction of several friends, the facility of carrying out the desires of the Committee, and would have taken up fresh districts, on every suitable occasion, but for the change of his residence from York to Oxford. The instrumental mounting being specially fitted for York and the circumstances of his residence there, he was unable to continue his work at Oxford; and several years, as far as this problem is concerned, have

been lost to him for want of an instrument of adequate power and suitable construction, conveniently placed and always at command.

8. Mr. Nasmyth, several years since, employed his fine reflector, with a peculiar apparatus for drawing, in these representations of the moon, which have justly earned for him a reputation in philosophic art of which even the inventor of the steam-hammer may be justly proud. He has lately preferred to use for his eye-draughts a fine achromatic by Cooke, of York,—the same instrument which has been turned with such unexpected results to a scrutiny of the solar spots.

Professor Smyth of Edinburgh, and Professor Challis of Cambridge, made examinations and preliminary sketches of the Mare Crisium, Plato, and other interesting objects; the former artist employing oil-colours in his scene-painting.

(b) *By Photography.*

9. Meantime a new and beautiful art was making itself auxiliary to the delineation of the moon,—first by the silver plate of Daguerre, afterwards by the increasingly sensitive collodion surface. The great achromatic of Cambridge, U.S., under the hands of Bond and Whiffle, gave results of much promise; at first the light-pictures were of the full moon, 2 inches diameter on daguerreotype plates *; afterwards we saw larger representations of the crescent moon, with stronger lights and shadows on the ridges and in the hollows, several inches across (1851–53).

10. While observing with the great reflector at Birr, every one was struck with the probability that almost instantaneous pictures could be obtained of the moon, stars, and planets, by the amazing quantity of light brought to the focus of that magnificent instrument. Some trials had indeed been made in 1852 by the distinguished constructor and Mr. Woods; but I am not aware of the results of later experiments with the great reflector. In 1853 I gave much attention to the use of collodion, and employed an apparatus attached to my achromatic (of 11 feet focus and 6 inches diameter), by which at first pictures of 1·2 inch diameter, and finally others from 2 to 3 inches were obtained, in times gradually reduced from 5′ to 30″ and less. I still possess many of these pictures; the best, however, was destroyed in attempting to print from it.

* Kosmos, iii. 2. 362.

11. Somewhat earlier than these trials of mine were the first efforts of Mr. De la Rue, of which I was not aware. These efforts have from year to year been rewarded with still increasing success, till we have had from his skilful hands maps of the full moon of positive value, and stereographic pictures of admirable beauty. These researches are still in progress, with every prospect of reaching a point from which eye-draughts may be started on a fresh basis for a systematic scrutiny of all parts of the moon, and the construction of maps on the scale of $\frac{1}{20}$th of an inch to a mile on the middle part of the moon's face (or as the moon would appear under a power of 1000).

12. Experiments rewarded by considerable success were completed by the Liverpool Photographic Society in 1854; and several of their valuable drawings of the moon, magnified to a large scale, were exhibited at the Meeting of the British Association in Liverpool, along with one of mine similarly handled.

II. Proposal of a Method for further progress.

13. By the labours, for the most part uncombined, of the last ten years, we have not achieved much beyond laying the foundation for further progress. We have acquired, by means of photography, a general view of the whole moon as to its proportions of light and shade, the degrees of light of different parts of its surface, the direction of the light-streaks, and other phenomena, better than eye-draughts and micrometry could furnish.

By eye-draughts and micrometry alone many of the "mountains" and "seas" of the moon have been sketched in beautiful landscapes by Nasmyth and Smyth; and two "ring mountains" have been surveyed and drawn in detail by Secchi (Copernicus) and Phillips (Gassendi).

The next ten years may, doubtless, be justly expected to give an equal rate of progress; photographic foundation will be made more effective for the whole moon and for different phases of the moon; and we may add, by individual and sporadic efforts, a few more ring mountains to our meagre catalogue of objects examined. It appears to me, however, that more than this can be attained, and ought to be attempted, on a plan of continuous work, by means of one instrument devoted to a survey of selected parts of the moon, and I proceed to explain my views.

14. By Mr. De la Rue's exertions principally, photographs of the moon have become an essential element in the desired delineations, and an impression is sometimes felt that by some possible further improvement in that wonderful art, eye-drawing may be dispensed with. This, I am persuaded, can never happen; but there is in my mind the firmest conviction that eye-drawing, founded on a basis of form obtained by photography, will produce results as to details of the moon's peculiarities which light-pictures alone can never reach. For whether the large photographs, on the scale of 100 inches to the moon's diameter, which we desire to obtain, are to be had by enlarging the primary pictures of 1 or 2 inches, or by direct photographs on a larger scale, it seems impossible to escape from some want of definition, by reason of the imperfect surfaces used, or by reason of the inexact following of the moon as she changes her rate or alters her declination. I know this latter error to be very likely of occurrence, even with disks taken beyond the negative eye-piece, with excellent clockwork movement, and am, on this account, the more ready to applaud Mr. De la Rue, whose skilful hands have so well mastered that and other difficulties. I cannot too strongly express my sense of the great value of the light-pictures obtained by that gentleman—as *a basis of form* on which to construct eye-draughts, showing the mind's interpretation of what the eye sees on the moon, but fails to discover in the finest photograph.

15. Reflecting on the comparatively very small degree of success which has rewarded the combination instituted nearly ten years since by the British Association,—remembering that instrumental means have been improved, while the scientific interest in a knowledge of the moon's peculiarities has not diminished,—it appears to me possible to obtain a larger measure of success by a vigorous effort in a different direction. It appears to me that, instead of requesting gentlemen who possess instruments already engaged in other researches to turn them to selenography and make drawings in which they may have no special interest, it will be better to carry a good instrument to an observer interested in the survey of the moon, and willing for a limited time to use his exertions for the accomplishment of a definite object. In my own case I feel sure that this would succeed; and I believe that my case is essentially that of many

intelligent observers of the moon accustomed to extra-meridional observations.

16. The first desideratum then is an Equatorial Instrument, constructed with the conditions of ample optical power,—great steadiness,—delicate adjustment, including a sufficient range for latitude,—the usual circular and micrometrical readings,—clock-movement, &c.,—so that it may be in every point of view adapted for special observations of the moon (sun, planets, comets, &c. may also be observed), and be available for many years, in the same optical and space-measuring condition. According to my view, founded on experience with various instruments, it must be an achromatic, mounted on a transportable solid stand, placed under the roof of a removeable observatory, capable of holding a clock and, if need be, a small transit. The object-glass should be of 6 inches diameter, the focal length 16 or 17 diameters.

Such an instrument has actually been made by my direction; it is finished, and stands complete in the workshop of the skilful artist, whose name is a guarantee of excellence, Mr. T. Cooke of York. Thus the first requisite to give effect to my proposal is practically reached.

17. The second desideratum is that the instrument shall become the property of some scientific body constituted for long endurance, and endowed with so much influence as to be able to give effect and gain adherence to a plan of continuous work, by definite persons, for such periods of time as each in succession may command. The instrument to be confided to each in succession, and mounted in a convenient manner for his use, at his home, during the time appointed. Each observer to furnish, at least once a year, an account of his observations, with drawings on the plan already detailed in the instructions furnished by the Moon Committee of the British Association. At the conclusion of his appointed period of observation, the instrument to be again at the disposal of the scientific body to which it belongs, either to be transferred to another observer, or to be again entrusted to the first observer, according as may seem best for the attainment of the object in view.

18. I entertain no doubt that, after the operation of one or two years, each yielding fruit, there will be no other difficulty of obtaining

suitable observers than the difficulty of choice among several proper persons, who will be glad to give their services. To remove any difficulty as to the first trial, I presume to offer for the first two years my own services at Oxford; having already sketched out a definite plan of work, which has not yet been attempted, and which I believe myself able to accomplish.

19. It would be no part of my plan to take photographs of the moon, but rather to obtain from other observatories the best examples of this kind of work, and devote every available hour to eye-sketching on a large scale of the exact appearance of selected parts of the lunar disk.

The drawings thus made, scrutinized and corrected in succeeding years, would gradually and not very slowly grow up to complete eye-draughts of the moon, under the conditions of sunrise, midday, and sunset; and would themselves be again a starting-point for the guidance of even closer scrutiny, with the greatest telescopes and the sharpest eyes.

20. Finally, my proposal, if allowed to make one, would be, that, for the purpose of securing a series of satisfactory drawings of the physical features of the moon, a six-inch achromatic, by Cooke, constructed for the purpose, be purchased out of the funds of the Government Grant Committee, and held by a Board composed of three Members of the Royal Society, to be nominated in the first instance and the number afterwards filled up by the Council of the Royal Society, in trust for the use of observers to be appointed by the Board, each for a limited period, and for a defined area of work: the drawings and observations to be communicated, at least once a year, to the Board. Cost of the instrument not to exceed 320 guineas, of a moveable house not to exceed £50.

March 27, 1862.

Major-General SABINE, President, in the Chair.

The following communication was read:—

"Theoretical Considerations on the Conditions under which the Drift Deposits containing the Remains of Extinct Mammalia and Flint-implements were accumulated; and on their Geological Age." By JOSEPH PRESTWICH, Esq., F.R.S., F.G.S. Received March 20, 1862.

(Abstract.)

In his former paper on the subject of the Flint-implements*, the author postponed the consideration of the theoretical questions, to allow time for a fuller investigation of the physical phenomena. The points then sought to be established were,—the artificial make of the specimens,—their position in undisturbed ground,—and their contemporaneity with the extinct animals. The points for present consideration relate to the structural and physical phenomena, and to various theoretical questions.

In the present paper the author proceeds to show that the flint-implements are found along the line of existing river-plains at heights varying from 20 to 100 feet above the rivers, and that the beds of sand and gravel in which they are imbedded can be divided into two more or less distinct series, one continuous along the bottom of the valleys and rising but little above the river-level, and to which he proposes to apply the term "*Low-level Gravels*," and the other in detached masses on the heights flanking the valleys, and at 50 to 200 feet above the rivers, and which he designates as the "*High-level Gravels*†." Both gravels consist of débris derived from rocks in the valleys through which the present rivers or their tributaries flow, and they both occasionally contain organic remains; both are, in fact, related to former plains and present valleys.

* Read before the Royal Society 26th May, 1859; Phil. Trans. 1860, p. 277.

† At the reading of this paper, the author used the terms "Terrace Gravels" and "Valley Gravels;" but he thinks it better to revert, with limitations, to terms which he suggested some years since, but has not hitherto defined.

This structure is then shown to apply to the Waveney, where there is a terrace of gravel on both sides of the valley at a level of about 40 feet above the river, and to which position, but to a more lacustrine condition, the Hoxne deposit belongs. Sections are given of this valley, and also of the valleys of the Lark at Icklingham and of the Ouse at Bedford, showing the constancy of this structure. In the valley of the Thames the phenomena are more complicated and are reserved for future consideration, notice being merely taken of the implements found at Herne Bay and Whitstable.

Owing to the absence of marine newer and post-pliocene beds in the North of France, these gravels are better exhibited and more distinct, being free from rock-fragments and boulders foreign to their own origin and area. Hence it has arisen that this part of the geological series has been more investigated in France than in England. In the admirable review of the Quaternary formations by M. d'Archiac, two general conclusions are set forth. With the first of these the author perfectly agrees. It is that each large hydrographical basin, although the boundaries may not be marked by any important elevation, has its own exclusive *drift*, and that in no case is there a mixture of the transported materials of the separate basins. The author, however, dissents from the opinion that these drifts, containing the remains of large extinct mammalia, have in any way depended on or resulted from any general cataclysm destroying these creatures nearly simultaneously over wide continents and entombing their remains in the sand, gravel, and shingle of the valleys and in the earth of the caverns; neither can he consider the excavation of the valleys to be anterior to the spread of the drift-gravels. On the contrary, he refers the phenomena to long-continued river-action.

An account is then given of the valley of the Somme, and it is stated that the relation between the high- and low-level gravels, which could not be proved with respect to St. Acheul and St. Roch, has been made clearly apparent at Montiers near Amiens, by the opening of a new ballast-pit on the side of the railway, some 50 feet above the level of the old gravel-pits in the valley just below, and in which latter *flint-implements* were first found by the author in the spring of last year. In the upper ballast-pit a considerable number of land and freshwater shells and some mammalian bones have been found,

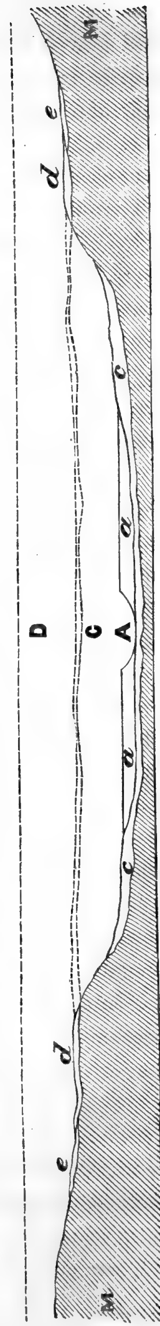
but as yet no flint-implements. This deposit, as also the now well-known flint-implement-bearing beds of St. Acheul, are considered to belong to the high-level gravels, whilst the gravel of St. Roch and that of the old Montiers pits are placed with the low-level gravels. Both sets of gravels are also developed in the neighbourhood of Abbeville, and both there contain flint-implements; Moulin Quignon belonging to the higher level, and Menchecourt and Mautort (village) to the lower level.

In the course of last year M. Gosse discovered flint-implements in association with the remains of the Mammoth in some gravel-pits near the well of Grenelle at Paris. This bed belongs to the low-level gravel. The same gravel is also worked to the S.E. of Paris at the Gare d'Ivry, where, as at Montiers, it abuts against the hill-side. On the hill above, and 115 feet higher, there occurs at Gentilly a deposit of sand and gravel, with land and freshwater shells and mammalian remains, precisely like that at St. Acheul. At Charonne, on the opposite side of the valley and distant 4 miles, a similar deposit, corresponding in its height above the river, in its collection of freshwater shells, and in its mineral contents, is met with. No flint-implements have yet been found in these beds, but in every other respect they agree with the gravel of St. Acheul. These deposits, which have been described by M. Duval and M. Charles d'Orbigny, contain the same débris as the present Seine valley, and amongst it fragments of *granite* derived from the hills of the Morvan, at a distance of 120 miles from Paris.

It is then shown, by reference to the works of M. Leymerie, M. Cornuel and other geologists, that the valleys of the Seine and of its tributaries above Paris are occasionally flanked by heights capped with gravel containing at places remains of the Elephant, Deer, Horse, &c. In some instances these gravels rise to a height of 190 feet above the river, but the general height is from 60 to 150 feet. Sometimes they expand to a breadth of 4 to 5 miles, but they more usually form narrow belts. Various other French authors are then quoted, to show that the same structure prevails in the valleys of the Oise (where one instance of a flint-implement is recorded by M. de Verneuil), of the Marne, the Aire, the Aube, and their numerous tributaries; and in each instance it is shown that the materials, both of the high- and low-level gravels, are derived strictly from the

district through which the valley passes; that only the Seine valley contains granite from the Morvan, the Oise slate-rock débris from the Ardennes, the Marne nothing older than oolitic débris, whilst the Thérain and the Somme valleys contain nothing but débris of the chalk and tertiary strata. The same rule applies to the English area; but the fact is not so apparent, owing to various conflicting elements pointed out by the author who shows, by a map of the two countries, how great are the range and spread of these beds, and how large a proportion of our drift-gravels are of fluviatile origin.

The High-level Gravels.—From the facts recorded by the several independent observers abroad, and from his own observations in this country, the author arrives at a general proposition illustrated by the accompanying diagram, which shows,—1. D, a major valley or plain of denudation anterior to the excavation of the river-valley. 2. *e*, a non-fossiliferous drift on the slope and base of D. 3. C, the river-valley. 4. *d*, the high- and *c*, the low-level gravels. 5. *a*, recent alluvium. 6. A, the present river-channel.



The high-level gravels (*d*) appear on both sides of the valley, and their connexion before the excavation of C is pointed out. This is one of the points insisted upon by the author; the two having been generally considered as contemporaneous, or even sometimes the higher-level deposits as newer than those of the lower level. It is to be observed that the phenomena here referred to relate to broad valleys, and not merely to river-channels.

The *loess* is not shown in the diagram, otherwise the section represents the condition of the case on the supposition that all the parts are complete. But this rarely happens. Some low-

level gravel is constant, but the high-level gravels are only occasionally preserved. Sections are then given to explain the cause of their absence—such as where the valley C being wider than the original bed of the old river which deposited the gravel *d*, the latter has been necessarily altogether removed.

That the formation of the higher gravels can be owing to the action of the present rivers is clearly impossible under existing conditions; for not only are they far above the level reached by the rivers at the highest floods, but also the sectional area of the valleys, compared to that of the present rivers, is so vast, that in no possible way, except by the sea, could they now be filled with water. Sections are given of the valleys of the Waveney, Ouse, Somme, and Seine, showing a disproportion between the rivers at their highest floods and the old valleys, on the average, about 1 : 500; and it is shown, with respect to the great flood of the Seine in 1658, when the waters at Paris rose to a height of 29 feet, that it would require a flood of at least one hundred times that magnitude to fill (with the water even in a state of rest) the valley of the Seine to the level of the high-level gravels of Gentilly and Charonne.

That the isolated beds of high-level gravels must at one time have been connected in length and breadth is evident from the circumstance of these detached parts having certain characters in common, and from the fact that if the deep valleys which they overhang, and the transverse valleys which they pass over, had then existed, they would have presented insuperable barriers to the deposition of the gravels at levels so much higher.

That the transport of this drift could have been caused by the bursting of lakes, by the sudden melting of the glaciers and snow of mountain-chains, or by the transient passage of a body of water over the land is not possible, because the spread of the débris would have been more general, would have held its course more irrespective of the existing watersheds, and would have shown an amount of wear in proportion to the distance travelled; whereas in each basin the débris is local, however low the watershed. None of the slate and oolitic débris of the Oise valley traverses into the valley of the Somme, notwithstanding the watershed between them is only six miles broad and eighty feet high.

There are two ways in which the author conceives the spread of

the débris in the various directions and distinct areas could have been effected; the one by the rise of the land from beneath the sea, and the other by the action of rivers on a larger scale than the present ones. As the later tertiary deposits show the existence of seas or of lakes over the districts in question, it follows as a necessary consequence that when the land rose from beneath them, a mass of débris, in quantity and length of transport proportionate to the greater or lesser rate of elevation, must have been spread over the bottom of the channels along which the water flowed off. Nearly associated with the high-level gravels there are remnants of another drift which may have had this older and independent origin. This mode of formation could not, however, be applied to the valley gravels, as they contain freshwater shells such as live in rivers, with land shells and mammalian remains, proving the existence of a dry land.

The author concludes that the high-level gravels are the result of river-action which took place at a period before the excavation of the present river-valleys. With regard to the mode of formation of these gravels, he remarks on the materials being often transported a considerable distance,—the frequent presence of large blocks or boulders of the harder rocks,—the presence of a certain proportion of angular débris,—and the commonly confused bedding and contortions. He shows this to exist in England and in France, and supports the case by quotations from various French authors. It is then shown that in the valley of the Somme these phenomena are most marked and decisive,—large blocks of sandstone, some weighing four to five tons, and derived from tertiary strata twenty to forty miles above Amiens, being found in the St. Acheul gravels, and the beds being much contorted. These contortions do not depend on any pressure exercised by the blocks, but result from some disturbing power applied and removed. To illustrate this point reference is made to two sections in his former paper (Phil. Trans. for 1860, p. 299).

The author conceives that the only adequate cause to produce many of these effects is river-ice, the transporting power of which is well known, whilst he quotes the observations of travellers in Northern America to prove the power of such ice to pile-up the shore shingle in great conical heaps. That the old pleistocene rivers were also larger and more rapid than the existing rivers is evident from

the great quantity of débris, the prevalence of gravels, the coarseness of the sands, and the general absence of mud-sediments. Another agent of considerable power is referred to, viz. ground-ice, but is reserved for consideration further on.

The Fauna of the High-level Gravels.—The organic remains are considered with reference especially to the climatal conditions of the period, and it is regretted that, owing to the scarcity of fossils except at a few places, and to the want of specific information with regard to the mammalian remains and the levels, the evidence on many points is unavoidably incomplete. The best-determined group is that of the Mollusca, in examining which the valuable assistance of Mr. Gwyn Jeffreys is acknowledged. The author gives a Table showing the group of land and freshwater shells inhabiting, in England and France, the area now described, from which comes out the striking result that out of 109 living species 43 are found in the deposits of the high-level gravel period. There is a scarcity of Unionidæ and Paludinidæ, whereas Limnæidæ and Helicidæ are very common. In many places shells are scarce or altogether wanting; but this is common in all rivers subject to floods or bringing down much shingle. All the species are of existing forms, and all, with four exceptions, inhabit the same districts as formerly. Their range is then reviewed, and it is shown that though a considerable proportion of them are found in the South of France, a still larger proportion exist in Scandinavia, and that as many as thirty-five out of the forty-three species are met with in Finland, including the common forms, such as *Succinea putris, S. Pfeifferi, Helix hispida, H. nemoralis, H. pulchella, Pupa muscorum, Limnæus pereger, L. palustris, L. truncatula, Planorbis corneus, P. vertex, P. marginatus, P. albus, P. spirorbis, Bythinia tentaculata, Valvata piscinalis, Pisidium amnicum,* &c. From these and other facts it is concluded that, while there is nothing in the Mollusca to necessitate a climate different from that of the present day, there is nothing to require restriction to an identical climate, while at the same time the tendency of development of the group is rather in a northern than in a southern direction.

The several genera and species of Mammalia are then considered, the principal being *Elephas primigenius, Rhinoceros tichorhinus, Bison priscus,* with several species of *Equus, Bos, Cervus,* whilst the

Reindeer is found in deposits of the same period; and an opinion is expressed that the evidence with respect to the climatal conditions furnished by the Mammalia, although slight, is more definite than that obtained from the Mollusca, and tends to show the probability of the climate at the period of the high-level gravel having been colder than that of these latitudes at the present day. The flora is scanty and of little avail. It is then remarked that if we had to depend only upon the organic remains for decisive evidence of the nature of the climate of the period under inquiry, we should at present fail to arrive at any safe and exact conclusion. If, however, these indications are taken in conjunction with the physical features, the conjoint evidence has weight and more preciseness; and the author concludes, from a review of all the facts, that there must have been a mean winter cold of not less than 20°, and possibly as low as 10°, or from 19° to 29° below the mean winter temperature (39°) of this part of Europe. The cave evidence would have helped this question.

The Flint-Implements.—These works of man are first discovered in beds of the high-level gravel period. The most ordinary shapes are the large spear-head form, either with a sharp point or a flat rounded one, and with the butt end sometimes blunt, and at other times chipped to an edge. With regard to the manner in which they came to be imbedded in the gravel, it can only be surmised from their condition, from our present experience, and by considering the uses to which they could possibly have been applied.

These conditions are then reviewed, and it is shown that the flint-implements rarely or never show indications of atmospheric weathering; that they are rarely worn, but are usually sharp and angular, like some of the large unworn flints which have been attributed to transport by ice; also that they are most common where the evidence of ice-action is the greatest, as at St. Acheul and Moulin Quignon. The climate of the period having been severe, it is essential to keep in mind the usages of tribes under like conditions at the present day. The mode of life of the Chipweyan Indians and the Esquimaux is then considered; and it is shown that a hatchet, an ice-chisel, a file and knives, of stone or metal, are all the instruments they need or use. It is further shown that when in winter the usually abundant supplies of Reindeer fail, these people resort commonly to fishing in the frozen rivers, and then use their ice-chisels

for making holes in the ice. These implements are also in daily use for keeping open the water-holes. Analogous facts are quoted from Wrangel respecting Siberia. The author suggests therefore that some of the mysterious flint-implements (such as fig. 5, pl. 12, Phil. Trans. 1860) of St. Acheul may have been used as ice-chisels. Reasons are then assigned for their presence chiefly at particular spots; and reference is made to other forms of flint-implements, all of which admit of explanation, except those of a flat ovoid shape, common at Abbeville, which are unlike any instrument in use amongst any existing uncivilized tribes.

Notwithstanding the probable severity of the climate, it was one by no means unsuited to the existence of man, whilst the character of the contemporaneous animal life of the period was perfectly fitted for his support and sustenance.

A difficulty has been raised because hitherto no human bones have been found in these gravels; but when it is considered how scanty is the population in northern latitudes, and how disproportionately numerous are the great herds of Deer, Oxen, and other animals (fossil remains of which are yet comparatively rare), this fact, taken in conjunction with the foresight of man, indicates how small are the chances of finding his remains. Nevertheless in other deposits probably of the age of these gravels, such as some of the caves near Liège described by Schmerling, the scattered bones of man have been found in association with a like mammalian fauna.

The Low-level Gravels.—Connected with this subject is the excavation of the valleys, and the duration of that operation. The author mentions how he hesitated to assign at first a much higher antiquity to the higher gravels than to the lower gravels, or rather, admitting a difference of age, to decide whether the excavation of the valleys might not have been effected by some more powerful agency acting through a short interval of time, and by so much contracting the period by which the St. Acheul deposit preceded that of St. Roch; but after repeated visits to Amiens, and looking at the question from every point of view, he finds himself unable to discover a sufficient explanation in the direction first sought, and obliged to adopt, in part, views differing materially in some points from those he at first thought to be the more probable. The low-level gravels

have been frequently described, and the author confines himself chiefly to pointing out the difference between them and the high-level gravels. The climate at the one period has been described as one of considerable severity; but there is evidence to show that in some part of the pliocene period, previous to that time, the cold was still more severe. At the period referred to the greater part of England was under the sea, whereas Switzerland and the greater part of France had emerged at an earlier or a miocene period, and there is no sufficient proof of their having been subsequently submerged. This was the period of the wonderful extension of the old European glaciers, which descended in the Swiss Alps, the Jura, and the Vosges to within 1200 or 1000 feet of the sea-level, the existing glaciers standing at 3400 to 3500 feet. M. Leblanc has calculated that such a difference of level might be accounted for by a reduction in the mean annual temperature of $12\frac{1}{2}°$ Fahr.; but the author questions this, as the gradients of the glacier beds were much less after they had emerged from the mountain-passes. The growth of the old glaciers is rather the result of the great cold than a measure of it. Still it can be conceived that their growth would be checked when the temperature had risen from the extreme cold to a point $12\frac{1}{2}°$ below the present mean annual temperature. This would reduce the mean annual temperature here to $37\frac{1}{2}°$,—that of Moscow and Quebec, with which the climate at the higher gravel period has been before compared, being respectively 40° and 41°,—and would agree with what has been considered the probable mean winter temperature of that period, viz. one between 10° and 20°.

Taking this as the starting-point, the effect of such conditions with reference to the quantity of ice and snow stored up during this period of cold, and to its effect on the river-discharges for many years afterwards during the period of the valley gravels, has to be considered. The melting of the winter snow would necessarily cause spring floods. Another cause of floods is the fall of rain whilst the ground is still frozen. These causes, combined possibly with a larger rainfall, must have afforded to the old rivers, either permanently or at all events during spring-time, a volume of water far exceeding any present supply, and giving them more of a torrential character. Instances are quoted from Sir R. Murchison's 'Russia' and Wrangel's 'Siberia,' and others, to show how this is still the case every spring in northern

countries, causing a rise in the rivers of from 10 to 40 feet, and inundating the adjacent valleys.

Other forces, however, besides an increase in the water-power, seem required to account for the excavation of the great valleys, and the author thinks that cold and ground-ice have performed a very important part in the operation. In support of this view, he adduces the opinion of Arago and the observations of M. Leclercq and Col. Jackson, both of whom show how constantly this ice is formed in cold climates in rivers with stony and gravelly bottoms, such as the old post-pleiocene rivers must have been. Amongst other observations given are those of M. Weitz, who states that in the north of Siberia the formation of ground-ice can be seen in the rivers at a depth of 14 feet and more, and that in "rising from the bottom, the masses of ice bring up with them sand and stones, and let them down at places far distant from whence they came;" and he concludes, "that not only does the current occasion a change in the bed of the river by its erosion of the looser soil, which it carries from one place to deposit in another, but that the ice, which forms at the bottom of rapid rivers in very cold countries, tends also to effect a change in the beds of those rivers."

Another agent would co-operate with the last; this is the freezing of the ground and the rending of rocks by frost. Taking extreme cases, Crantz shows to how great an extent this operates in Greenland; Dr. Sutherland gives some still more striking instances on the shores of Barrow Strait, and Sir J. Richardson on the Mackenzie River. Even in our country, the disintegration produced during one severe winter on a fresh vertical section of chalk is very striking. A remarkable instance is quoted from Sir R. Murchison's 'Russia,' of a long terrace of angular blocks of limestone broken up and left by the winter-ice 30 feet above the summer level of the Dwina near Archangel.

With all these combined operations, the author still doubts whether, without an uplifting of the land, the effects in question could have been produced; and he shows that the coasts of this part of England and France are fringed here and there by a raised beach, which he correlates with the low-level gravel of Abbeville, whilst the high-level gravel of St. Acheul is correlated with beds occupying on the coast a level higher by 50 to 100 feet, marking the difference of level

between the two periods. The effect of this slow elevation would be to increase the velocity and erosive power of the rivers. This action, with the other agencies before alluded to, operating upon the successive portions of the substrata, has gradually worn even those deep and long valleys, through which so many of the rivers of these districts flow. According to variability in the rate of elevation, to intervals of repose, or to deflections in the current and velocity of the river, there may exist intermediate levels or terraces of gravel, and variations in the inclination of the slopes, which may add much to the complexity of the problem.

The Fauna of the Low-level Gravel.—Of the forty-three species of Mollusca found in the higher gravels, thirty-four occur also in the low levels, together with seven others, making a total of forty-one species. Added to these, there are eight marine species found at Menchecourt, with the *Cyrena fluminalis* of the Nile and of Grays. With this one exception, they are all common living species of England and France. As with the former group, there is nothing to give a definite clue to the character of the climate of the period. The general absence of southern forms, and the preponderance of such as have a wide northern range, may, however, be noticed. With regard to the Mammalia, the number of determined species is small, and the general argument follows nearly the same line as that relating to the Mammalia of the higher gravels. As with the Mollusca, most of the species are common to the two series, whence it is inferred that there was no great or sudden break, and that the change both of conditions and of climate was transitional. There is one genus only, viz. the *Hippopotamus,* about which some difficulty has been felt with reference to the condition of climate. Four tusk teeth of this creature have been found at St. Roch, and in this country its remains are found associated with those of the Reindeer. Without pretending to explain the difficulty, the author does not see why, if the other large Pachyderms were fitted, as they are now known to have been, by warm covering and special adaptation to inhabit cold climates, this extinct species of Hippopotamus should not also have been so adapted.

The physical phenomena point to an increased volume of water in the rivers, and want those marked indications of ice-action seen in the high-level gravels. Still, boulders of considerable size were trans-

ported. From this fact, and the general balance of evidence furnished by the fauna, and also from the contraction of the excavation as the valleys became deeper, the author infers a gradual amelioration in the temperature, ending in the present climatal conditions.

Flint-implements.—The author observes that flint-implements are nowhere so abundant in the low- as they are in the high-level gravels. The pointed lance-shaped form with blunt butts of the latter is almost wanting in the former, whereas the ovoid disks of Menchecourt are rare at St. Acheul; again, flakes or flint-knives are common in the low-level gravels and rare in the higher beds. Of the twenty-four specimens found in the low-level gravel at Paris, twenty-two are mere flint-flakes. The author is disposed to attach some value and significance to this difference of form, and observes, that, admitting the climate to have become less severe during the low-level gravel period, it would follow that the necessity of having the strong ice-chisels would have diminished. In all these cases we are of course much limited to conjectures, seeking to make them in accordance with what we know of life under like conditions, and guided by the probabilities of the concurring circumstances. The mode of distribution of the flint-implements at the two periods certainly seems to afford some grounds for believing that the difference of form may arise from difference in the pursuits and occupations of the primitive tribes by whom they were used—pursuits necessarily and primarily influenced by the climate and life of the period.

Concluding Remarks.—The question of time is then entered upon, and it is shown that the flint-implements must be carried back through the periods of the low- and of the high-level gravels, and that they must be considered to be antecedent to the excavation of many of our great river-valleys. All these phenomena indicate periods of long and great changes. The author only slightly touches upon the formation of the *loess,* which he concludes to be the result of temporary floods; and he remarks that, so far as the question of the antiquity of the fluviatile gravels is concerned, little value need be attached to the additional element presented by this covering of loam and brick-earth. This deposit is succeeded by the alluvial beds of the valleys connected more immediately with our own times. With regard to a measure of time, the author does not consider that either the excavation of the valleys or the life evidence of the periods

furnish available data; nor does he admit the formation of the channel between England and France in the calculation; and he gives reasons to show that this channel is of older date than generally assumed, and that the separation existed at the time of the high-level gravels, and had attained somewhat of its present dimensions at the time of the newer gravels. Most of the land and freshwater shells and the Mammalia had crossed over at a period anterior to this; and, as even now at the Island of Saghaleen in lat. 52° N., the narrow strait freezing during the winter would admit of the passage of large land animals and man during the cold periods following the more extreme glacial conditions.

The author, however, suggests two new modes by which he conceives that eventually some approximate and more exact estimate may be made both of the age of the high-level gravels and of the lapse of time since the extreme glacial period, and embracing therefore the several periods under consideration. At present the evidence is only sufficient to indicate the possibilities of the problem, but it will need many years of careful observation before sufficient data can be obtained for accurate calculation.

1st. With the high-level gravels there are connected a number of sand and gravel pipes perforating the underlying chalk to the depths generally of from 5 to 50 feet, and from 1 to 10 feet wide, or more. As these are caused by the slow action of carbonic acid in the water gradually percolating through the overlying porous beds, dissolving the chalk or other calcareous strata, and gradually letting down the superincumbent drift, it is evident that if the rate of solution and removal can be determined, one element for the calculation of a certain period will be obtained. In this various meteorological questions will have to be considered.

2nd. In conducting observations on the temperature of deep mines, wells, &c., certain discrepancies in the increment of heat at increasing depths and at different places have been noticed. No explanation of these anomalies has been offered. The author suggests that they may arise from disturbing causes originating with a former period of intense cold. At Yakutsk, where the ground is now frozen to a depth of 382 feet, the permanent line of 53° Fahr. would, taking at an average an increase of 1° for every 60 feet, be found at a depth of 1642 feet. If, from some geological change, the mean tempera-

ture of Yakutsk were raised to that of our own climate, this line of 53° would undergo a vertical displacement of 1550 feet. The time required for its uniform re-adjustment over a large area would depend upon various conditions, the chief one being the conductivity of the different strata. The question, therefore, arises, whether traces of perturbation in the temperature of the outer part of the earth's crust in these latitudes, resulting from the action of the extreme cold of the glacial period, may not yet exist, and, if so, whether they may not admit of exact determination with reference to the time elapsed since the removal of the disturbing cause.

In conclusion, the author thinks that in the present state of the inquiry it would be premature to attempt to fix even approximately the lapse of time attaching to the flint-implements. It is obvious, however, that our present chronology with respect to the first appearance of Man must be very greatly extended; but, like a mountain-chain in the distance, its vast magnitude is felt before an exact measurement of its height and size can be taken.

Attention is then directed to the remarkable uninterrupted succession of life from the pleistocene period under review to the present time—a succession so large and important, that it is not possible to imagine the occurrence of any intervening catastrophe of such a nature as to destroy the life of the period over this part of Europe at any recent geological period. There are difficulties in the problem, especially the disappearance of the larger animals; but the remarkable and convincing feature in the case is the transmission to our time of so large a proportion of the small and delicate land and freshwater shells, which even now follow almost precisely the same law in their distribution as they did at these latest geological periods.

Looking at the special nature of the glacial period, and seeing its exceptional character, the author feels strongly impressed with the belief that its effect has possibly been to give increased rigidity and immobility to the flexible crust of the earth, and to produce a state of equilibrium which might otherwise have been of long and slow attainment, whereby it has been rendered fit and suitable for the habitation and pursuits of civilized man*.

* In this and his former paper the author has used the term "pleistocene" in the sense of post-pleiocene, including also some beds placed in the newer pleiocene.

April 3, 1862.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Law of Expansion of Superheated Steam." By WILLIAM FAIRBAIRN, Esq., LL.D., F.R.S., and THOMAS TATE, Esq. Received March 20, 1862.

(Abstract.)

In a former paper selected for the Bakerian Lecture, entitled "Experimental Researches to determine the Density of Steam at different Temperatures, and to ascertain the Law of Expansion of Superheated Steam" (Phil. Trans. 1860, p. 185), it was shown that although Dumas, Gay-Lussac, and other distinguished physicists had determined the density of steam at 212°, it was, however, left for these researches to ascertain the law of density, volume, &c. at all temperatures, and also the law of expansion of superheated steam. These experiments have therefore been continued, and have elicited remarkable results as regards the rate of expansion at various temperatures.

The earliest experiments on this subject were made by Mr. Frost in America, but without sufficient accuracy to be of scientific value. Mr. Siemens has also experimented on steam isolated from water; his results give a much higher rate of expansion for steam than for ordinary gases; but, owing to some obvious defects of Mr. Siemens's method of conducting the experiments, we consider his results are not reliable.

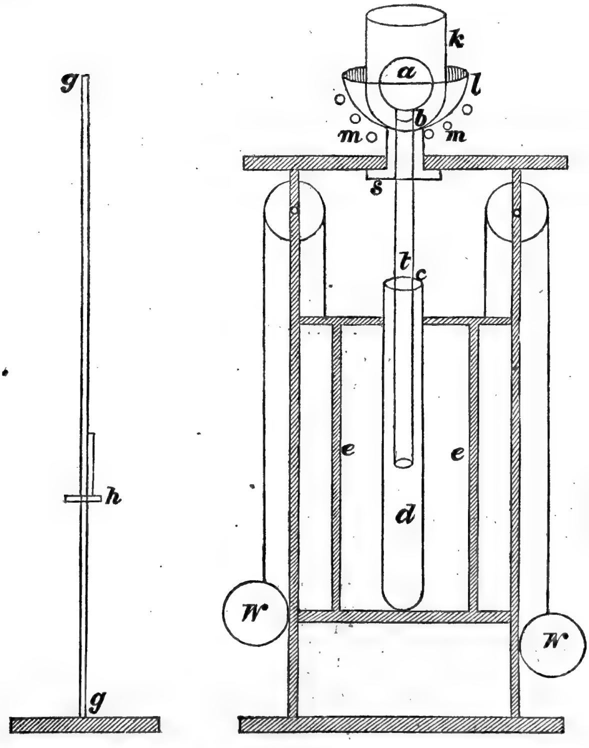
For gases, the rate of expansion is expressed by the formula for constant volume,

$$\frac{P}{P_1}=\frac{E+t}{E+t_1}, \ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

where E is a constant determined by experiment, and decided by Regnault as 459 in the case of air. In the paper alluded to, it was shown that, with a certain proviso, the rate of expansion of superheated steam nearly coincided with that of air. Within a short

distance of the maximum temperature of saturation the rate of expansion of steam was found to be exceedingly variable; near the saturation-point it is higher than that of air, and decreases as the temperature is increased, until it becomes sensibly identical with that of air. The results upon which this law was based were too limited in their range for much numerical accuracy in the constants deduced.

Hence it has been our object in the present paper to supply the deficiency in the previous one, by affording experimental data of the expansion of steam at higher temperatures and with a greater range of superheating than was possible with the apparatus employed in ascertaining the density of steam. The results obtained in these later experiments, however, confirm the general law deduced from the previous ones.



The figure represents the apparatus used when the pressure did

not exceed that of the atmosphere, consisting of a glass globe (*a*) 3 inches in diameter, and stem 35 inches long; the capacity was known to a point (*b*), where a piece of platinum wire was twisted to mark accurately the point at which the mercury column in the stem was to be brought to maintain a constant volume in the globe.

A $1\frac{1}{4}$-inch tube (*d*), filled with mercury, rested upon the frame (*e e*). The weight of the tube and frame was counterbalanced by weights (*w w*). By such an adjustment the tube (*d*) could be regulated with facility, preserving the upper level of the mercury column at one uniform height. A cathetometer (*g g*), with vernier (*h*), to read the lower and variable level of the mercury column, was introduced. To heat the globe, the oil-bath (*k*) was used, fitted to the tube (*t*) by a stuffing-box (*s*); the oil-bath is itself immersed in a mercury bath (*l*), surrounded by a coil of jets of gas (*m m*).

The globe, filled with dry and warm mercury, the air-bubbles being extracted by means of an air-pump, was inverted to form a Torricellian vacuum. A small glass globule of water was then inserted, the platinum wire fixed in its place, and an india-rubber cap fitted to the extremity of the stem. Being transferred to its place, and the india-rubber cap replaced by an open glass cistern, so that the glass (*d*) could be elevated to its position, the jets were lighted, and the temperature elevated to 300°.

From this point the levels of the column were read off at intervals of 50° until the temperature of saturation was reached. The levels were taken in a series of descending temperatures, to avoid the influence of steam boiling out of the mercury as the temperature rose, and to eliminate the effect of the cohesion of the glass on the water, as explained in our previous paper on the density of steam.

Twelve cubic inches of mercury were measured into the globe, and a file-mark made on the stem, below which, at a distance of 14·45 inches, another file-mark was made, affording a fixed point for ascertaining the correspondence of the upper file-mark with the readings on the cathetometer.

Let a be the reading on the fixed rod of the level of the column, b the reading of the lower file-mark on the globe-stem; then $b-a$ =the height of the column of mercury on the globe-stem.

To correct for temperature, $7\frac{1}{2}$ inches of mercury, enclosed by the oil-bath and its stuffing-box, were corrected for the temperature of

the oil, and the remainder of the column for the temperature of the atmosphere at the time. By deducting the column so corrected from the reading of the barometer at the time, the total pressure in the globe is obtained. The readings of the thermometer are corrected for the portion out of the oil-bath. The pressure of mercurial vapour is calculated from data supplied with great courtesy by M. Regnault, and embodying the results of unpublished experiments. The pressure of this vapour is assumed to be the same as that in a vacuum, as the vapour in the globe remains still for a sufficient time (it is believed) for saturation to take place. In this view we have been strengthened by M. Regnault's opinion. By deducting the pressure of mercury vapour from the total pressure in the globe, the pressure of the steam is obtained.

On referring to the experiments contained in the paper, it will be seen that the law of expansion of gaseous bodies is expressed by the formula

$$\frac{E+t}{E+t_1}=\frac{PV}{PV_1}; \qquad \therefore \quad E=\frac{PVt_1-P_1V_1t}{P_1V_1-PV},$$

where E is a constant. Taking Regnault's constant 459 as the rate of expansion of air for constant volumes, a remarkable coincidence will be observed in the experiments contained in the paper when reduced to the same standard of value. The values of E thus deduced have been placed in the last column of the calculated experiments. They show a decreasing rate of expansion from the saturation-point upwards, until at no great increase of temperature the rate of expansion coincides with that of a perfect gas.

Taking from the Tables the two results, which in each instance represent the case of expansion at the greatest distance from the saturation-point, we have the following values of E:—

E = (1)	474·48	(3)	466·85	(5)	460·28
	450·11		451·94		
(2)	455·57	(4)	464·83		
	443·86		460·49		

Mean value of E deduced from these numbers =458·74.

Hence the conclusion which we suggested in our previous paper has been satisfactorily demonstrated by a more extended series of experiments, and the rate of expansion of superheated steam is shown to be almost identical with that of air and other permanent

gases, if calculated at temperatures not too close to the maximum temperature of saturation.

II. "On a New Method of Approximation applicable to Elliptic and Ultra-elliptic Functions."—Second Memoir. By CHARLES W. MERRIFIELD, Esq. Communicated by WILLIAM SPOTTISWOODE, Esq., F.R.S. Received March 20, 1862.

(Abstract.)

Since my first memoir on this subject was read before the Society in May 1860, Mr. Sylvester has published a method, more general than mine, of applying rational approximation to facilitate the computation of the integrals of irrational functions. His process, at which he had arrived independently, included, *à majori*, the one which was the subject of my memoir. Aided by his papers, my subsequent studies have enabled me to view the method with more generality, as well as with more precision and completeness of detail, and I am now able to present it in a sufficiently finished and practical shape for the immediate use of the computer. I append auxiliary Tables to facilitate its use in certain cases.

I begin with the common radical form, starting from Mr. Sylvester's definition of the approximants. Then decomposing the approximant into partial fractions by means of the roots of unity, and increasing indefinitely the number of these fractions, I show that the method is in reality the application of quadratures to a definite integral which is substituted for the surd. The application of the process to integration in like manner rests on the substitution, for the single indefinite integral, of a double integral, definite in respect of one variable, and indefinite for the other. The form of this double integral is such that the indefinite integration can be performed directly; and the application of quadratures to the definite one is facilitated by a peculiar property of functions of the form $\frac{1}{1+n\sin^2\phi}$, namely, that the quadrature does not require the use of differences, but is obtained simply from the mean of the ordinates. Legendre had previously noticed and discussed this peculiarity, which is best illustrated by effecting the quadrature by differential coefficients instead of differences. It will be found that these coefficients (which are all of odd

order) each involve in their numerator the term $\sin\phi . \cos\phi$, which vanishes at both the limits 0 and 90 degrees. It is this feature which gives success to the method.

In a second section I have given with some detail the mode of effecting the actual computation of elliptic functions by this means. I have given several formulæ for using trigonometrical tables with the exactness which these calculations require, and I think they will be found handier for the purpose than those usually given in the books; at all events I find them so myself. Some of them are my own, and some are taken, with more or less modification, from Legendre.

In a third section I have stated what has been done with a view to the extension of the method to radicals of a higher index than the square, and to a certain class of differential equations.

It should be understood that these processes only enable us to find the integral from the amplitude. They do not enable us to find the amplitude, modulus, or parameter from a given value of the integral.

April 10, 1862.

Major-General SABINE, President, in the Chair.

The BAKERIAN LECTURE was delivered by WARREN DE LA RUE, Esq., F.R.S., "On the Total Solar Eclipse of July 18th, 1860, observed at Rivabellosa, near Miranda de Ebro, in Spain."

The Lecturer gave an account of the more interesting phenomena of the eclipse, and of the methods employed in observing and recording them; the details of his observations being given in an elaborate Paper bearing the above title. The Lecture was illustrated by a great number of diagrams and models. The photographic images of the eclipse were projected on a screen by means of the electric lamp, and some of the more striking phenomena were imitated by apparatus contrived for that purpose.

The following is an abstract of the Paper:—

The author, for some time previous to the organization of the Astronomer Royal's expedition to Spain, had contemplated making an attempt to photograph the phenomena of the total eclipse of

July 18th, 1860, but as soon as he was informed of the Astronomer Royal's views he agreed to join his party, now known as the Himalaya Expedition, from the name of Her Majesty's ship which conveyed the astronomers composing it to Spain. He attributes much of the success of his operations to the admirable arrangements of Professor Airy in England, and to those concerted with Mr. Vignoles in Spain; for he was able in consequence greatly to increase the extent of his preparations, and to convey a complete temporary observatory fitted up with all the numerous requirements which are essential in astronomical photography. Besides himself, his party consisted of Mr. Beckley of the Kew Observatory, Mr. Reynolds (now Mr. De la Rue's private assistant), Mr. Downes, and Mr. E. Beck, and subsequently the late Mr. Clark. The author expresses himself greatly indebted to these gentlemen for their most efficient assistance.

The party took up their station at a village called Rivabellosa, situated near the town of Miranda de Ebro; the site selected was a thrashing-floor, on which the observatory was erected.

The instruments employed consisted of the Kew heliograph, for the photographic records; an achromatic telescope, by Dallmeyer, mounted on a sort of alt-azimuth stand contrived by the Astronomer Royal, which permitted of an equatorial movement by the ingeniously arranged joint action of two racked radius bars. To this telescope the author fitted a diagonal eyepiece of his own contrivance, which allowed of the use of reflexion from plain glass in the first instance, and then from a portion silvered on the top surface the instant the period of totality commenced. By its means he avoided the perplexity and loss of time occasioned in unscrewing and screwing portions of the apparatus at the most critical period. To these were added a small transit theodolite, three chronometers, two barometers, and several thermometers.

The weather proved so unpropitious that it was with much difficulty the objects of the party could be carried out; and it was only by using every available opportunity that even the Kew instrument could be placed in position.

The geographical position of the site of the observatory was ascertained to be—north latitude $42^\circ\ 42'$, west longitude $11'\ 42''\cdot7$, elevation above the mean high-water mark 1572·4 feet.

The author made two sketches of the luminous prominences during the period of totality, on paper previously ruled to represent the position-lines drawn on a piece of parallel glass placed in the focus of the eyepiece, which magnified about 60 times. These position-lines consisted of a square calculated to exactly include the lunar disk, and two external squares, one exactly one minute of arc distant from the central square and from the other. The angles of the squares were joined by diagonal fainter lines. The whole system was moveable through an arc of 90°, and its position could be read off on a graduated external circle divided from 10 to 10 degrees. The drawings were by chance made of nearly the exact diameter of the lunar disk in the photographs (4 inches), and proved very valuable in interpreting the phenomena revealed by the latter, as the one could be compared by superposition with the other, and the several prominences be thus identified.

One of the prominences, situated about 30° from the north point towards the east, became visible several minutes before totality, even during the employment of the unsilvered portion of the diagonal reflector. As the sun disappeared the author watched for the so-called Baily's beads, but no such phenomenon occurred, which occasioned no surprise to him, as he had always believed that it arose in all probability from the atmospheric disturbance of an image formed by a telescope wanting in definition.

The author goes on to describe the various appearances presented by the several protuberances, which were not all of a rose-colour, and those which presented this hue were much paler in colour than his previous reading had led him to expect. He is able to speak with considerable certainty on this point, having before the eclipse painted several colours on his drawing-paper, and was thus enabled to compare these directly with the prominences by means of the light emitted by the corona, it being sufficiently great and polychromatic for that purpose. The light of a lamp which was at hand proved not only useless, but was detrimental in making the comparisons. There was a considerable amount of detail, both of form and colour, in the prominences, which the author has shown in two coloured drawings which accompany the paper; these are founded on the original sketches, which are also given in fac-simile, but to some extent corrected by means of the photographs.

That the prominences belong to the sun and not to the moon was rendered evident to the observer by the progressive covering of the luminous prominences on the east in the direction of the moon's motion, and the gradual uncovering of fresh prominences on the west; while prominences situated in a position nearly at right angles to the moon's path shifted their angular position on the moon's edge several degrees during the observations. The prominence which became visible before totality, which the author designates by A, was found to have shifted 3° 25′ on the moon's limb in an interval of about $2\frac{1}{2}$ minutes; it was therefore evident that the region of the moon which at the commencement of the period was in apparent contact with the prominence was at some distance from it at the end; and as the prominence underwent no change during that time, the theory falls to the ground which ascribes the phenomenon of the luminous protuberances to some peculiar action of the moon's edge on light coming originally from the sun.

The author describes the general effect of the eclipse to the unassisted eye. He was particularly struck with the peculiar illumination of the surrounding landscape as the sun became reduced to a small crescent; the shadows of all objects were so sharp and the light so brilliant that it reminded him of the illumination produced by the electric light; at the same time peculiar hues were assumed by the sky and landscape, which suggested the idea that the light of the sun near the periphery is not only less intense than that of the centre, but that it may be different in quality.

No attempt was made to obtain accurate observations of the corona, but nevertheless a few seconds were devoted to this phenomenon. Even several minutes before totality the whole contour of the moon could be distinctly seen; when totality had commenced, the moon's disk appeared of a deep brown in the centre of the corona, which was extremely bright near the moon's limb and appeared of a silvery white, softening off with a very irregular outline and sending forth some long streams. It extended generally to about from 0·7 to 0·8 of the moon's diameter beyond her periphery.

The darkness during the totality was not nearly so great as might have been expected from accounts of previous total eclipses. The illumination was markedly distinct from that which occurs in nature on any other occasion, and certainly was greater than on the brightest

moonlit night, although at the time the light appeared to the author as less bright than what he remembered of bright moonlight. By subsequent trials he was led to conclude that the light during a total eclipse most resembles that degree of illumination which exists in a clear sky soon after sunset, when, after having made out a first-magnitude star, other stars of less brilliancy can be discerned one after another by an attentive gazer. Jupiter and Venus were the only objects the author had time to identify, but some neighbouring observers saw also Castor.

The most important part of the paper treats of the photographic observations. The several preparations are minutely described, and drawings, showing the general arrangements of the observatory, are given. In the focus of the secondary magnifier of the Kew heliograph, two position-wires, crossing at right angles, are fixed at approximately an angle of 45° to a parallel of declination. The object-glass has an aperture of 3·4 inches and a focal length of 50 inches: the primary focal image of the sun at his mean distance is 0·47 inch; but before it is allowed to fall on the sensitive plate, it is enlarged to about 3·8 inches by means of an ordinary Huyghenian eyepiece. The object-glass is so constructed as to ensure the coincidence of the chemical and visual foci; this coincidence is, however, disturbed in a slight degree by the Huyghenian magnifier, which renders a slight adjustment necessary. For ordinary sun-pictures, and those of the several phases of the eclipse except the totality, the aperture was reduced to 2 inches,—a peculiar instantaneous apparatus being employed to regulate the exposure of the sensitive plate.

The driving-clock of the heliograph was, for convenience, kept going during the taking of the partial phases of the eclipse; but it was not really necessary to keep it in motion, because the time of exposure certainly did not exceed the $\frac{1}{50}$th of a second.

The position-wires, by stopping off the sun's light, are depicted in the negatives as white lines crossing the solar disk. It was essential, in order to turn these several pictures to account, to note exactly the time of their being taken, which was done by Mr. Beckley; the clicking noise made by the instantaneous apparatus, when it struck against a stop after releasement, indicating the epoch, which was noted to the nearest half-second. The exact position of the cross

wires was also ascertained by observations of the sun made on each side of the meridian; this was necessary, because, in consequence of the weather, the pole of the heliograph could be only approximately adjusted in position.

Upwards of fifty plates were placed in the heliograph between $11^h\ 28^m$ A.M. and $4^h\ 16^m$ P.M. on July 18th; some before the commencement of the eclipse, and some after. During totality two photographs were obtained. One picture was produced on a plate which was exposed from the exact commencement of totality during the minute succeeding this epoch; the second picture was exposed from about a minute previous to the reappearance of the sun until not more than a second before he became visible. In these pictures the several prominences are depicted with great clearness; and when one negative is superposed on the other, corresponding parts exactly coincide. During the taking of the second photograph, an excusable curiosity on the part of two of the assistants disturbed the telescope twice, so that the prominences have depicted themselves three times; but there was no difficulty in stopping out the images not belonging to either of the three phases thus recorded. The author has moreover turned this accident to account, and estimated the relative brightness of the prominences in comparison with the sun's photosphere; and he considers that they are at least 600 times less brilliant than it. This conclusion has been drawn from the minimum time required by the prominences to depict themselves, which can be made out from the photograph in question.

By means of a new micrometer contrived for that purpose by the author, the several photographs have been measured and discussed. The position-angles of the line joining the sun's centre and the moon's centre, and the distances of these centres for the several epochs of the photographs, have been calculated and compared with the corresponding values calculated by Mr. Farley for the geographical position of the observatory. Other calculations have also been made from the photographs and compared with certain elements of the eclipse calculated by Mr. Carrington. The results show that the photographic method of observing solar phenomena is capable of great exactness.

The nearest approach of the centres of the sun and moon, as ascer-

tained from the photographic measurements, was $11''\cdot8$, calculation giving as a mean $12''\cdot8$. The relative diameter of the moon, that of the sun being taken as unity, as derived from measurements of the photographs, comes out 1·0511, which is precisely the theoretical number; on the other hand, they tend to show that the diameters at present assumed for the sun and moon, taken conjointly, are about $4''\cdot0$ in excess of the truth.

The paper is accompanied by an extensive series of calculations, which it is not here necessary to describe. Those, however, relating to the measurements of the positions of the luminous prominences on the two totality-pictures have especial interest. These measurements were made in two ways: 1st, the original negatives were measured by the author's new micrometer; 2nd, enlarged positive copies were taken on glass, and the contours of the prominences traced and etched upon the glass, which was afterwards centered on a dividing engine and divided, the divisions being subsequently etched. Copper duplicates were then made of the glass plates, which served to print off diagrams which accompany the paper.

Without describing minutely the measurements, it will suffice here to state that the results go to prove that the luminous prominences must belong to the sun and not to the moon. For example, the change in the angular position of the prominence at a right angle to the moon's path, and designated A in the paper, has been calculated to have been $5^\circ\ 21'$ for the assumed geographical position of the station; by measurement of the two photographs it is $5^\circ\ 32'$. The motion of the moon in covering and uncovering a prominence in the line of her path was calculated to have been $92''\cdot8$; by measurement it was found to have been $93''\cdot7$. The accordance of these numbers is so extremely close, that it would be difficult to obtain more convincing proofs that the luminous prominences belong to the sun.

The Society adjourned over the Easter Holidays to Thursday, May 1st.

May 1, 1862.

JAMES PAGET, Esq., Vice-President, in the Chair.

In accordance with the Statutes, the names of the Candidates recommended for election into the Society were read, as follows:—

George Bentham, Esq.
Henry William Bristow, Esq.
Alexander Ross Clarke, Capt. R.E.
John W. Dawson, Esq.
Frederick J. Owen Evans, Esq.
John Braxton Hicks, M.D.
The Very Rev. Walter Farquhar Hook, D.D.
George Rolleston, M.D.
Charles William Siemens, Esq.
Maxwell Simpson, Esq.
Balfour Stewart, Esq.
Thomas Pridgin Teale, Esq.
Sir James Emerson Tennent.
Isaac Todhunter, Esq.
C. Greville Williams, Esq.

Professor Albert Kölliker, of Würzburg, who was elected a Foreign Member in 1860, was admitted into the Society.

The Croonian Lecture was delivered by Prof. A. Kölliker, For. Memb. R.S., "On the Termination of Nerves in Muscles, as observed in the Frog; and on the disposition of the Nerves in the Frog's Heart," as follows:—

When I was honoured by an invitation to deliver the Croonian Lecture, I at first hesitated to undertake the task, however gratifying to me, because I was not prepared with a subject of discourse which I thought likely to prove of sufficient general interest to the Fellows of this distinguished body, engaged as they are in the pursuit of very various branches of "Natural Knowledge." I felt that on such an occasion it was desirable to lay before the Society the result of some original research, but I feared that the matter I had actually at my command, referring more immediately to a question in Microscopic Anatomy, was scarcely of adequate importance. Knowing, however, that the purpose for which this lecture was instituted is the elucidation of the "Nature and Laws of Muscular Motion," and considering that my researches, although in themselves purely anatomical, have a

decided bearing on that great physiological question, I have felt encouraged to lay them before the Society.

Termination of the Nerves in the Voluntary Muscles.

The investigation of the termination of the nerves in the muscles has for some time occupied the attention of various able inquirers, but the results attained are by no means in mutual accordance. The recent researches of Kühne, in particular, on the muscular nerves of the Frog, have led him to conclusions differing so widely from those of Wagner, Reichert, Schaafhausen, Beale and others, that I was induced to apply myself to the question, in the hope that I might be able to contribute something towards its elucidation*.

While previous observers have been unable to trace the muscular nerves further than the surface of the muscular fibres, Kühne †

* As the more immediate purpose of my present inquiry was to examine into the accuracy of Kühne's recent descriptions, I did not deem it necessary in the Lecture to do more than advert to the labours of previous inquirers. In order, however, to prevent misconception, I think it as well to explain that whilst pale nucleiferous branching fibres have been seen and repeatedly described as the terminal ramifications of sentient nerves, some observers have also described the nerves as ending on muscular fibres by similar pale filaments. Thus Axmann, in 1853, represented the nerves of the cutaneous muscle of the mole as ending in networks of fine fibres; and more recently, Schaafhausen (1859) described the terminations of the nerves in muscles as a very fine network which could be rendered visible by means of carmine. In justice to Dr. Beale also, I must here state that, in the account he has given of his elaborate researches on this difficult point of microscopic anatomy (Phil. Trans. 1860), he describes the nerves of striped muscles as terminating on the sarcolemma in a network of pale fibres connected with nuclei, rendered visible by carmine and chemical reagents. Speaking of these fibres, Dr. Beale observes (p. 616) that their "general appearance and refracting power are the same in every part except where the nuclei are situated"; he adds (p. 617), that "the axis-cylinder gradually loses its hard fibrous character (frog), and the white substance its peculiar refractive power and consistence. The whole fibre, as seen in my specimens, seems to consist of a very transparent and perhaps delicately granular substance, which can be shown to be composed of fatty and albuminous materials," &c. Judging, however, from Dr. Beale's figures, which represent the terminal network of the nerves distributed on the muscular fibres of the mouse, and from his description, I am led to conceive that he has not observed what Kühne and I have seen. He does not mention that he has seen *single* dark-bordered nerve-fibres running out into *simple* and *fine*-branched pale fibres, and, moreover, gives no figures showing such ramifications. It is true Dr. Beale says that the same pale nerve-fibres he has described and figured from the mouse, occur also in the frog; but from what I have seen I cannot admit that pale fibres, of the size and with the arrangement represented in his figures from the mouse, are to be met with in the frog.—A. K.

† Ueber die peripherische Endorgane der motorischen Nerven. Leipz. 1862.

believes he has discovered that the nerve-fibres, on reaching a muscular fibre, penetrate into its interior and end amidst the muscular substance by several pale branches. He states that the nerve-fibre, on reaching the muscular fibre, divides into several branches which retain their characteristic dark contours until they enter the muscular fibre, within which they become pale and faintly outlined. He conceives that the tubular membranous sheath of the nerve-fibres coalesces with the sarcolemma, and that the branches when they enter the muscular fibre lay aside not only their membranous sheath but their white substance, to which they owe their dark outline, and are in fact reduced to mere ramifications of the axis-cylinder or central thread of the original fibre.

In connexion with these internal fibres, Kühne further describes certain bodies which he proposes to name "terminal nerve-buds" (Nervenendknospen). These are attached laterally to the pale fibres, which then may end at some distance beyond in free pointed extremities, while some of the pale fibres appear actually to terminate in these end-buds. The bodies in question are stated to be oval-shaped corpuscles, smaller than the well-known muscular nuclei, usually pointed at their distal extremity, where they bear a minute filamentous tuft. According to Kühne each consists of a little oval mass of finely granular substance, into which a fine filament, apparently derived from the axis-cylinder of the pale nerve-fibre, enters, like a pedicle, at one end, and runs along the middle as a sort of axis-cylinder of the corpuscle, at the free end of which it swells out into a small interior pyriform body containing minute spherules very different in aspect from the granules of the surrounding granular substance. The structure thus described presents, as Kühne observes, some resemblance to that of a Pacinian Body, but yet with marked differences, and he does not lay any great stress on the point.

These observations of Kühne were made with a magnifying power of from 1000 to 1800 linear, on fresh muscular fibres (from the gastrocnemius of the frog) immersed in vitreous humour or blood-serum, also on fibres prepared by macerating a portion of muscle for 24 hours in extremely dilute sulphuric acid, then digesting for an equal time in distilled water at about 100° F., and finally shaking it briskly with a little water in a test-tube so as to separate the tissue into single fibres.

Believing that the wide divergence in the conclusions heretofore arrived at has been owing in great measure to difference in the methods of investigation employed, I made many trials in order to find out reagents which would increase the transparency of the muscular fibres without attacking or at least obscuring the finer fibres of the nerves, and have found the following to be well adapted for the purpose in view. 1. Extremely dilute acetic acid*. 2. Diluted hydrochloric acid in the proportion of 1 part of acid to 1000 of water; but as this reagent eventually softens and destroys the muscles, it is well to observe that the suitable time of exposure to it is from about 12 to 24 hours. 3. Artificial digestive fluid, the use of which for a similar reason requires precaution. 4. Very dilute nitric acid in the proportion of 1 part of acid to 1000 of water.

The object selected for examination was for the most part the cutaneous pectoral muscle of the frog, in which the ramification of the nerves has been so successfully traced and represented by Reichert†. In general I have found it better not to separate the muscular fibres, although I have not omitted also to examine single fibres from the same muscle, and from the gastrocnemius. The magnifying power employed was from 500 to 600 linear, obtained by Hartnach's lenses "à immersion" No. 9 & 10. I have also tried a power of from 1000 to 1500 as used by Kühne, but I could discover nothing by its aid which I did not see equally well with the lower amplification.

In proceeding now to give an account of my own observations, I have first to state that I have been able to confirm the observation of Kühne that in the frog's muscles the nerve-fibres really branch out at their ends into delicate pale filaments—a fact not hitherto recognized by Wagner, Reichert and others, who have investigated the relation of the nerves to the muscles in that animal.

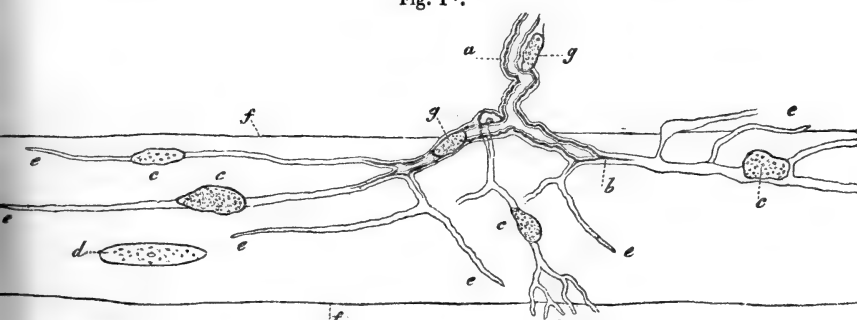
But whilst agreeing with Kühne as to the existence of these pale fibres, I am satisfied that they are situated, not within the muscular fibre but on its surface, as I shall more fully explain hereafter; and that they lie outside the sarcolemma, through which they do not penetrate. As to their nature, Kühne regards them as prolongations

* I find the best proportion to be from 8 to 16 drops of concentrated acetic acid, sp. gr. 1·045, to 100 cubic centimetres ($3\frac{1}{2}$ ounces) of distilled water.

† Müller's Archiv, 1851.

of the axis-cylinder alone; but it decidedly appears to me that, besides the axis-cylinder, they are furnished with a prolongation of the membranous sheath; indeed I have seen this so clearly in a great many favourable instances, that I can have no doubt on the point. According to my view, therefore, the delicate membranous sheath does not quit the nerve-fibre and coalesce with the sarcolemma, as Kühne believes, but continues to surround a pale prolongation of the soft contents of the nerve-tube. As to the matter contained in the membranous sheath, it no doubt always comprehends the axis-cylinder, and is chiefly formed by a prolongation of that structure; but I have seen examples in which the contained matter showed slight varicosities and a certain darkness of outline, from which I infer that here and there at least a thin layer of the white substance extends along the pale fibre. But whilst it is easy in most cases to

Fig. 1*.



perceive the membranous sheath and its enclosed matter distinct from each other at the commencement of the pale fibres, yet in their further progress these structures coalesce together, and the terminal fibres then appear as uniform pale filaments. They are still, however, to be regarded as tubes; for, in the first place, they are prolongations of a decidedly tubular fibre, and, secondly, when treated with

* Fig. 1. Termination of a dark-bordered tubular nerve-fibre on a muscular fibre, *f*,*f*, of the cutaneous muscle of the frog, seen with Hartnach's Objective "à immersion" No. 10, and Eyepiece No. 1. *a*, Sheath of the nerve-fibre continued at *b* upon the pale terminal fibre; *b*, axis-cylinder and contents of the nerve-tube continued into the pale fibres; *c*, *c*, nuclei of the pale fibres; *d*, a nucleus belonging to the muscular fibre *f*,*f*; *e*, *e*, *e*, *e*, ends of the pale fibres; *g*, *g*, nuclei of the dark-bordered fibres. [The figure shows the continuation of the dark-bordered fibres into the pale filaments, but fails to exhibit the faint outline and characteristic aspect of the latter.]

certain reagents, such as acetic acid, they exhibit a precipitate of fine sparse granules in their interior, as Kühne has also observed; although when examined in the fresh state, or after being treated with hydrochloric acid, or artificial digestive fluid, they appear pellucid and uniform throughout. It is further worthy of remark, that whilst the pale fibres are for the most part rectilinear prolongations of the dark-contoured fibres, instances occur in which a dark fibre ends by dividing at once into two or three pale fibres. More remarkable still are the cases in which a pale fibre comes off laterally, and sometimes at right angles from a dark fibre, or where two pale fibres come off together from opposite sides; because such cases, which Kühne appears not to have seen, or at least has not represented, are well calculated to show that the pale fibres are furnished with a prolongation of the membranous sheath.

The next question of interest arising from Kühne's observation refers to the bodies which he looks upon as peculiar terminal organs, and names the terminal nerve-buds. As regards these corpuscles, I must confess that, after the most careful investigation, I have failed to discover that they possess the peculiar internal structure which Kühne assigns to them. Considering the skill and address in microscopic investigation which Kühne has evinced in his inquiry, and feeling persuaded that he must have met with what seemed to him sufficient indications of the structure he has described, I bestowed the greatest pains on the examination of these so-called end-buds. I studied them in muscles and muscular fibres, both fresh and treated with reagents. I employed the same magnifying powers, and equally good lenses; but the only conclusion to which I could arrive is, that the corpuscles in question are nothing but ordinary nuclei connected with the membranous sheath of the pale nerve-fibres, not essentially differing from those found attached to the sheath of the dark-contoured fibres from which the pale fibres are derived. It is true that some of them show a dark streak in the middle or towards the border, which at first sight might suggest a peculiarity of structure; but the appearance is obviously due to a fold or crease on the surface, and the same thing is seen in the undoubted nuclei of the dark-contoured fibres. Their shape, position, and relative size may be judged of from the figures I have given; and I have only further to remark, that in fresh preparations they are usually very

faint, and therefore somewhat difficult to recognize; but with certain reagents, such as acetic acid, they become darker, more granular, and somewhat shrunken, with an irregular outline; with others, such as hydrochloric acid, they appear more homogeneous and pale. As to their situation, they are attached along the fibres; and I have never seen them at the ends, as Kühne in some instances found, although sometimes they are placed very near the end. Sometimes one is placed in the angle of division of a fibre. They appear to be attached laterally; yet I cannot doubt that they lie within the membranous sheath of the pale fibre, although the prolongation of the latter over them cannot be shown as a separate structure. Were further evidence required concerning the true nature of these corpuscles, I might add that nuclei of precisely the same character exist on the pale terminal fibres of the sensory nerves distributed to muscles, as I shall more fully explain in the sequel, and indeed on the final ramifications of nerves in general, of which we have examples in the skin of the mouse and frog, in mucous membranes, in the cornea, and in the electric organ of the torpedo.

I come now to a more difficult question, namely, whether the pale end-fibres of the nerves really lie within the muscular fibres or not. Important as the determination of this question is in relation to physiology, I can confidently say that I entered upon it without bias, and studiously put aside every consideration which might militate against the notion that the nerves penetrate into the muscular fibres. Indeed I myself at one time thought that in certain parts of the cutaneous pectoral muscle of the frog I had seen muscular fibres penetrated by nerve-fibres. On careful investigation, however, I became convinced that the seeming interior situation of the pale fibres is an illusive appearance, and so many proofs against its reality presented themselves, that I was finally constrained to come to a different conclusion from Kühne on this point also.

According to my view, therefore, the pale terminal ramifications of the nerves lie wholly without the muscular fibres, that is, on the outer surface of the sarcolemma. In support of this opinion I may, without insisting on negative evidence, which may be less regarded, adduce, in the first place, the fact that not unfrequently a pale fibre may be seen running on a muscular fibre towards the border, and then turning round to the other side, obviously outside the sarcolemma. Again

a pale fibre running along the border of a muscular fibre sometimes presents serpentine bendings, which appear alternately under and over the muscular fibre, and in the latter case are clearly exterior to the sarcolemma. Moreover I have observed that the pale fibres derived from one dark fibre are sometimes distributed to two muscular fibres, and must therefore run outside and between them,—an appearance not reconcileable with the descriptions of Kühne, who states that nerve-fibres retain their dark contours until they penetrate the muscular fibre, and that the whole of the terminal pale filaments derived from a nerve-fibre are contained within one muscular fibre.

In further corroboration of what has been said I may add, that in surface-views of the parts in question the pale nerves always appear above the cross striæ of the muscular fibre, also above its nuclei; which fact would, it is true, not decide whether the pale fibres were outside or inside the sarcolemma, but is sufficient to prove that they do not lie amidst the contractile substance. Further, this contractile substance within the sarcolemma may, by means of dilute hydrochloric acid, be softened and reduced to a fluid state, and nevertheless the pale fibres retain their original position unaltered, even when the liquefied contents of the sarcolemma with the muscular nuclei flow backwards and forwards within the tube. Lastly, when muscular fibres are treated with acetic acid of a certain strength, the whole contents of the sarcolemma are squeezed out in the form of long transversely striated cylinders; so that on cutting across the fibres of a muscle near the part where it receives a nerve, and treating it in the way indicated, the proper substance of the fibres may be examined apart from the sarcolemma, at the place where their nerves reach them. Now, on repeatedly trying this experiment, I have never found a trace of the nerve-fibres on the extruded portions, whilst on the other hand they are still to be seen on the emptied sarcolemma.

It remains for me yet to say a word as to the mode in which the pale nerve-fibres actually terminate, a point on which I confess I have still some doubt. It is true that I have observed apparently free ends as represented by Kühne; but, on the other hand, appearances sometimes present themselves which suggest the question, whether on the muscular fibre, as in the physiologically allied electric organ of the torpedo, there may not be *an extremely fine network*

in which the pale nerve-fibres terminate. Thus in many cases there is an appearance on the pale fibres of numerous short, pointed or blunt, lateral processes, or at least a certain want of definition in their outline, indicating that they may possibly send out still finer offsets. At the same time I have not been able to trace the matter further in this direction; and as regards the few undoubted cases of conjunction of the pale fibres which I have hitherto met with, I am not disposed to interpret them in the above sense. Moreover we can often see the pale fibres so sharply and beautifully defined, and maintaining so long their rectilinear course, that it is difficult not to regard them as the true terminations; so that the above-mentioned appearances to the contrary may perhaps be owing to the effect of the reagents used, which, while they clear up the muscular fibres, may attack more or less the extremely delicate substance of the pale nerve-fibres.

On the Distribution of Sentient Nerves in the Muscles of the Frog.

Having thus laid before the Society the results of my observations on the motorial nerves of the frog's muscles, I have now to speak of the terminations of other nerves which are distributed to the muscles of the same animal, and which are probably sensory in their office. Nerves which are probably of a sentient nature have already been observed in human muscles by myself, and in the cutaneous pectoral muscle of the frog by Reichert, who takes the same view as to their nature. As, however, the mode of termination of these nerves has not heretofore been fully investigated, I have been led to extend my inquiries to that question, and beg leave now to state the principal results.

The sentient nerves of the cutaneous pectoral muscle of the frog are supplied by the common moto-sensory nerve of the muscle (fig. 2, *a a a*), from which they come off at different points as single fibres, and, proceeding to the muscle, branch out upon it in its whole extent, even in parts which are destitute of motorial nerves. As regards the details of distribution, however, there is not even an approach to uniformity in any two muscles, and therefore, instead of attempting a general description, I will refer to fig. 2, which represents an individual case as seen under a low magnifying power, carefully copied from nature.

In this instance the sentient nerves consisted of five principal

Fig. 2 *.

* Fig. 2. *a, a, a,* Common trunk and principal branches of moto-sensory nerve.

truncules as they may be called, each, however, being but a single fine fibre; one of these (fig. 2 *b*), larger than the rest, supplied the upper part of the muscle with branches, two (*c*, *d*) went to the middle, and two rather long ones (*e*, *f*) were distributed on the lower part of the muscle. As to the original source of these truncules, I agree with Reichert in thinking that they are all derived from one principal sentient fibre, which is mixed up with the more numerous motor fibres in the small nerve *a*, *a*, *a*, which is supplied to the cutaneous muscle. It is true that I have not been able, any more than Reichert, actually to trace back these truncules to their parent fibre in the trunk of the common nerve. Nevertheless I feel much confidence in believing that they arise in the way stated; and especially because I have met with cases in which the parent fibre of all the sentient nerve-fibres of the cutaneous muscle escapes from among the motorial fibres of the common muscular nerve at some distance from the muscle; so that its division into the secondary truncules may be seen. Moreover I have never seen fibres, which from their final distribution may be reckoned as motory, coming from the sentient fibres, nor the latter from the former; although I have sometimes met with a deceptive appearance to the contrary, caused by true motor fibres attaching themselves for a little way to a sentient fibre, and then seeming to come off as branches of it.

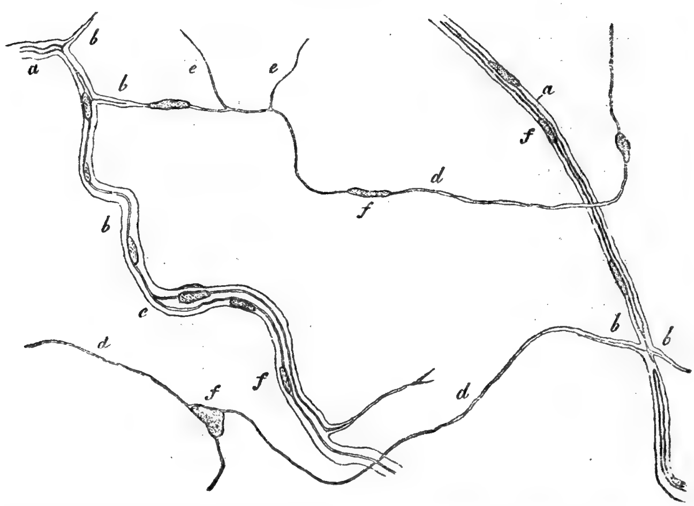
In their progress the dark-bordered sentient fibres for the most part tend towards the outer or cutaneous surface of the thin muscle, on reaching which they branch out upon it underneath a thin fascia, which covers the muscle and forms also the wall of an adjacent lymph-space.

The terminations of these sentient nerve-fibres were not seen by Reichert, and doubtless because he used potash in his examination of them; but with the aid of some of the reagents already mentioned they may be traced out, although from their extreme tenuity and paleness this is no easy matter. Their mode of termination is on the whole similar to that of the motor nerves, only the pale end-fibres have a more extended distribution and are much finer. The

of the cutaneous muscle; *b*, *c*, *d*, sentient fibres (truncules) to upper and middle part of muscle, and *e*, *f*, to lower part, all dividing into finer terminal filaments connected with nuclei which are not represented in this general view; *h*, *h*, *h*, *h*, branches of *f* which went to abdominal muscles; *i*, *i*, *i*, swollen parts of muscular fibres containing 'nerve-tufts.'

figures will serve better than a long description to give a correct idea of their terminations. Whilst fig. 2, drawn under a low magnifying power, represents their general mode of distribution, fig. 3 shows a small part of their ramification and two or three terminations, highly magnified. It will be seen that the larger fibres divide and redivide into smaller branches, which still retain their dark contours, but give off in their course lateral pale branches, and finally end in pale and numerous prolongations. In the pale fibres, at their commencement the membranous sheath is distinct from the contained fibre, which doubtless consists of the axis-cylinder, with at first some part of the medullary substance; in their progress the sheath becomes blended with the contents, and can no longer be distinguished as a separate structure. Nuclei (*f*, *f*, fig. 3) are seen

Fig. 3*.



all along the pale fibres, and also on the smaller dark fibres. In short, the structural character of the pale sentient fibres is, so far as it can be discovered, essentially the same as the pale terminations of

* Fig. 3. Part of ramifications of the sentient fibres of the cutaneous muscle, as seen with Hartnach's Objective No. 7, and Eyepiece No. 1. *a*, *a*, Dark-bordered fibres with fine sheath and nuclei; *b*, *b*, *b*, pale fibres still having a sheath and pale contents (axis-cylinder); *c*, division of the axis-cylinder of one of these fibres; *d*, *d*, *d*, non-medullated terminal fibres, with nuclei *f*, but on which no sheath can be recognized.

the motor nerves. As already noticed, however, they become smaller in size; indeed the extreme prolongations are in some places attenuated to the size of filaments of connective tissue. Their fine terminations, moreover, lie quite superficially in the perimysium or sheath of the muscle, and between it and the muscular fibres, and are in a great measure confined to the outer or cutaneous surface of the muscle, although here and there fibres are seen turning to the under surface.

Nerves of the Blood-Vessels.

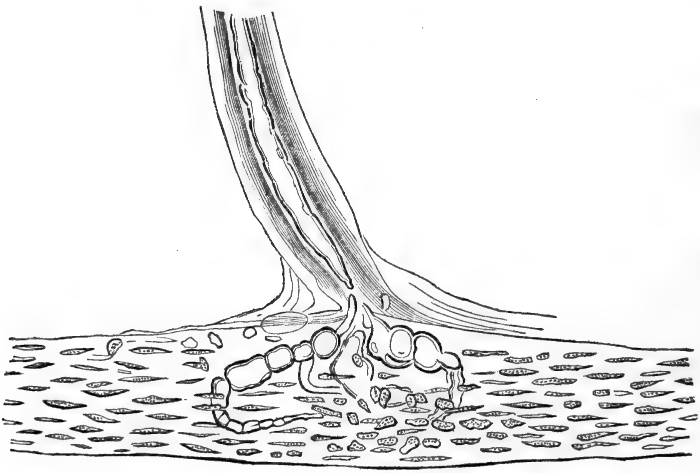
Besides the sentient nerves which I have here described, I have found, in the same cutaneous muscle of the frog, nervous filaments on the blood-vessels, although I have not yet been able fully to satisfy myself as to their source and distribution. These vascular nerves entirely agree in character with the pale sentient fibres, and, like them, are furnished with nuclei. I have found them chiefly on small veins, and on fine vessels on the arterial side of the capillaries, which were, however, destitute of a muscular coat. On these vessels I have traced them a considerable way, passing from one branch to another and often dividing, but have not been able to observe distinct terminations. Now and then also I have recognized them on vessels possessing a decidedly arterial structure; but I am unable to state anything positive as to their arrangement in this case, inasmuch as the nuclei in the outer coat of the small arteries occasion much difficulty in following out pale nucleiferous fibres. Once only I was fortunate enough to trace the origin of one of these vascular fibres from a dark-bordered sentient fibre; and this observation, so far as it goes, together with their distribution to vessels destitute of a muscular coat, favours the notion that the nerve-fibres in question are of the sentient kind.

On the Nerve-tufts in the Cutaneous Muscle of the Frog.

In the same muscle of the frog to which I have already made so constant reference, there may be observed, and most commonly about the end of the winter season, from three to five objects of peculiar structure, which at first sight appear to be of the nature of tactile corpuscles or terminal nerve-bulbs, but which really do not belong to that class of organs. The objects in question are indicated in

figure 2 at *i i*, and one is represented on a larger scale in figure 4. They are found on the finer muscular fibres, and appear on a superficial inspection as nodular swellings or somewhat thickened portions of the fibre, marked by a profusion of rather roundish nuclei, and receiving a single very thick nerve-fibre, loosely surrounded by its comparatively wide membranous sheath. On further examination it is found that the nerve-fibre, on reaching the nodule of the muscle, is wound up at one or more spots on the surface into a coil or tuft, in the mean time undergoing repeated division; and for the most part it may be seen to enter the muscular fibre, in which the dark-bordered fibres, becoming finer, are finally lost to view.

Fig. 4 *.



The last-mentioned fact struck me as specially important, inasmuch as it seemed to afford an instance of the penetration of a nerve into the interior of a muscular fibre, as maintained by Kühne. On a careful examination, however, of the structures in question in muscles rendered transparent by acetic acid, I found that the apparently simple muscular fibre bearing the swelling is really a small bundle of from three to seven fine fibres (fig. 5), and that the

* Fig. 4. A nodule or swelling of a muscular fibre (or rather fasciculus), with a large nerve-fibre, nerve-tuft, and nuclei; highly magnified.

apparently penetrating nerve-fibres merely pass between these muscular fibres.

Fig. 5 *.

On tracing the peculiar and seemingly simple muscular fibres towards the ends of the muscle, I found that they there evidently consist of several smaller but still transversely striated fibres, and this brought to my mind the bundles of fine muscular fibres described by Weismann †, and known to me also from actual inspection, which induced that observer to infer the occurrence of longitudinal divisions of the striated fibres. This led me to study these bundles singly by treating the cutaneous muscle with strong solution of potash, and then I found that it is precisely these particular bundles which (appearing as single fibres) exhibit the peculiar swellings with the coiled nerve. In the situation of the swelling the finer component fibres of the bundle cling fast together, even after the operation of the alkali, and a certain amount of striated granular uniting matter is found between them, which may be partly the remains of fine nerve-fibres and capillaries, and some accompanying connective tissue. Now, if it be admitted that the finer muscular fibres composing the bundle are generated by the division of thicker muscular fibres, as Weismann justly concludes, the explanation of the nerve-tufts becomes easy; inasmuch as they may be conceived to arise from a simultaneous growth and division of the nerve-fibre belonging to the parent muscular fibre, in order that each of the young muscular fibres may obtain its branch of nerve. The process by which this is effected cannot be satisfactorily studied in consequence of the close cohesion of the fine muscular fibres at the spot, but most probably the original pale terminal fibres of the parent nerve undergo further development by growth and by multiplication of the nuclei, so as finally to supply all the new muscular fibres; and I think it not

* Fig. 5. A small bundle of fine muscular fibres into which an apparently simple (but really longitudinally dividing) fibre has been resolved by means of potash. *a*, Nodule or swelling which contained a nerve-tuft.

† Zeitschr. für rationelle Medicin, 1860, Band x.

unlikely that a part of the numerous nuclei in the neighbourhood belong to the nerve-fibres.

Simultaneously with the increase of its terminal fibres the dark-bordered parent fibre doubtless augments in thickness, which explains the fact, otherwise difficult to understand, that it is invariably of much larger size than the nerve-fibres proceeding to the other muscular fibres.

From what has been stated it will thus be seen that the relation of the nerve to the peculiar structures described does not support the views of Kühne.

On the Termination of the Nerves in the Involuntary Muscles, and on the disposition of the Nerves in the Heart of the Frog.

Having, to the best of my power, investigated the ending of the nerves in voluntary muscles, I thought it right to extend my researches to those of the involuntary muscular organs, respecting the ultimate terminations of which very few, if any, satisfactory observations have been heretofore made known.

The muscles I selected for examination were those of the heart and pharynx of the frog, and the methods of preparing them were the same as for the voluntary muscles.

The nerves of the frog's heart are the two nervi vagi or pneumogastrics, which, having reached the heart at the place where the venous sinus joins with the right auricle, enter the septum between the two auricles, and proceed therein to the ventricle. During this part of their course the two trunks of the vagi, which join together at their entrance into the heart, are almost everywhere beset with numerous ganglionic cells. These cells in some parts form larger masses, well described by Bidder several years ago*. One or two of these masses lie in the upper part of the septum, and may be called the "superior auricular ganglions"; another is situated in the lower part of the septum, near the ventricle, the "inferior auricular ganglion"; and a very remarkable and constant ganglion is seated in each of the two larger valves placed between the ventricle and the auricles,—these are the "ventricular ganglia" of Bidder.

Apart from these larger ganglionic masses on the trunks of the vagi, there are found, although without regular arrangement, gangli-

* Müller's Archiv, 1852.

onic cells, single or in small clusters, on the larger branches distributed to the venous sinus and the auricles, but none on those of the ventricle; so that the ganglionic matter of the ventricle exists only at its venous aperture.

Respecting the microscopic structure of these parts, it is known that amongst the ganglionic cells there are many which give origin each to only one nervous fibre, and have therefore been termed unipolar cells, whilst there are others that apparently give off no fibres, and are named apolar cells; but it is as yet altogether unknown whether the proper or radical fibres of the vagi connect themselves with the ganglionic cells of the heart or not, and it is equally uncertain from what immediate source the nerve-fibres proceeding to the muscular substance of the heart are derived.

Most physiologists seem to believe that the fibres of the vagus or pneumogastric are not directly distributed to the muscular fibres of the heart, but that they act only through the medium of the ganglionic cells, and that the fibres derived from these cells are the real motory nervous elements. So far as I know, however, notwithstanding its importance, no one has hitherto examined into this matter.

My own inquiry has had a twofold object: first, to ascertain the relation of the pneumogastric to the ganglionic cells; and secondly, to find out the mode of termination of the ultimate divisions of the nerves on the muscular fibres.

As regards the first question, all my observations tend to show that the nervous system of the frog's heart is constituted on a very simple principle. There are, in fact, two distinct although associated systems of nervous fibres distributed to every part of the muscular walls of the heart: namely, first, the proper or radical fibres of the vagus, which pass through the ganglia of the heart, without, so far as I can discover, connecting themselves with the ganglionic cells, and proceed straightway to the muscular fibres; and secondly, fibres derived from ganglionic cells, which are associated with the original vagus-fibres, and run with them to the muscular tissue.

The obvious bearing of this matter on important questions of physiology requires that I should here explain the grounds on which the conclusion now stated is founded. On this head I have first to state that, in following with the microscope and with the aid of potash the fibres of the vagus as they pass through the several ganglions of the

heart, it is easy to see that they run through these bodies without forming any connexion with the ganglionic cells. Of course I do not pretend to have traced every individual fibre, but I can speak positively of the large majority, and conclude that the same holds good generally, the more so as the ganglion-cells themselves present conditions which corroborate this conclusion. These cells, that is to say, all of them which are connected with nerve-fibres and whose connexions can be clearly made out, are unipolar, or send out but a single fibre, and that in a peripheral direction, without having any connexion with the transcurrent fibres of the vagus. Bipolar or multipolar cells are not to be seen: some apparently apolar cells present themselves, but concerning these it may be doubted whether they are not unipolar cells whose issuing fibre is in some way hidden from view. I may add that, as the cells lie mostly at the side of the vagus and its branches, and not amongst its fibres, their relation to the latter is less difficult to determine.

From what has been said I feel justified in my conclusion that there are in the frog's heart two distinct systems of nervous fibres, one ganglionic, the other directly proceeding from the vagi or pneumogastric nerves.

Before proceeding, in the next place, to describe the terminations of the nerves, I have to explain that the muscular tissue of the frog's heart is entirely made up of short spindle-shaped uninucleated fibres, or fibre-cells, resembling in every respect those described by me in the involuntary muscles generally, except that they have very distinct transverse striæ. These striated fibre-cells, as correctly described by Weismann, are arranged so as to form larger and smaller bundles, and these unite into a network which, in each of the three parts of the heart, is continuous throughout. Now, respecting the nerves, it is easy to show that their smaller branches, composed of dark-bordered fibres from the vagus and the ganglion-cells, also form a general network, with larger and smaller meshes, on or between the secondary muscular fasciculi. As the ramifications become finer the component nerve-fibres gradually lose their medullary sheath, and are finally continued into pale nucleated filaments which lie singly or two or three together in the finest muscular bundles. The last terminations are pale, nucleated fibres, entirely agreeing in aspect with those described as the ends of the sentient nerves in the voluntary muscles.

They enter here and there into the fasciculi of muscular fibre-cells, ramify in these bundles, and end in very fine pointed extremities. The muscular fasciculi are on the whole rather richly supplied with these terminal fibres, so that two or three may be readily found in each bundle by any one accustomed to such investigations. Still it is plain that the nerve-terminations are by no means equal in number to the muscular fibre-cells, so that several of the latter must be governed by one and the same terminal nerve-fibre.

The same mode of nerve-termination as in the heart prevails in the non-striated muscular tissue of the pharynx and bladder of the frog, excepting that the terminal fibres are more scanty; but as these run over a larger extent of the muscular tissue before coming to an end, their comparative rarity may be compensated by their acting at the same time upon a larger number of the muscular fibres.

With these remarks I conclude the account of my anatomical investigation of the terminations of nerves in muscles. It will be obvious to every one conversant with the physiology of the muscular system, that many of the facts on which I have dwelt are of more or less importance in reference to physiological questions; and I would have gladly availed myself of the opportunity now so courteously afforded me of addressing this learned Society, to take up also the physiological side of the question, did I not fear to have already made too large a demand on your patience. I refrain therefore from entering on so large a subject, and shall take leave only to offer the following as what appear to me to be the more interesting physiological inferences from the anatomical facts I have described.

1. As the motor nerves lie outside the sarcolemma, and are confined in their distribution to a comparatively short portion of the muscular fibre, it may be inferred that there must be action at a distance.

2. As the ends of the motor nerves are pale fibres destitute of medullary sheath, it would appear that the latter is of but secondary importance. The same fact may perhaps also afford an explanation of the special action of certain poisonous agents, as the urari, on the ends of these nerves.

3. The muscles have numerous sentient nerve-fibres distributed at their surface, or on the surface of their larger divisions.

4. The heart, at least the heart of the frog, has two distinct sets of nervous fibres, those of the pneumogastric and those from the ganglion-cells, which are both distributed to the muscles. The vagus therefore acts directly on the muscular fibres of the organ, and the well-known experiment of Weber can scarcely be explained through a supposed action of the vagus on the ganglia. On the other hand, the ganglia and their fibres are also motorial organs of the heart, and alone act when it is separated from the body.

May 8, 1862.

Major-General SABINE, President, in the Chair.

Heinrich Wilhelm Dove, of Berlin, elected a Foreign Member in 1850, and Henri Victor Regnault, of Paris, elected a Foreign Member in 1852, were admitted into the Society.

The following communication was read:—

"Appendix to the Account of the Earthquake-Wave Experiments made at Holyhead." By Robert Mallet, Esq., C.E., F.R.S. Received March 27, 1862.

(Abstract.)

This communication contributes the sequel of the author's "Report on Earthquake-Wave Experiments" (made at Holyhead), as published in part 3 of the 'Philosophical Transactions' for 1861. At the conclusion of that paper the author expressed his hope of being able soon to lay before the Royal Society some experiments for the determination of the modulus of elasticity of perfectly solid portions of both the slate and the quartz rock formation through which his wave-transit experiments had been made at Holyhead, with a view to throw light upon the relations between the theoretic velocity of transmission (if the rocks were all solid and homogeneous) and the actual velocity as determined by experiment.

He has now determined the elastic modulus for both rocks, and for each rock in two directions, viz. parallel to and transverse to its lamination; and he has extended his determinations to specimens of

each rock of maximum and of minimum compactness and hardness, so that the series of experiments upon the compressibility of these rocks (from which the modulus is derived) assumes the following divarication, viz. :—

Slate rock ..	Hardest .	B. Parallel to laminæ, Table 2.
		A. Transverse to laminæ, Table 1.
	Softest ..	F. Parallel to laminæ, Table 6.
		E. Transverse to laminæ, Table 5.
Quartz rock .	Hardest .	D. Parallel to laminæ, Table 4.
		C. Transverse to laminæ, Table 3.
	Softest ..	H. Parallel to laminæ, Table 8.
		G. Transverse to laminæ, Table 7.

Involving thus eight distinct series of experiments.

The compressions were conducted at the Royal Arsenal, Woolwich, by the aid of the excellent American machine belonging to the Royal Gun-factories, permission to use which was accorded to the author.

The specimens of rock submitted to pressure were all equal cubes of 0·707 inch on the edge, presenting thus a surface on each side of 0·5 square inch—a dimension presenting facilities for tabular reduction, &c.

The cubes were cut from the chosen rock specimens (selected with care as fairly representative) by means of the lapidary's wheel, and had opposite faces rigidly parallel and equal.

The pressures advanced by 1000 lbs. per square inch of surface, from zero up to the crushing point of the specimen; and at each advance the actual compression of the column of rock was measured by instrumental arrangements that admitted of reading space to ·0005 of an inch. The results are given in Tables numbered 1 to 8, referred to above, and these are compared in two Tables numbered 9 and 10.

The following are the mean compressions for each 1000 lbs. per square inch:—

Slates.				Quartz.			
A.	B.	E.	F.	C.	D.	G.	H.
inches. ·000627 up to 23,000 lbs.	inches. ·0025000 up to 26,000 lbs.	inches. ·0039144 up to 14,000 lbs.	inches. ·0037000 up to 7000 lbs.	inches. ·0007085 up to 35,000 lbs.	inches. ·0010947 up to 19,000 lbs.	inches. ·0014666 up to 12,000 lbs.	inches. ·0172666 up to 6000 lbs.

Crushing usually took place at 1000 to 2000 lbs. additional pressures beyond the above limits, up to which the compressions were tolerably uniform.

The discussion of these Tables fully presents some interesting and novel results.

Generally the quartz rock is less compressible than the slate; the softest quartz, however, is much more compressible than the softest slate in a direction parallel to the lamination of both. In this direction also the hardest slate is more than double as compressible as the hardest quartz. Transverse to the lamination, however, both the hardest slate and quartz have nearly the same coefficient of compressibility, which is very small for both. In the latter direction also the *softest* slate and quartz have almost the same coefficient, but one about *four times* as great as for the *hardest* like rocks.

The author points out several conclusions of much interest deducible from these experiments as to the physical and geological conditions under which these rocks were formed and consolidated. The compression by natural forces has already been greatest in directions transverse to the lamination. The great compressibility in the opposite directions, or parallel to the lamination, appears to arise chiefly from the mass of the rock being made up of minute wedge-shaped mineral particles, deposited all with their largest dimensions on the plane of lamination, and so acting on each other like wedges.

Some curious circumstances in the mode of giving way of the rocks under pressure are shown by the author to be probably connected with their mass being formed of an aggregate of several simple minerals.

He points out the great differences in wave-transmissive power in directions transverse to and parallel to the lamination which these experiments disclose. The specific gravities of the several specimens of rock are then given, to enable the modulus of elasticity to be obtained in feet, and the general results of the experiments are comprised in the following Table (p. 87):—

The author then proceeds to apply these results to the comparison of the theoretic and actual transit-periods of the wave of impulse.

The general expression for elastic wave-propagation in a homogeneous medium may be expressed by an equation of the form

$$V=\sqrt{gL}=8{\cdot}024\sqrt{L},$$

Holyhead Rock Compression.

General results reduced, modulus of cohesion and of elasticity, &c.—Slate and Quartz.

No.	Class of rock, and direction of pressure in relation to structure.	Coefficient of compression on unit surface for 1000 lbs.	Elastic limit for compression.	Crushing load on the unit of surface.	Modulus of cohesion (compression).	Modulus of elasticity.	Modulus of elasticity.	Coefficient T_r.
		inches.	lbs.	lbs.	feet.	lbs.	feet.	
1	Slate *hardest across* lamination	·0006217	22,000	24,000	20,014	8,042,464	6,706,524	1·2432
2	Quartz *hardest across* lamination	·0007085	32,000	37,000	32,095	7,057,163	6,121,758	2·1830
3	Slate *hardest parallel* to lamination	·0025000	18,000	27,000	22,515	2,000,000	1,667,778	5·6241
4	Quartz *hardest parallel* to lamination	·0010947	17,000	20,000	17,349	4,567,461	3,962,013	1·8240
5	Slate *softest across* lamination	·0039144	12,000	15,000	12,586	1,277,335	1,071,769	4·8930
6	Quartz *softest across* lamination	·0014666	11,000	14,000	12,158	3,409,246	2,960,699	1·7108
7	Slate *softest parallel* to lamination	·0037000	6,000	9,000	7,552	1,351,351	1,133,874	2·7747
8	Quartz *softest parallel* to lamination	·0172666	7,000	8,000	6,948	289,576	251,477	11·6112
	Calculated Means.							
9	Slate, mean for hard and soft *across* lamination	·0022680	17,000	19,500	16,311	2,204,585	1,844,069	3·6855
10	Quartz, mean for hard and soft *across* lamination	·0010875	16,500	25,500	22,132	4,597,701	3,990,455	2·3103
11	Slate, mean for hard and soft *parallel* to lamination	·0031000	12,000	18,000	15,056	1,612,903	1,349,145	4·6494
12	Quartz, mean for hard and soft *parallel* to lamination	·0091806	12,000	14,000	12,151	544,627	472,684	10·7100
	Calculated Means of Means.							
13	Slate, hard and soft, mean for both directions (Nos. 9 and 11)	·0026840	14,500	18,750	15,684	1,862,880	1,566,541	4·1914
14	Quartz, hard and soft, mean for both directions (Nos. 10 and 12)	·0051340	16,750	19,750	17,141	973,899	845,252	8·4490
15	General mean for slate and quartz, hard and soft, and in both directions (Nos. 13 and 14)	·0039090	15,625	19,250	16,398	1,279,099	1,089,615	6·2697

where L is the modulus of elasticity in feet. Where, from want of homogeneity or shattering, &c., as found in nature, the experimental value of V differs from this, we may express it by the same form of equation,

$$V' = \alpha \sqrt{L},$$

the coefficient α having to $\sqrt{2g}$ the rate that the actual bears to the theoretic value of V.

He then determines the value of α for three of his mean experimental transit-velocities at Holyhead, and obtains as follows:—

Feet per side.	
$V' = 1089$	$\alpha = 0{\cdot}637$
$V' = 1352$	$\alpha = 0{\cdot}791$
$V' = 1220$	$\alpha = 0{\cdot}714$

The actual velocity of wave-transmission in the slate and quartz rocks, taken together, was to the theoretic velocity due to their materials, if perfectly solid,

$$\alpha : \sqrt{2g}, \text{ or as } 1{\cdot}00 : 8{\cdot}89;$$

so that nearly eight-ninths of the full velocity of wave-transmission due to the solid material is lost by reason of the heterogeneity and discontinuity or shattering of the rocky mass as it is piled together in nature.

The author then shows that were the rocks *quite solid*, the velocity of wave-transmission would be—

Mean of slate and quartz transverse to lamination V=13,715 feet per second.

Mean of slate and quartz parallel to lamination V=7659 feet per second.

This difference is probably reversed *in nature* by reason of the greater discontinuity in the former direction. The author then shows that his results, which appear at first sight to conflict with those of an analogous character obtained by Helmholtz and others for wood, in the three principal directions of its section, are strictly in accordance and analogy with the results of these experimenters.

The author concludes by deducing some conclusions as to the bearing power, safe load, and proper direction as to lamination when exposed to pressure, of these rocks, of a practical character, and valuable to the civil engineer or architect.

May 15, 1862.

Major-General SABINE, President, in the Chair.

Dr. William Stokes and George Johnstone Stoney, Esq., were admitted into the Society.

The following communications were read:—

I. "On the Sensory, Motory, and Vaso-Motory Symptoms resulting from Refrigeration and Compression of the Ulnar and other Nerves in Man." Second Communication. By AUGUSTUS WALLER, M.D., F.R.S. Received April 12, 1862.

In the 'Proceedings of the Royal Society,' No. 46, I have given a brief account of the effects of refrigeration of the ulnar nerve in man, by the application of ice. After repeated experiments on this nerve on myself, I never had occasion to witness any permanent disturbance in the nerve from the application of the ice, beyond a slight hyperæsthesia, which may have been partly due to the increased attention of the mind directed to the part whose functions were under investigation.

The like immunity does not extend to experiments in which a much greater degree of cold is employed, as will be seen from the following instance:—

September 13, 1861.—A freezing mixture, consisting of equal parts of pounded ice and common salt, was applied to the elbow of the left arm over the ulnar nerve. The subject of the observation was a gentleman in perfectly sound health, aged twenty-seven.

Before the application of the mixture, the temperature between the little and ring fingers and the index and median was the same. Successive observations gave the following results:—

Duration of application of refrigerating mixture. Minutes.	Temperature Cent. between ring and little fingers at roots.	Temperature Cent. between index and median fingers at roots.
5	25°·3	27°·8
7	25·6	28·0
10	26·1	28·2
25	26·6	29·6

Duration of application of refrigerating mixture. Minutes.	Temperature Cent. between ring and little fingers at roots.	Temperature Cent. between index and median fingers at roots.
27	27°·8	30°·1
29	26·6	29·1
33	26·4	28·5
35	27·5	29·0
37	29·2	28·8
40	30·5	28·0
44	31·2	27·5
46	32·5	27·0
47·5	33·5	26·6
50	33·5	26·0
52	33·8	26·0

At this time the freezing mixture was removed.

In the course of the application the following results were observed:—

Soon after the application of the freezing mixture, the nerve began to be painful from the elbow downwards; after twenty-seven minutes' application the little finger was already somewhat paralysed and insensible. When the nerve was compressed or vibrated, it was found to be very sensitive and tender, and its excitability much increased*.

After thirty-two minutes the symptoms were—

1st. Paralysis of the movements of the little finger almost complete.

2nd. A state of semi-flexion of all the fingers, diminishing from the little one outwards, so that they could not be straightened completely by the extensor muscles.

3rd. Great weakness in all the fingers and thumb, so that great difficulty was experienced in grasping or holding any object.

4th. Complete loss of the power of abduction and adduction of the fingers, so that when they were extended as far as possible they

* I apply the term "vibration of a nerve" to the act of pushing it aside with the tips of the fingers so as to render it tense, and then suddenly removing them. By so doing a sound is generally produced by the nerve, as by a vibrating cord. This little operation causes a slight degree of pain, sufficient to enable us to judge of the degree of sensibility of the nerve. It likewise causes slight movements of the last finger from muscular contraction, sufficient to indicate the degree of excitability of the nerve. For further account see my paper on the subject of irritation of the ulnar nerve, &c. in the 'Medical Gazette.'

remained spread out without there being any power of bringing them together.

After fifty-two minutes the freezing mixture was withdrawn, and the skin at the elbow, which was completely frozen, was allowed to thaw. During this process the arm and hand became rapidly very painful upwards and downwards in the course of the nerve, the pain even extending to the chest, where there was a feeling of great uneasiness and constriction. At the same time the heart's action was so much lowered as to threaten immediate syncope, which was, however, prevented by a free administration of ammonia and spirits. An hour after the removal of the freezing mixture the temperature (Cent.) was as follows:—

Left hand,	between the index and median	20°·5
„	between the little and ring fingers....	21·2
Right hand,	between the index and median	21·8
„	between the little and ring fingers ..	21·8

September 16th.—Over the skin which had been frozen there was vesication and inflammation of the subcutaneous tissue, which was swollen around the part blistered to the extent of 2 or 3 inches over the arm and forearm. Over the prominent parts of the elbow-joint the bones were very tender on pressure. The ulnar nerve was likewise exceedingly painful to the touch up the arm and down the forearm and the hand. The brachial plexus was very tender at some points. Over the little finger and the inner side of the ring finger the skin was nearly insensible. Friction over these parts caused a sensation of heat, pricking, and discomfort. The temperature of these parts was not more elevated than other parts of the hand. The paralysis of the muscles of the hand was in the same condition as on the 14th. The two points of compasses, when more than an inch apart, were felt as one over the palmar surface of the little finger.

September 17th.—Paralysis and anæsthesia the same as the day before. The hyperæsthesia of the ulnar nerve was diminished.

September 22nd, one week after application of ice.—Considerable and constant hyperæsthesia is experienced in the little finger and down the ulnar side of the hand. The power of motion is somewhat restored, but neither the little nor any of the other fingers can

be completely extended. The spot to which the freezing mixture was applied is still sore, and the ulnar nerve immediately under it very sensitive.

The following observations were made on the temperature of both upper extremities:—

Right (unaffected) side.		Left (paralysed) side.
$28^{\circ}\cdot6$ Cent. (radial side of second finger)	......	$26^{\circ}\cdot6$.. diff.—2°
24·7 (after some minutes' exposure to *cold air*)		22·7.. diff.—2°
34·3 (at bend of arm)	....	33·7.. diff.—·6
33·4 (at middle of inner side of arm)	....	33·6.. diff.+·2

Right forearm.		Left forearm.	
Radial side.	Ulnar side.	Radial side.	Ulnar side.
31°·4	29°·1.. diff. 2°·3.	31°	28°·6.. diff. 2°·4

After some minutes' exposure to cold air, the temperature of the little fingers of both hands sunk to—

	Right side.		Left side.
(1)	$22^{\circ}\cdot1$	....	$20^{\circ}\cdot7$.. diff. $1^{\circ}\cdot4$
(2)	21·4	....	19·7.. diff. 1·7
(3)	21·5	....	19·7.. diff. 1·8

Both arms were then well exercised; as the result of which the temperature rose in successive observations to—

	Right side.		Left side.
(1)	31°	....	25° .. diff. 6°
(2)	34	....	31·1.. diff. 2·9
(3)	34·5	....	31·5.. diff. 3
(4)	35	....	33·5.. diff. 1·5
	35	....	34 .. diff. 1

These observations show:—

(1) That the mean difference of temperature between the paralysed and sound arms was 2° Cent.=3°·6 Fahr.

(2) That this difference lessened as the temperature of both arms was lowered.

(3) That the temperature of the sound side was *at first* increased much more rapidly than that of the side paralysed, but that after a short time the normal difference 2° was re-established.

September 27th.—The power of motion in the little finger is

almost restored; but there is still sensible hyperæsthesia in it, as well as on the inner side of the palm of the hand. There is also some stiffness in that finger, and a complete inability, when the fingers are extended, to bring them into opposition with one another.

The spot on the upper arm to which cold was applied is now quite healed, and all swelling has subsided; but the ulnar nerve is still very sensitive on being touched.

The weakness of the left hand in grasping objects, as in using a fork at dinner, &c., even ten days after the experiment, was most perceptible.

The temperature on the radial side of the little finger of both hands is—

Right.		Left.
33°·5 Cent.	....	32°·3.. diff. 1°·2

October 1st.—Temperature of hands.

Right.		Left.	
Little finger.	Fore-finger.	Little finger.	Fore-finger.
33°·5	33°·7	32°·2	32°·5

There was still considerable numbness in the little finger and on the inner side of the palm of the hand. The fingers could now be extended almost as completely as those of the right hand, but they could not be easily expanded, or brought into opposition with one another. At times the nerve was very painful along its whole course, from the elbow downwards. This is mostly the case after the arm has been used a little. Pain at these times, even if not actually present, was easily induced by placing the arm in a dependent position so as to cause congestion of its vessels, and it was immediately relieved by elevating the limb above the head so as to deplete the vessels. The nerve was very sensitive to pressure all along its course, from the elbow downwards, although all painfulness arising from the inflammation produced in the cutaneous tissue by the cold had disappeared for some days.

On both hands being held for some minutes in a stream of running water at a temperature of 14°·4 Cent., considerable pain was experienced in the little finger and inner side of the left hand; and on withdrawing both hands their temperature was—

Right little finger.	Left little finger.
16°·5	16°·3

The effect of the reduction of temperature on the paralysed hand was very distinct; the three inner fingers were more or less contracted, the little one being semiflexed, and could not be extended completely, those of the right hand being easily extensible.

In half an hour the temperature of both hands had risen to—

Right little finger.	Left little finger.
°	°
34·2	33
34·5 (12 P.M., one hour after immersion)......	34

At this moment, although the temperature of the paralysed fingers was higher than before immersion in water, the little finger was still semiflexed, and none of the others could be perfectly extended.

February 14th, 1862.	Right hand. Cent.	Left hand. Cent.
Temperature between little and annular fingers	19°·5	18°·5
" " annular and median	18·8	18·3
" " median and index	19·0	18·1
" " thumb and index	21·1	20·1
" of palmar surface of little finger at root	18·0	17·9
Temperature of forearm, posterior surface, lower fourth	28·3	28·0
Temperature of radial side of forearm........	27·0	27·6
" of palm of hand when closed	19·8	19·6

There is still a feeling of stiffness in the little finger, accompanied with slight inability to move it. There is also considerable sensitiveness along the course of the ulnar nerve in the palm of the hand when compressed or percussed. The fingers can be moved to and from one another more freely, but the power of so doing is still imperfect; and that of grasping, so far as the whole hand is concerned, is much weaker than in the other hand. Cutaneous sensibility is decidedly inferior over the dorsal and palmar surface of the inner part of the hand and two corresponding fingers when compared with the right, but the sensibility to cold is the reverse; whenever the left hand becomes cooled by exposure, the little finger is always more or less painful. Vibration of the nerve at the elbow produces the same effect on both sides.

For about two weeks during the cold weather in this month there existed over the palmar surface of the left little finger a chilblain, the only one to be found on either hand.

The left hypothenar eminence was likewise then discovered for the first time to be less firm and smaller than on the right hand, as if atrophied in consequence of the results of the experiment; and to such a degree that the swelling formed by this eminence on the inner side of the hand, which was considerable on the right hand, was nearly absent on the left.

To ascertain how far this difference is normal in right-handed persons, I examined the hands of several persons accustomed to manual labour, where the preponderance of the upper right limb is great; but I found much less than in the present instance. I must therefore attribute this atrophy of the left hypothenar eminence partly to the semi-paralysis of the muscles, and especially to the diminished nutrition from constriction of the vessels.

Compression of the Ulnar Nerve.

I compress this nerve simply by resting the elbow on some hard body slightly padded, and holding a heavy book in the hand to increase the weight on the elbow, which would otherwise be insufficient.

The symptoms are sensory, motory, and vaso-motory.

The sensory symptoms are those first perceived, being the well-known formication over the palmar territory of the nerve, viz. the little finger, the inner side of the annular, and the hypothenar eminence.

The symptoms of anæsthesia over the dorsal surface of the hand and fingers do not appear until a later period, when the anæsthesia is considerably advanced in the palmar portion.

While the anæsthesia is gradually progressing, the muscles of the hand governed by the ulnar nerve become weakened, stiff, and imperfect in their movements.

The first muscles which lose their power are the interosseous, which govern the adductive and abductive movements of the fingers. On trying alternately to spread out and to bring the fingers in contact, we find them to move imperfectly and weakly, in a trembling uncertain way, like the movements of old age. Later still the little

finger cannot be placed in apposition with the annular, but remains apart at an angle of 10 or 15 degrees. At the same time paralysis of the abductor and adductor muscles of the other fingers takes place.

The index and thumb alone retain some of their powers of adduction and abduction ; the former probably from the action of its proper extensor; the latter from its abduction not being animated by the ulnar nerve.

When paralysis of these muscles is complete, the appearance of the hand, when the fingers are in extension, is pathognomic, and as follows :—

The little finger in complete adduction at about an angle of 40° from the annular, which is likewise a little apart from the median. Both these fingers are slightly flexed and incapable of *complete extension.* The flexor muscles are much weakened, as might be anticipated from the distribution of the ulnar nerve. In addition, the flexion movements of the thumb and the extensor power of this and all the fingers are considerably weakened.

The tendency of the fingers to semiflexion, and the inability of the little, the annular, and even to a slight extent the median finger to accomplish complete extension is probably referable to the state of tonicity of the paralysed flexors. The weakness of the extensor and flexor powers of the thumb are not, in my opinion, sufficiently accounted for by the lost power of its adductor muscle. Still less can we account for the diminished power of extension of the other fingers by any direct or descending action of the ulnar nerve. I am led therefore to refer the diminished power, in this case, of the extensor muscles of the fingers to reflex action of the ulnar nerve. In support of this view, I may state that I have not unfrequently experienced, after vibrating the ulnar nerve at the elbow, a great lassitude of the whole limb, particularly marked over the deltoid muscle, to so great an extent as to occasion much discomfort for at least an hour afterwards.

Vaso-motory symptoms.—Under this head I include all the perturbations of temperature of the integuments animated by the ulnar nerve, which attend its compression.

Mechanical irritation of this nerve, such as vibrating it, will frequently cause an immediate fall of 0°·5 Cent. to 1°·0 Cent. of the

mercury in a thermometer placed between the little and annular fingers, while the others remain nearly unaffected.

It is still more easy to ascertain this influence when the nerve is paralysed by pressure.

Thus, in the following observation, I found—

	°Cent.
When the integuments of the little finger began to tingle between the roots of the little and annular fingers	=22·4
At the same time between the roots of index and median	23·4
When the anæsthesia over the little finger was more advanced and its adductor muscle somewhat paralysed between roots of little and annular fingers	21·4
At the same time between index and median	22·4
Some minutes later between little and annular finger	21·0
At the same time between index and median	22·3
When the paralysis and anæsthesia were nearly complete between little and annular fingers	19·0
At the same time between index and median	22·3
The temperature of the right hand between the fingers remained nearly stationary	24·0

In other experiments on compression of the ulnar nerve I have observed a much greater fall of temperature as the integuments became insensible.

With regard to the elevation of temperature, I have never been able to obtain it by compression of the ulnar nerve to the extent produced by refrigeration.

Compression of the Left Radial Nerve.

Feb. 10, 1862.—This nerve was compressed at the lower part of its course at the arm, where it winds round the humerus to its external side. This was effected simply by pressing the outer part of the arm against the padded arm of a chair.

In about half an hour the skin of the back of the hand had become somewhat insensible, and the muscles of the forearm so much weakened that the hand dropped by its own weight. These symptoms continued to increase until the extensor muscles of the forearm were quite paralysed. Examination three-quarters of an hour after the commencement of the experiment gave as follows:—

The skin over the back of the forearm, that of the carpus, of the thumb, of the two first phalanges of the index, and over the outer half of the thenar eminence was nearly insensible to the contact of external bodies; but when pricked or pinched, pain was produced of a hot, burning character, which lasted for about a minute after the removal of the cause. Over the back of the other fingers, *i. e.* the median, annular and little fingers, and the last phalanx of the index, no loss of sensibility could be detected.

Flexion of the fingers was very imperfect; their tips with difficulty could be made to touch the palm of the hand. The movements of the last phalanx of the thumb were almost paralysed, while those of its second phalanx and metacarpal bone appeared little affected.

The apposition of the tips of the thumb and index, as well as that of the thumb and median, could be made, but not that of the thumb and tips of the two last fingers. All extension of the carpus on the forearm was impossible. The hand, when left to itself, fell into a state of semiflexion of the carpus on the forearm, with semiflexion of the fingers; while the thumb placed itself on the palm of the hand by the flexion of its metacarpal bone and first phalanx, the second phalanx remaining unbent.

The movements of supination of the forearm and abduction of the carpus were almost entirely paralysed.

	Left side. ° Cent.	Right side. ° Cent.
Temperature between roots of thumb and index.	28·0	28·3
Temperature over dorsum of first phalanx of index	23·4	26·7
Temperature over dorsum of second phalanx of index	20·2	25·7
Temperature over dorsum of first phalanx of little finger	19·0	27·0

One minute after removal of the pressure the thumb could be flexed at both phalanges into the palm of the hand; apposition of the thumb and annular finger was possible. The skin over the part supplied by the nerve was rather less numb.

Two minutes later the carpus could be placed in a straight line with the forearm, but could not be retained in that position.

Thirteen minutes later the hand was in the same condition. All delicate movements, such as buttoning and unbuttoning the dress, were impossible, although the sensibility at the tips of the index and thumb was equally acute in both hands.

A quarter of an hour later the temperature of the hand was as follows:—

	Left hand. °Cent.	Right hand. °Cent.
Temperature over dorsum of carpus	23·0	25·0
,, between roots of index and median	19·5	21·9
,, between roots of annular and little finger	19·8	21·6
(1) Temperature over dorsum of forearm	29·0	32·1
(2) ,, of external side of forearm ..	29·0	31·2
(3) ,, of external side of forearm ..	27·5	29·0

Temperature of (1) and (2) was obtained with the forearm covered by the usual dress; that of (3) was obtained after the arms had been denuded for some time.

February 10, 2.25 P.M.:—

	Left side. °Cent.	Right side. °Cent.
Temperature between the roots of the thumb and index	18·8	23·5
Temperature over dorsum of median and metacarpal bone	19·0	23·5
Temperature between the tips of thumb and index	19·0	30·5
,, between roots of index and median	26·5	29·5
,, between median and annular	30·3	30·9
,, between annular and little finger..	31·3	30·5
,, (second observation) between thumb and index	21·0	32·3
Temperature over dorsum of forearm, under coat and flannel vest	26·0	29·3
Temperature of anterior part of forearm, covered as above............................	28·0	30·5

6.45 P.M. After an hour's walk:—

Temperature between thumb and index	23·3	32·5
,, between index and median at roots	30·8	31·5
,, between median and index	33·7	33·3
,, between annular and little finger..	34·3	34·5

	Left side. °Cent.	Right side. °Cent.
Temperature over dorsum of carpus over second metacarpal bone	26·8	29·3
Temperature over dorsum of first metacarpal bone	28·5	31·1
Temperature over dorsum of first phalanx of thumb	30·8	30·2
Temperature of anterior part of first phalanx of thumb	32·1	32·2
Temperature over thenar eminence	28·5	32·5
Temperature over dorsum of forearm (lower part covered)	29·0	31·5
Temperature over dorsum of forearm (upper part covered)	28·8	30·6
Temperature over anterior part of forearm (lower extremity covered)	30·3	32·6
Temperature over anterior part of forearm (upper extremity covered)	30·5	31·5

The sensibility of the skin was in the same condition as on the previous day; the hand continued to drop, the fingers remaining in a state of semi-flexion as before, and there existed the same difficulty in performing all delicate movements. In conveying food to the mouth the supinators act so imperfectly that the operation is performed with considerable difficulty.

February 11, 10 A.M.:—

	Left side. °Cent.	Right side. °Cent.
Temperature over dorsum of thumb	19·0	21·9
,, between roots of thumb and index	21·7	27·0
,, between roots of index and median	21·5	23·1
,, between roots of median and annular	21·5	24·0
,, between roots of annular and little finger	21·0	22·8
Temperature of palm of hand when closed	22·6	24·8
Temperature over dorsum of forearm, lower extremity (uncovered)	25·1	30·5
Temperature over dorsum of forearm, middle part (covered)	28·7	30·8
Temperature over anterior surface of forearm, lower extremity (uncovered)	28·5	31·0
Temperature over anterior surface of forearm, upper part (covered)	30·2	31·0

Over the dorsum of the thumb, carpus, and forearm the sensibility was slightly improved.

Buttoning could be performed with difficulty. The fingers, the carpus, and the forearm could now be placed in a straight line. Abduction of the thumb, which on the right side forms an angle of 90°, on the left can be made to an angle of 45°.

February 12, 8 A.M. :—	Left side. ° Cent.	Right side. ° Cent.
Temperature between thumb and index at roots.	31·9	32·7
„ between index and median	31·7	32·3
„ between median and annular	31·8	32·4
„ between annular and little finger..	31·1	32·0
„ of palm of the hand when closed..	30·2	34·0
„ over lower part of forearm	30·0	31·3

The hand felt stronger and the movements were more free. Buttoning was effected more easily. Extension of the left hand backward as free as that of the right while the fingers were semiflexed, but not when they were extended.

February 13 :—	Left side. ° Cent.	Right side. ° Cent.
Temperature between thumb and index at roots.	24·3	27·8
„ between index and median	23·5	24·3
„ between median and annular	26·0	25·0
„ between annular and little finger..	26·7	26·8
„ over dorsum of thumb	27·5	30·0
„ over dorsum of index	25·3	25·9
„ over dorsum of lower part of forearm	26·5	28·3
„ over dorsum of upper part of forearm	31·0	31·8

Left hand felt much stronger. Left thumb could be placed in abduction at an angle of 60° with the index.

On the following days the sensibility of the skin continued gradually to improve, and also the motor powers, and after the lapse of twelve days from the date of the experiment there existed no difference in the sensibility, motor power, and the temperature of the two hands.

Compression of the Right Median Nerve.

January 2, 11 A.M.—The arm was allowed to hang over the back of a chair so as to compress the median nerve and brachial

vessels about 2 inches above the bend of the arm. In the course of about half an hour the nerve became paralysed, and the following symptoms were observed.

Motory Symptoms.—The thumb was incapable of flexion at the last joint, but could be flexed on the metacarpal bone, so as to be brought in contact with the tips of the three first fingers, but not with that of the little finger. There was considerable stiffness of the thenar muscles, particularly of the abductors. The index and median fingers remained slightly flexed, but by an effort of the will they could be straightened and could perform all movements of adduction and abduction. The index could only be made to flex very slightly, but the median could be completely flexed. The annular and little fingers retained all their movements, but felt weak. Flexion of the entire hand on the forearm was imperfect, and the muscles of the external part of the forearm felt very stiff.

Sensory Symptoms.—The thumb and two first fingers felt cold and numb, and nearly insensible, but when pressed, a sensation of heat was experienced. The insensibility at the tips of the thumb and two first fingers was nearly complete when objects were brought in contact with them. Numbness of the skin existed at the anterior part of the forearm.

	Right hand. °Cent.	Left hand. °Cent.
Temperature between index and thumb	20·4	
Temperature between index and median at roots of fingers	26·5	34·3
Temperature between median and annular	25·3	34·8

Some minutes after pressure was removed and motor power had returned, while there still remained some numbness and insensibility of the fingers, the

	Right hand. °Cent.	Left hand. °Cent.
Temperature was between index and median fingers	27·2	
„ between median and annular	25·3	

A quarter of an hour later:—

Temperature between index and median fingers .	28·0	34·3
„ between median and annular	26·8	34·5

Twelve o'clock:—

Temperature between index and median	27·6	
„ between median and annular	27·2	

There was still slight numbness of the fingers. After the right arm and hand were exercised for a few minutes, the

	Right hand. ° Cent.	Left hand. ° Cent.
Temperature between index and median	27·8	
„ between median and annular	26·8	

At 2 P.M., after walking and using both hands:—

Temperature between index and median	29·8	23·0
„ between the tips of thumb, index, and median	31·2	22·7

At 7 P.M. the temperature of both hands was the same.

II. "On the Rigidity of the Earth." By Professor WILLIAM THOMSON, F.R.S. Received April 14, 1862.

(Abstract.)

The author proves that unless the solid substance of the earth be on the whole of extremely rigid material, more rigid for instance than steel, it must yield under the tide-generating influence of sun and moon to such an extent as to very sensibly diminish the actual phenomena of the tides, and of precession and nutation. Results of a mathematical theory of the deformation of elastic spheroids, to be communicated to the Royal Society on an early occasion, are used to illustrate this subject. For instance, it is shown that a homogeneous incompressible elastic spheroid of the same mass and volume as the earth, would, if of the same rigidity as glass, yield about $\frac{7}{9}$, or if of the same rigidity as steel, about $\frac{2}{5}$ of the extent that a perfectly fluid globe of the same density would yield to the lunar and solar tide-generating influence. The actual phenomena of tides (that is, the relative motions of a comparatively light liquid flowing over the outer surface of the solid substance of the earth), and the amounts of precession and nutation, would in the one case be only $\frac{2}{9}$, and in the other $\frac{3}{5}$ of the amounts which a perfectly rigid spheroid of the same dimensions, the same figure, the same homogeneous density, would exhibit in the same circumstances. The close agreement with the results of observation presented by the theory of precession and nutation, always hitherto worked out on the suppo-

sition that the solid parts of the earth are perfectly rigid, renders it scarcely possible to admit that there can be any such discrepance between them as 3 to 5, and therefore almost necessary to conclude that the earth is on the whole much more rigid than steel. But to make an accurate comparison between theory and observation, as to precession, it is necessary to know the absolute amount of the moment of inertia about some diameter; and from this we are prevented by the ignorance in which we must always be as to the actual law of density in the interior. Hence the author anticipates that the actual deformation of the solid earth by the lunar and solar influence may be more decisively tested by observing the lunar fortnightly and the solar half-yearly tides*. These tides, it may be supposed, will follow very closely the "equilibrium theory" of Daniel Bernouilli for all oceanic stations, and the author suggests Iceland and Teneriffe as two stations well adapted for the differential observations that would be required.

The earth's upper crust is possibly on the whole as rigid as glass, more probably less than more. But even the imperfect data for judging referred to above, render it certain that the *earth as a whole must be far more rigid than glass*, and probably even more rigid than steel. Hence the interior must be on the whole more rigid, probably many times more rigid, than the upper crust. This is just what, if the whole interior of the earth is solid, might be expected, when the enormous pressure in the interior is considered; but it is utterly inconsistent with the hypothesis held by so many geologists that the earth is a mass of melted matter enclosed in a solid shell of only from 30 to 100 miles thickness. Hence the investigations now brought forward confirm the conclusions arrived at by Mr. Hopkins, that the solid crust of the earth cannot be less than 800 miles thick. The author indeed believes it to be extremely improbable that any crust thinner than 2000 or 2500 miles could maintain its figure with sufficient rigidity against the tide-generating forces of the sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they are.

* High tide, as far as the influence of either body is concerned, is produced at the poles, and low (average) water at the equator, when its declination, whether north or south, is greatest, and low water at the poles and high water at the equator, when the disturbing body crosses the plane of the equator.

III. "On the Difference in the Properties of Hot-Rolled and Cold-Rolled Malleable Iron, as regards the power of receiving and retaining Induced Magnetism of Subpermanent Character." By GEORGE BIDDELL AIRY, Esq., F.R.S., Astronomer Royal. Received April 22, 1862.

(Abstract.)

The author states that he had been desirous of examining whether differences in the degree of change of subpermanent magnetism, such as are exhibited by different iron ships, might not depend on the temperature at which the iron is rolled in the last process of its manufacture. By the good offices of Mr. Fairbairn he had received gratuitously from Richard Smith, Esq., Superintendent of Lord Dudley's Iron Works at the Round Oak Works near Dudley, twenty-four plates of iron, each 16 inches long, 4 inches broad, and $\frac{1}{4}$ inch thick; twelve of which, after having been manufactured with the others in the usual way, had been passed through rollers when quite cold. Each set of twelve was divided into two parcels of six each, one parcel being cut with the length of the bars in the length of extension of the fibres of the iron, the other being cut with the length of the bars transverse to the length of extension.

For experimenting on these, a large wooden frame was prepared, capable of receiving the 24 bars at once, either on a plane transverse to the direction of dip at Greenwich, or on a plane including the direction of dip. In some experiments, these planes were covered with flag-stones, and the bars were laid upon the flag-stones; in others, the bars were laid immediately upon the wood. While there lying, they were struck with iron or wooden hammers of different sizes. The bars of the different classes were systematically intermingled, in such a way that no tendency of the arm to give blows of a different force or kind in special parts of the series could produce a class-error in the result. For examination of the amount of polar magnetism in each bar, it was placed at a definite distance (5 inches) below a prismatic compass, which was used to observe the apparent azimuth of a fixed mark; the bar was then reversed in length, and the observation was repeated in that state.

The number of experiments was 21. They were varied by differ-

ence in the succession of positions of the bars, difference of time allowed for rest, difference in the violence of the blows, &c.

The principal results appear to be the following:—

1. The greatest amount of magnetism which a bar can receive, appears to be such as will produce (on the average of bars) a compass-deviation of about 11°, the bar being 5 inches below the compass. It was indifferent whether the bars rested on stone or on wood, or whether they were struck with iron or with wood, the bars lying on the dip plane while struck.

2. When the bars, thus charged, lay on the plane transverse to the dip, they lost about one-fifth of their magnetism in one or two days, and lost very little afterwards.

3. When the charge of magnetism is smaller than the maximum, the diminution in a day or two is nearly in the same proportion as for the maximum.

4. The effect of violence on the bars, when lying on the plane transverse to the dip, is not in all cases to destroy the magnetism completely, sometimes it increases the magnetism.

5. The Cold-Rolled Iron receives (under similar violence) or parts with (under similar violence) a greater amount of magnetism than the Hot-Rolled Iron, in the proportion of 6 to 5.

6. There is some reason to think that the Hot-Rolled Iron has a greater tendency to retain its primitive magnetism than the Cold-Rolled Iron has.

7. There is some reason to think that, when lying tranquil, the Hot-Rolled Iron loses a larger portion of its magnetism than the Cold-Rolled Iron loses in the same time.

IV. "On the Analytical Theory of the Conic." By Arthur Cayley, Esq., F.R.S. Received May 8, 1862.

(Abstract.)

The decomposition into its linear factors of a decomposable quadric function cannot be effected in a symmetrical manner otherwise than by formulæ containing supernumerary arbitrary quantities; thus, for a binary quadric (which of course is always decomposable) we have

$$(a, b, c \between x, y)^2 = \frac{1}{(a, b, c \between x', y')^2} \text{ Prod. } \{(a, b, c \between x, y \between x', y') \pm \sqrt{ac-b^2}(xy'-x'y)\};$$

or the expression for a linear factor is

$$1\frac{1}{\sqrt{(a, b, c \between x', y')^2}}\{(a, b, c \between x, y \between x', y') \pm \sqrt{ac-b^2}\,(xy'-x'y)\},$$

which involves the arbitrary quantities (x', y'). And this appears to be the reason why, in the analytical theory of the conic, the questions which involve the decomposition of a decomposable ternary quadric have been little or scarcely at all considered: thus, for instance, the expressions for the coordinates of the points of intersection of a conic by a line (or, say, the line-equations of the two ineunts), and the equations for the tangents (separate each from the other) drawn from a given point not on the conic, do not appear to have been obtained. All these questions depend on the decomposition of a decomposable ternary quadric, which decomposition itself depends on that for the simplest case, when the quadric is a perfect square. Or we may say that in the first instance they depend on the transformation of a given quadric function $U=(* \between x, y, z)^2$ into the form W^2+V, where W is a linear function given, save as to constant factor (that is, $W=0$ is the equation of a given line), and V is a decomposable quadric function, which is ultimately decomposed into its linear factors, $=QR$, so that we have $U=W^2+QR$. The formula for this purpose, which is exhibited in the eight different forms I, II, III, IV, I(bis), II(bis), III(bis), IV(bis), is the analytical basis of the whole theory, and the greater part of the Memoir relates to the establishment of these forms.

It will be sufficient for the present abstract to quote one only of these forms, viz.,

$$\text{I.}\quad (a, .. \between x, y, z)^2 = \text{Quotient by } (a, .. \between x', y', z')^2 \text{ of}$$

$$\left[\begin{array}{l} [(a, .. \between x, y, z \between x', y', z')]^2 \\ + \text{Quotient by } (A, .. \between mz'-ny', ..)^2 \text{ of Product} \\ \left\{ (A, .. \between mz'-ny', .. \between yz'-y'z, ..) \pm \begin{vmatrix} x, & y, & z \\ x', & y', & z' \\ l, & m, & n \end{vmatrix} \sqrt{-k(a, .. \between x', y', z')^2} \right\} \end{array}\right.$$

where the notation (which is of course explained in the Memoir) will, I think, be understood without difficulty, and I do not stop to explain it here.

The solution of the geometrical questions above referred to is, as

shown in the Memoir, involved in and given immediately by these forms. It is also shown that the formulæ are greatly simplified in the case *e. g.* of tangents drawn to a conic from a point in a conic having double contact with the first-mentioned conic, and that in this case they lead to the linear automorphic transformation of the ternary quadric. The Memoir concludes with some formulæ relating to the case of two conics, which, however, is treated of in only a cursory manner.

May 22, 1862.

Major-General SABINE, President, in the Chair.

The following Gentlemen were proposed by the Council for Election as Foreign Members, and it was announced that they would be balloted for at the next Ordinary Meeting of the Society, viz.:—

César Mansuete Despretz, of Paris.
Franz Ernst Neumann, of Königsberg.
Ernst Heinrich Weber, of Leipsic.

The following communications were read:—

I. "Letter to the President from Mr. William Lassell, F.R.S., dated Malta, May 13, 1862, giving an account of Observations made with his large Equatorial Telescope." Received May 22, 1862.

9 Piazza Stierna, Malta,
May 13, 1862.

Dear General Sabine,—I have ventured to think that a word of my proceedings may be acceptable to you, though I have been much more tardy in getting into observing order than I had expected. It is indeed only now that I am able to make observations without finding some one part or other of my apparatus capable of improvement. At length, however, I find my hopes exceeded in the perfection, precision, and facility with which my colossal equatorial is directed and carried on: the driving motion is indeed as perfect and uniform, I believe, as that of any telescope with which I am acquainted. For the luxury of observing two assistants are necessary,

when the observer has really nothing to do but keep his eye at the telescope.

We have passed through what was called, for this climate, an unusually cloudy winter, and it is only now that the weather is becoming settled for the summer, and only now that I may be said to be entering upon regular work. I have indeed carefully observed some of Lord Rosse's nebulæ, and in at least two or three instances can fully confirm the spiral character attributed to them by his Lordship,—not, I think, when the objects are well seen, to be overlooked, even when the mind is not previously possessed with the idea. I am making careful drawings of these nebulæ as I see them, some of which closely resemble Lord Rosse's, while others are so different as to suggest (with the fact of the lost nebula in our remembrance) the idea of a real change of form. With new objects, however, of so much delicacy it is necessary to survey them again and again, under different circumstances, in order to arrive at a trustworthy conclusion.

One object, on which I scarcely intended to bestow any attention, has fascinated me greatly—I allude to the moon, in which I see minute details with a hardness and sharpness and reality I have never seen before. My opportunities of scrutiny have, however, been fewer than might have been supposed, from my having frequently been engaged in showing this very popular object to many visitors. Yet, notwithstanding that I have thus been able to see more into the moon than ever before—so much so that I believe, if a carpet the size of Lincoln's Inn Fields were laid down upon its surface, I should be able to tell whether it was round or square,—I see nothing more than a repetition of the same volcanic texture—the same cold, crude, silent and desolate character which smaller telescopes usually exhibit.

Saturn is just now an object of much less physical beauty than when I was here in 1852. I observed, however, on the 15th of April, the passage of Titan on to the disk of the planet, near the northern limb, a phenomenon which of course can only be observed in or near the present position of the ring, and therefore interesting from its rarity.

With respect to the climate, I have not yet used this telescope in its most favourable season. In 1852 I may be said to have gauged the purity of the sky during the Indian summer with an aperture of two feet; now I have been gauging it during a less favourable season

with a four-foot aperture; and therefore it is no wonder if I find nights of the requisite degree of tranquillity somewhat more rare. Yet I find my own physical strength insufficient to allow me to use up half the quantity of available sky, and my next want will probably be some efficient and energetic assistance in the duty of observing.

To General Sabine, I remain, &c.,
President of the Royal Society, &c. Wm. Lassell.

II. "On the Theory of the Motion of Glaciers." By William Hopkins, Esq., F.R.S. Received April 14, 1862.

(Abstract.)

Almost all the numerous discussions which have taken place during the last twenty years respecting our theories of glacial motion have had for their object the assertion of some particular view, rather than the establishment of a complete and sufficient theory founded on well-defined hypotheses and unequivocal definitions, together with a careful comparison of the results of accurate theoretical investigation with those of direct observation. Each of these views has been regarded, improperly, in the author's opinion, as a *Theory of Glacial Motion*. The Expansion Theory ignored the Sliding Theory, though they were capable of being combined; the latter theory was equally ignored by the Viscous Theory, in which, moreover, instead of the definitions of terms being clear and determinate, no definition of *viscosity* was ever given, though that term designated the fundamental property on which the views advocated by this theory depended. Again, the Regelation Theory is not properly a theory of the motion of glaciers, but a beautiful demonstration of a property of ice, entirely new to us, on which certain peculiarities of the motions of glaciers depend.

When we shall have obtained a *Theory of the Motion of Glaciers* which shall command the general assent of philosophers, no qualifying epithet will be required for the word *theory*; it would indeed be inappropriate, as seeming to indicate the continued recognition of some rival theory. If, for instance, it should be hereafter admitted that the sliding of a glacier over its bed and the property of regelation in ice are equally necessary, and, when combined, perfectly

sufficient to account for the phenomena of glacial motion, there would be a manifest impropriety, not to say injustice, in selecting either of the terms *sliding* or *regelation* by which to designate this combined theory. The author makes these remarks because he believes that the preservation of the partial epithets above mentioned has a tendency to prevent our regarding the whole subject in that more general and collective aspect under which it is one of the principal objects of this paper to present it.

This object must necessarily give to the present paper something of the character of a *résumé* of what has hitherto been done, whether it be our purpose to adopt or reject the conclusions of others. There are periods in the history of almost every science when its sound and healthy progress may almost as much demand the refutation of that which is erroneous as the establishment of that which is true. It is not intended, however, to enter into any review of the past labours of glacialists with respect to exploded theories, but only to notice those more recent researches and speculations which appear either to demand refutation as erroneous, or to be admitted into any well-founded theory as correct.

With a view, in the first place, to remove the ambiguities which have beset this subject from the want of explicit definitions, the author enters into the following discussion and explanation of terms employed to express properties of ice on which our theories of glacial motion must essentially depend.

1. The external forms of all bodies in nature may be changed in a greater or less degree, and without producing discontinuity in their mass or destruction of their internal structure, by the action of any external forces, the original or undisturbed form from which the change of form is to be estimated being that which the body would assume if acted on by no external forces whatever. This change of form necessarily implies a change in the relative positions of the component particles of the mass, or a certain greater or smaller amount of *molecular mobility*, or power in the particles of moving *inter se*. We may speak either of the general change of the form of the whole body, or of that which takes place in each of its small elementary portions; it is, in fact, in this latter sense that we are obliged to regard it in any accurate investigations, because the change of form for different elements will usually be different. Change of

form in an element may or may not be accompanied by a change of its volume. In the first case it leads to *cubical* extension or compression; in the latter, merely to extension or compression of the surface and not the volume of the element. It may be called *superficial* extension or compression. These changes of volume and form in any element must be produced by the forces acting on it. Thus we may conceive linear extension alone produced at any interior point of the mass by two equal and opposite tensions acting on two elementary component particles there in the direction of the line joining their centres of gravity, while compression alone would result if those tensions were changed into pressures. In such cases extension or compression would be the result of forces which may be called *direct* or *normal* forces. In the case above mentioned, in which the volume and density of every element of the mass remain unaltered, there can be no such direct normal action as that just mentioned. It must be perpendicular to the normal action, and therefore a *transversal* or *tangential* action. There would be no tendency to make the contiguous particles approach to or recede from each other, but to cause the one to *slide* tangentially past the other.

If the body have a structure like that of any hard, vitreous or crystalline mass, pressure at any point will tend to break or crush the body, and thus to destroy the continuity of its structure. This tendency will be opposed by the *resisting power* of the substance. The tendency of the direct or normal tension is to separate the contiguous particles, and thus produce a finite fissure, or a discontinuity in the mass. It is resisted by the *normal cohesive power*; and in like manner the transverse or tangential action is resisted by the *tangential cohesion*, or that which prevents the component particles from sliding past each other. Again, when the component particles at any point of a body are relatively displaced, they have always a certain tendency to regain their originally undisturbed position, and the force thus excited, considered with reference to the force of displacement at that point, affords a measure of what is called the *elasticity* of the body. Since the force of restitution may vary from zero to the corresponding force of displacement, the elasticity, when measured by their ratio, may vary from zero to unity.

2. We may now define such terms as *solid, plastic, viscous,* and the like, with all the accuracy which their definitions admit of. We

may call a body emphatically a *solid* body when it possesses the following properties:—(1) small extensibility and compressibility, (2) great power of resistance and great cohesive power, both normal and tangential, and (3) great elasticity. It will thus require a comparatively great force to produce any sensible relative displacement among the constituent molecules of the body; if we conceive the force required to become infinitely great, we arrive at absolute *rigidity* as the limit of solidity. Again, we shall best, perhaps, define *plasticity* or *viscosity*, if we suppose the forces of displacement to be such as to produce only a small transverse or tangential displacement of the constituent particles, *i. e.* a superficial, not a cubical, extension or compression. Then if the force of restitution bear only an inappreciable ratio to the corresponding force of displacement, *i. e.* if the *tangential elasticity* be not of sensible magnitude, the mass may be emphatically said to be *plastic*. This is the essential condition of what may with strict propriety be termed *plasticity*; it might also be added that, as bodies are constituted in nature, the force required to produce the original displacement in plastic bodies will be small as compared with that required in solid bodies. Viscosity and semi-fluidity are terms which only express similar properties of bodies, but usually indicating that still smaller forces only are required to produce a given displacement in such bodies than in plastic ones. The limiting case is that of perfect fluidity, in which both the forces of original displacement and those of restitution are indefinitely small. In these latter cases the tangential cohesion is necessarily small, and such also (as bodies are usually constituted) will be the normal cohesion. At the same time the power of resisting compression of volume may be very great, as in fact it is in nearly all masses not technically designated as *elastic masses*. In other words, the *normal* elasticity, with reference to pressure, may be of any magnitude, while the *tangential* elasticity equals zero.

It will be observed that a body is here spoken of as held in a state of constraint by internal forces, but without any kind of dislocation which should destroy its continuity or injure its structure. If, however, the external forces should be sufficiently increased, the structure of a vitreous or crystalline mass, or that of any mass possessing hardness and brittleness, will be destroyed by a pressure greater than its power of resistance can withstand; or the continuity of its mass will

be destroyed by any normal tension greater than the normal cohesion; or, again, by any tangential tension greater than the tangential cohesion. The normal tension would thus produce an open fissure; and the tangential tension would cause one particle of the mass to slide past another, but without producing any open discontinuity. On the contrary, in a properly plastic or viscous mass there is no definite structure for excessive pressure to destroy; there is no question as to the formation of open fissures; and the characteristic absence of *tangential* elasticity allows of any amount of change in the relative positions of the constituent particles of the mass without breach of its continuity.

It would of course be impossible to draw an exact and determinate line of demarcation between solidity and plasticity, but it is not therefore the less certain that there are bodies which do unequivocally possess the property of solidity, and others which do as unequivocally possess the property of plasticity, according to the definitions here given of these terms. Solidity and plasticity with respect to numerous cases in nature thus become determinate properties of those aggregates of material particles which we call bodies. Ice, a vitreous or crystalline and brittle mass, which will neither bear any but the smallest extension without breaking, nor more than the smallest compression without being crushed, must be solid, and cannot be plastic, if we are to use those terms as significant of determinate properties of bodies.

3. The advocates of the viscous theory would not probably admit the necessity of the above rigorous definition of the term viscous in its application to glacier ice. But the defect of that theory has always been in the entire want of any accurate definition of that term. When such definition was demanded, it was said that glacier ice must be viscous, because a glacier adapted itself to the inequalities of its valley as a viscous mass would do. This was equivalent to saying that the mass was viscous because it moved in a particular manner, instead of asserting that the mass moved in that particular manner because it was viscous. Now this kind of inversion of the direct enunciation of the proposition is only admissible when there is no other physical cause than the one assigned, to which it is conceivable that the observed phenomena should be ascribed. Thus we may assert with perfect conviction, that gravity exists as a property of

matter and acts according to a certain law, because the bodies of the solar system move as if such were the case; but the conclusiveness of this inductive proof of the proposition—that "gravity is a property of matter"—rests entirely on our conviction that matter has no other property by which we could equally account for the phenomena of the celestial motions. And so with regard to glaciers. If viscosity were the only conceivable property of ice by which we could possibly account for the observed motion of glaciers, then would the observed phenomena of that motion perfectly convince us of the existence of the property in question. But here the two cases entirely differ, inasmuch as there was no general conviction, nor even a decided probability at the time I allude to, that no physical property of ice could exist besides viscosity which might account for the observed phenomena of a glacier's motion; and at the present time it is proved that there *is* another property of ice by which those phenomena are perfectly accounted for, and the inductive proof becomes altogether valueless. Moreover, in the case of universal gravitation, the inductive proof is the only possible one, whereas in glacier motion we are concerned with a property which, in whatever sense the definition of it may be regarded, must be as capable of being rendered patent by experiment in ice, if it exist, as in any other substance.

The answer, then, that was given to the question—what is viscosity?—comprised no definition at all of that term. The viscous theory ignored the possibility of the molecular mobility of a glacial mass united with the preservation of its continuity, being attributable to any other property than that which was designated as *viscosity*, but without giving any exact definition of the term. If it was meant to define by it the property which is here defined by the same terms, the theory had a legitimate claim to be considered a *physical* theory, because it assigned a determinate physical property as the cause of certain observed phenomena. In this sense, however, the author conceives that it would now be admitted to be entirely disproved by Professor Tyndall's experiments, in which the ice exhibits so clearly the property of solidity, and the absence of all indication of plasticity. It may be presumed that the hypothesis of viscosity could only have been adopted in the first instance from the apparent absence of any other property of ice which might account equally well for the molecular mobility of the glacial mass.

4. But if the determinate property of viscosity, as here defined, be not recognized in ice, what, it will be asked, is really the idea which has been attached to the term plastic or viscous? The question, as already observed, is difficult to answer. Perhaps the best way of doing so is to refer to the *Prefatory Note* to Principal Forbes's 'Occasional Papers' (p. xvi). He there intimates that the expressions "bruising and re-attachment," and "incipient fissures re-united by time and cohesion," used by him in 1846, are to be regarded as having the same meaning as the expression "fracture and regelation," first introduced into the subject in 1857. Now there is no ambiguity whatever in this latter expression. "Fracture" means the breaking and splitting of the ice regarded as a brittle and crystalline *solid*, and could never be intended to have the slightest reference to viscosity. In fact the expression is altogether inapplicable to any body which can be called viscous without a violation of scientific language. Still this, it may be said, may be only a want of strict accuracy of expression, rather than of accuracy of conception. But if a notion of cracking and breaking, so foreign to any idea of plasticity, should be admitted, it could not be said that a glacier moved as it is observed to move, because it was plastic, but merely that it moved *as if* it were plastic. The true inference from the motion would have been that glacier ice possessed not necessarily real plasticity, a definite property of bodies, but a *quasi-plasticity*, which expresses no determinate property at all, but may consist with many different properties. It merely expresses, in fact, the power of the component elements of the mass of changing to a certain extent their relative positions. But this is not the peculiar property of ice; it is common, indeed, to all bodies exposed to disruptive forces which, as in the case of ice, the cohesive power is unable to withstand. The mass of any other substance, as well as that of a glacier, will then be broken into fragments sufficiently small to allow it to follow the impulses of the external forces acting on it. To say, therefore, that a glacier moves *as if* it were plastic is not to assign to ice any property peculiar to itself, and therefore does not properly constitute a *physical* theory of glacier motion at all.

5. But if we pass over the difference between true plasticity and that which, as we have pointed out, is merely apparent, there still remained the great difficulty, which was only removed by the experiments of Mr. Faraday and Dr. Tyndall. Every one who believed ice

to be a solid body, believed as a matter of necessity that a glacier must, on account of the external conditions to which it is subjected, be excessively broken and dislocated in the course of its motion. The author was himself one of those who fell into the error of attributing too much influence to the larger and more visible disruptions of the mass; but the great difficulty was in the perfect subsequent reunion of portions which had thus been separated, whether by larger or smaller dislocations. And here it will necessarily be asked whether, in the expressions above quoted, "*re-attachment*" and the "*reunion* by time and cohesion" of separated portions when again brought into contact, really mean the same thing as *regelation*? This question the author thinks can be answered only by saying that, whatever might be the intended meaning of those expressions, they failed to convey to the minds of others the most remote idea of regelation, as a property of ice at a particular temperature. No better proof can be given of this than the general conviction which appeared to flash across the mind of every glacialist when he first heard of Professor Tyndall's experiment, that the recognition of the property of instantaneous regelation was a well-marked and important discovery, which had at once completely removed a great stumbling-block in glacial theory. In fact, the viscous theory assigns no physical cause for the reunion in question. All we could do, before the publication of those experiments, was to infer from the observed facts that ice did possess some property which facilitated the reunion of separate pieces in contact; but this was like the attempt to define viscosity by an appeal to the phenomena which that property was intended to explain. Regelation has, in fact, no connexion with viscosity, but stands in direct antagonism to it.

An *imperfect plasticity* in ice has sometimes been spoken of. The fact is, all solid bodies may be said to have an imperfect plasticity, if we chose to admit this vagueness in scientific language, since all are capable of greater or less extension or compression. As to the apparent plasticity inferred from the motion of glacial masses, and arising from the crevicing of the ice as already explained, it has no relation whatever to real plasticity. Such crevices are the necessary consequences of the external forces acting on the glacier, and are as essential to the theory of regelation as they are unconnected with any property of plasticity.

The author then briefly describes the experiment, by which it is shown that ice will slide down an inclined plane at an inclination to the horizon less than that of any known glacier, provided its lower surface be in that state of disintegration in which it will necessarily be when its temperature = zero (C.). The motion is then slow and uniform. That glaciers do slide over their beds, has been established as clearly as it can be by the comparatively few observations which have been made on the subject; and every existing glacial valley, and every valley which is believed to have been such at former geological periods, testify to the truth of that conclusion. The author also explains that both theory and observation agree in the result that the temperature of the lower surface of a glacier of any considerable depth in the latitude of the Alps must necessarily be = zero (C.). He regards this sliding motion as far too important a part of the whole motion of a glacier to be neglected in any complete theory of that motion.

The author then proceeds to investigate certain properties of the internal tensions and pressures at any point (P) in the interior of a mass held in a state of constraint by external forces. He shows that at every point (P) there are three determinate directions, at right angles to each other, in which the direct tension is such that in one of them it is a maximum, in another a minimum, and in the third neither a complete maximum nor a complete minimum; it is convenient to call this the *mean* axis. The tensions or pressures in these directions are called *principal tensions* or *pressures*; there are also two other directions through P characterized by a peculiar property. If we take two adjoining particles, P and P′, in the line of maximum tension, that tension will exert a greater effort than there will be in any other direction to separate those particles; or if the internal force be the maximum pressure, those points will be more compressed together than in any other direction. In the two directions (now to be defined) the forces on P and P′, acting perpendicularly to the line joining those particles, will exert a greater tendency than is exerted in any other direction, to separate them by making one *slide tangentially* past the other, and then to twist and contort any internal elementary portion of the mass. These two directions are perpendicular to each other, and bisect the angles between the directions of maximum tension and maximum pressure. This problem

is treated entirely mathematically; it is the *typical* problem of this part of the subject. The results are applied to a real glacier by the analogy which it bears to the typical one.

For the application of these analytical results, the author then considers the nature of the forces called into action by the two primary characteristics of the motion of a glacier—that its central move faster than its marginal portions, and the portions near the upper faster than those near the lower surface of the mass. He also takes account of the modifications to which these forces may be subjected by changes of form and inclination in the containing valley. He likewise explains the different modes in which the mass may be fractured when the forces become such as to overpower its powers of cohesion or resistance. If the cohesion give way to the maximum tension, an open fissure must be formed in a direction perpendicular to that tension. If the *resisting power* of the ice give way to the maximum pressure at any point, the mass will be *crushed* at that point, but its continuity will be immediately restored by regelation, the internal constraint will be momentarily removed, and the mass will move on. By a repetition of this process the glacier is enabled to move forward, preserving at once the continuity of its motion, of its mass, and of its structure.

The *veined structure* of glacial ice is then examined, and it is shown that, so far as Professor Tyndall's *pressure theory* of that structure involves the condition of the structural surfaces being perpendicular at each point to the maximum pressure there, it is perfectly accordant with the theoretical results of this paper. Whether the structure be marginal, longitudinal and central, or transversal, this is equally true, assuming always that the structure in each locality is the direct and immediate consequence of the forces acting there and tending to produce it. Probably, however, the veined structure in one locality may have originated in another from which it has been *transmitted* by the motion of the glacier. Supposing this to be so entirely, the author examines how this motion of transmission would modify the forms of the transmitted structure. Practically, and within the limits to which observation has yet extended, these modifications would produce forms sensibly coincident with those which would result, as in the previous case considered, from the immediate action of the forces, independently of transmission. The respective effec-

tiveness of these two causes, therefore, in producing the veined structure in any particular locality is not at present determined. Its determination would require more accurate and detailed observations than have yet been made on this subject.

The *differential theory* of the veined structure is then considered; but here the author dissents entirely from all Professor Forbes's mechanical reasoning, by which he professes to determine the positions of the surfaces of maximum differential motion, which, according to this theory, are coincident with the structural veins. Mr. Hopkins contends that the actual differential motion of two contiguous particles must necessarily take place in the common direction of their motions. He cannot understand the effectiveness of such motion in any other sense, in producing the phenomena in question. He has investigated for this case the forms of the veined surfaces, but finds them altogether different from the observed forms; and with respect to Prof. Forbes's investigation he cannot possibly admit it, as he at present understands it.

The author then examines the *intensity* of the *dislocating forces* acting on the glacier. He demonstrates Prof. Forbes's error in supposing that it is much augmented by an enormous hydrostatic pressure within the mass, tending to push it onward in the direction in which it may be most free to move. It is proved that, under the existing conditions of a glacier, the hydrostatic pressure from the water contained in the pores of the mass can but little exceed the atmospheric pressure on its surface. But Mr. H. shows that there must in many localities be a very large increase in the intensity of the internal tensions and pressures arising from the free sliding motion of the whole glacier. Where the motion of a particular part of the mass is retarded by local circumstances, there will probably be an enormous pressure upon it *à tergo*, from the mass behind; or there may, in other cases, be a great additional tension, arising from the freer motion of the mass in front. Hence the dislocating forces must often be greatly increased, the dislocation is ensured, and the operation of regelation brought into action; and the continued motion of the glaciers is preserved when it might otherwise be arrested.

III. "Experiments on Food; its Destination and Uses." By William S. Savory, Esq., F.R.S. Received May 1, 1862.

(Abstract.)

The experiments which are related in this paper refer to the destination of food and its uses.

Abundant, nay, superfluous evidence has been furnished to prove that no one principle of food will alone suffice for nutrition; but clear and unequivocal evidence is still wanting to show how far each principle of food is essential to life and health, provided all else save that one be sufficiently supplied. This is a very different question.

Again, ever since Liebig's famous classification of food into plastic or nutritive and respiratory or calorifacient, some most important questions in connexion with it have engaged the attention of physiologists. Amongst them are these:—

Is any food destined to the production of heat without being concerned in the repair of the tissues—that is, is any portion of the food directly burnt in the blood?

Is any portion of albuminous food directly calorifacient, that is, burnt in the blood without forming tissue?

This last question has more recently assumed another form, viz. what is the source of urea? Is it derived wholly from the metamorphosis of tissues, or directly to some extent from the blood? In other words, does any portion of nitrogenous food undergo a directly retrograde metamorphosis into urea, carbonic acid, and water?

The experiments were performed upon rats* and a hawk. The animals were fed upon different diets, and the experiments may be divided into three classes accordingly. In one class the diet was a non-nitrogenous one, consisting of equal parts by weight of arrowroot, sago, tapioca, lard, and suet; for this mixture was found upon analysis to yield only ·22 per cent. of nitrogen. In another class the diet was a nitrogenous one. It consisted of lean veal from which every visible particle of fat had been carefully removed. This yielded upon analysis only 1·55 per cent. of fat. In the third class the diet was a mixed one. It consisted of a combination of the two former diets.

* Rats were chosen as subjects for these experiments because they are omnivorous and will readily feed on almost any kind of diet. Moreover from their size they are very convenient to manage.

The weight, temperature, and general condition of the animals were especially noticed, and in some cases the urine was collected and the amount of nitrogen it contained determined.

From these experiments the following conclusions are drawn:—

Nitrogenous materials are not only calorifacient, but, at least under some circumstances, sufficiently so to maintain alone the requisite temperature.

It is in the highest degree probable that, under certain circumstances, nitrogenous materials may prove directly calorifacient without forming tissue.

Non-nitrogenous substances are, at least under some circumstances, directly calorifacient without entering into the composition of tissue of any kind.

While non-nitrogenous food only is taken, all the nitrogen which is excreted in the urine, and more, may be accounted for by the disintegration of the original tissues, without assuming that any fraction is assimilated from any other source.

While life cannot be maintained without nitrogenous food, even though every other kind be abundantly supplied, death in this case being due to loss of tissue, life and even health and the normal temperature can be maintained, at least for a long period, upon a diet almost exclusively nitrogenous, with proper inorganic substances in which there exists only a small fraction of non-nitrogenous matter. Such a minute proportion of fat must be but a poor representative of non-nitrogenous food.

Moreover in these experiments some of the rats sustained a loss of weight considerably above 50 per cent.

The difference in this respect between former experiments and mine may be, perhaps, in some measure accounted for by considering the immediate cause of death in the former ones. Chossat satisfactorily showed that the subjects of his experiments died from cold. In my experiments, the animals being freely supplied with calorifacient food, this cause of death was for a while averted, so that time was allowed for a further disintegration of tissue.

When their temperature is maintained from external sources, or when they are freely supplied with calorifacient food, warm-blooded animals may die rather from waste than loss of temperature, as perhaps is the case with cold-blooded animals when they are starved.

Lastly, in these experiments the significant fact appeared, that while the weight, strength, and general condition of the animals varied very widely under the different diets to which they were subjected, no considerable fluctuation was observed in their temperature. Even the slight variation from time to time recorded seemed rather to result from other causes than to depend directly on the food.

IV. "On a New Series of Compounds containing Boron." By Dr. EDWARD FRANKLAND, F.R.S. Received May 15, 1862.

(Abstract.)

This paper contains the full details of the author's researches on boric ethide—a body partially described by Mr. B. F. Duppa and the author in the 'Proceedings of the Royal Society,' vol. x. p. 568, and also their extension to the homologous compound containing methyl.

Boric ethide combines with ammonia with great energy; if a few drops of boric ethide be passed up into a dry eudiometer filled with mercury, and dry ammoniacal gas be then admitted into the same tube, each bubble of gas collapses with a shock like that produced by a bubble of steam projected into cold water. The analysis of the body thus formed leads to the formula

$$NH_3 + B(C_4H_5)_3.$$

Ammonia boric ethide is a somewhat oily liquid possessing an aromatic odour and an alkaline reaction. It cannot be distilled, except *in vacuo*, without decomposition. Carbonic acid has no action upon it, even in the presence of water, but other acids decompose it instantly and liberate boric ethide. Exposed to atmospheric air, ammonia boric ethide scarcely absorbs a perceptible amount of oxygen, even after the lapse of several hours.

The author considers boric ethide to be formed from boracic ether and zincethyl by the substitution of the ethyl in zincethyl for the oxygen in boracic acid,

$$\underbrace{2B\left\{\begin{matrix}O\\O\\O\end{matrix}\right. + 3\left.\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}O_2}_{\text{Boracic ether.}} + \underbrace{3Zn_2\left\{\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right.}_{\text{Zincethyl.}} = \underbrace{2B\left\{\begin{matrix}C_4H_5\\C_4H_5\\C_4H_5\end{matrix}\right.}_{\text{Boric ethide.}} + \underbrace{6\left.\begin{matrix}C_4H_5\\Zn\end{matrix}\right\}O_2}_{\text{Ethylate of zinc.}}.$$

Another, but far less probable, view of the reaction presents itself in the supposition that the three atoms of ethyl in boric ethide were already present in the boracic ether, the action of the zincethyl being simply to remove the whole of the oxygen from the boracic ether. Kekulé has, in fact, unreservedly adopted this latter view of the reaction.

So long as the organic radical of the zinc compound and that of the boracic ether are identical, it is impossible to prove whether the three individual atoms of ethyl in boric ethide were originally present in the boracic ether, or have been derived from the zincethyl. Indicating by an asterisk the atoms of ethyl which finally become part of the boric ethide, it is impossible to prove conclusively whether the reaction takes place according to the first or second of the following equations:—

$$(1)\quad 2\,B\left\{\begin{array}{l} C_4\,H_5\,O_2 \\ C_4\,H_5\,O_2 \\ C_4\,H_5\,O_2 \end{array}\right. + 3\,Zn_2\left\{\begin{array}{l} C_4\,H_5{*} \\ C_4\,H_5{*} \end{array}\right. = 2\,B\left\{\begin{array}{l} C_4\,H_5{*} \\ C_4\,H_5{*} \\ C_4\,H_5{*} \end{array}\right. + 6\left.\begin{array}{c} C_4\,H_5 \\ Zn \end{array}\right\}O_2.$$

$$(2)\quad 2\,B\left\{\begin{array}{l} C_4\,H_5{*}\,O_2 \\ C_4\,H_5{*}\,O_2 \\ C_4\,H_5{*}\,O_2 \end{array}\right. + 3\,Zn_2\left\{\begin{array}{l} C_4\,H_5 \\ C_4\,H_5 \end{array}\right. = 2\,B\left\{\begin{array}{l} C_4\,H_5{*} \\ C_4\,H_5{*} \\ C_4\,H_5{*} \end{array}\right. + 6\left.\begin{array}{c} C_4\,H_5 \\ Zn \end{array}\right\}O_2.$$

Although we cannot thus label, as it were, the atoms taking part in the reaction, we can unerringly trace the movements of the alcohol radicals, if we secure their identification by varying their composition in the two compounds used in the reaction. The study of the action of zincmethyl upon boracic ether would obviously thus decide between these views. If boric ethide were produced from these materials, Kekulé's hypothesis would be established; but if, on the other hand, boric methide were the result of the reaction, then the correctness of the view originally taken by Mr. Duppa and the author would be proved to be correct. The following results were obtained in pursuing this inquiry:—

Boric Methide.

When a strong ethereal solution of zincmethyl is added to boracic ether, an elevation of temperature to the extent of 8° or 10° Cent. is observed, whilst at the same time a most intensely pungent odour is developed; this odour, although it resembles that of boric ethide, is far more powerful and more persistently irritating to the mucous

membrane. A slow evolution of spontaneously inflammable gas, burning with a splendid green flame, was also noticed ; and this evolution of gas became more rapid when the warmth of the hand was applied to the flask containing the ingredients. Purified and submitted to analysis, the gas exhibited a composition agreeing with the formula

$$B \left\{ \begin{array}{l} C_2 H_3 \\ C_2 H_3 \\ C_2 H_3. \end{array} \right.$$

Boric methide is produced from boracic ether and zincmethyl by the following reaction :—

$$\underbrace{2\,B \left\{ \begin{array}{l} C_4 H_5 O_2 \\ C_4 H_5 O_2 \\ C_4 H_5 O_2 \end{array} \right.}_{\text{Boracic ether.}} + \underbrace{3\,Zn_2 \left\{ \begin{array}{l} C_2 H_3 \\ C_2 H_3 \end{array} \right.}_{\text{Zincmethyl.}} = \underbrace{2\,B \left\{ \begin{array}{l} C_2 H_3 \\ C_2 H_3 \\ C_2 H_3 \end{array} \right.}_{\text{Boric methide.}} + \underbrace{6 \left. \begin{array}{c} C_4 H_5 \\ Zn \end{array} \right\} O_2}_{\text{Ethylate of zinc.}}.$$

The formation of boric methide under these circumstances proves conclusively that the corresponding ethyl compound is formed, not by the removal of the whole of the oxygen from boracic ether, but by the actual substitution of the three atoms of oxygen in boracic acid by three atoms of ethyl ; whilst boric methide is in like manner produced by the similar substitution of methyl for oxygen, which is quite in harmony with the mode of formation of very numerous compounds in the organo-metallic family.

Boric methide exists at ordinary temperatures as a colourless and transparent gas possessing a peculiar and intolerably pungent odour, irritating the mucous membrane and provoking a copious flow of tears. Its specific gravity is 1·93137. It retains its gaseous condition when exposed to a cold of $-16°$ Cent. ; but at 10° Cent., and under a pressure of three atmospheres, it condenses to a colourless, transparent, and very mobile liquid. It is very sparingly soluble in water, but very soluble in alcohol and in ether. In contact with atmospheric air, it takes fire spontaneously, burning with a bright green flame, which is very fuliginous if the volume of the flame be considerable. If the gas issue into the air through a tube $\frac{1}{10}$th of an inch in diameter, the amount of smoke is surprisingly great ; 2 or 3 cubic inches of gas, when consumed in this way, filling the atmosphere of a large room with large comet-like flocks of carbonaceous matter. This curious phenomenon is probably due, in part at least, to the forma-

tion of a superficial coating of boracic acid, which envelopes the particles of carbon and prevents their combustion. Suddenly mixed with atmospheric air or oxygen, boric methide explodes with great violence. In contact with atmospheric air, both boric methide and the vapour of boric ethide exhibit two distinct kinds of spontaneous combustion. Thus when these bodies issue very slowly from a glass tube into the air, they burn with a lambent blue flame invisible in daylight, and the temperature of which is so low that a finger may be held in it for some time without much inconvenience. Under these circumstances partial oxidation only takes place, and it is to the products thus formed that the peculiar pungent odour of boric ethide and boric methide is due. When, on the other hand, these bodies issue into the air more rapidly, the lambent blue and nearly cold flame changes to the green and hot flame above mentioned. I have not examined the spectra of the two differently-coloured flames from the same compound; but they doubtless present a widely different appearance, thus affording another instance of the dependence of the spectra of bodies upon temperature,—a phenomenon to which I recently called attention in the case of lithium*.

Boric methide is not acted upon by binoxide of nitrogen or by iodine. Solution of bichromate of potash scarcely affects it, but the addition of concentrated sulphuric acid at once determines the reduction of the chromic acid. When boric methide is allowed to bubble through water into chlorine, each bubble burns explosively with a bright flash of light and the separation of carbon: it has no tendency to unite with acids. Concentrated sulphuric acid has no action upon it; when mixed with hydriodic acid gas, it suffers no change; but, on the other hand, it is freely absorbed by solutions of the fixed alkalies and by ammonia. If a very rapid current of the gas mixed with half its volume of marsh-gas be passed through a stratum of strong solution of ammonia only half an inch deep, not a trace of boric methide escapes absorption.

Ammonia Boric Methide.

When dry ammoniacal gas is mixed with an equal volume of dry boric methide, both gases instantly disappear, with the evolution of a considerable amount of heat and the production of a white, volatile,

* Phil. Mag. Dec. 1861, p. 472.

crystalline compound. The latter is also formed when boric methide is passed into solution of ammonia. The colourless liquid stratum which forms upon the surface soon solidifies when it is placed over sulphuric acid *in vacuo*. A quantity of the compound obtained by this latter process was purified by solution in ether and subsequent recrystallization: on being submitted to analysis, it yielded numbers corresponding to the formula

$$NH_3 + B(C_2H_3)_3.$$

Ammonia boric methide is deposited from its ethereal solution in magnificent arborescent crystals, which rapidly volatilize without residue when exposed to the air. They possess a caustic and bitter taste and a very peculiar odour, in which both the smell of ammonia and that of boric methide can be recognized. Ammonia boric methide fuses at 56° Cent., and boils at about 110° Cent. In a current of air, or better of carbonic acid, it sublimes at a very gentle heat and condenses in arborescent crystals. Several determinations of the specific gravity of the vapour of ammonia boric methide gave the mean number 1·253, which indicates that the vapour of ammonia boric methide consists of equal volumes of boric methide and ammonia united without condensation. Thus the formula of ammonia boric methide is a four-volume formula*—a state of condensation which is usually considered to be abnormal, and which, where it occurs, is generally explained by the assumption of a decomposition of the body at the moment of conversion into vapour. The proof of the disunion or integrity of the vaporous molecule of ammonia boric methide would be interesting in connexion with these so-called anomalous vapour-densities, but the author regrets his inability to offer any sufficiently decisive solution of this problem; for although fused chloride of copper absorbs ammonia from the vapour, yet it does so under circumstances which admit of the assumption that the vapour of ammonia boric methide is *decomposed* by the chloride of copper.

Ammonia boric methide scarcely absorbs a perceptible amount of oxygen at ordinary temperatures, even after several days' exposure to the gas; but it takes fire below 100° Cent. when heated in contact with the air. Its vapour is also very inflammable; thus when ammonia boric methide is placed under the receiver of an air-pump, and

* $H_2O_2 = 2$ vols.

the air is being withdrawn, the explosion of the mixture of air and vapour in the cylinders of the pump is frequently determined by the rise of temperature consequent upon the depression of the pistons when the rarefaction has become considerable.

Boric methide is also absorbed by aniline with great avidity. Acids expel the gas from this compound unchanged.

Terhydride of phosphorus has no action upon boric methide. A mixture of equal volumes of the two gases is spontaneously inflammable, burning with a yellowish-white flame, in which the characteristic green tinge attending the combustion of boric methide is no longer perceptible.

Compounds of Boric Methide with Potash, Soda, Lime, and Baryta.—Solution of caustic potash absorbs boric methide with great energy. The saturated solution, exposed over sulphuric acid *in vacuo,* dries down to a gummy mass, which scarcely exhibits signs of crystallization. The same body may be more conveniently formed by decomposing ammonia boric methide with alcoholic solution of potash, taking care to employ an excess of the former. On evaporation over sulphuric acid *in vacuo,* the excess of the ammonia compound volatilizes and is decomposed by the sulphuric acid, with the elimination of boric methide; thus the solution of the potash compound evaporates in an atmosphere of boric methide. Nevertheless, even by this method the potash compound could not be obtained in a state of perfect purity, the numbers obtained on analysis indicating only remotely the formula

$$KO + B\,(C_2\,H_3)_3.$$

Boric methide is also readily absorbed by solution of neutral carbonate of potash, bicarbonate of potash and potash boric methide being apparently formed. Although boric methide and potash unite with remarkable energy, they are separated by acids with the greatest readiness; even carbonic acid in the presence of water can expel boric methide from its potash compound; thus if an aqueous solution of potash boric methide be passed into carbonic acid standing over mercury, the acid gas soon becomes replaced by pure boric methide.

Soda boric methide, baryta boric methide, and lime boric methide are similar bodies produced by the absorption of boric methide gas by caustic solutions of soda, baryta, and lime; they are all readily soluble in water and react alkaline.

Boric methide in combination with the alkalies and alkaline earths has almost entirely lost its powerful affinity for oxygen; nevertheless, when these bodies are placed in contact with a known quantity of oxygen over mercury for several days, the volume of the gas perceptibly diminishes.

The great difficulty, not to say danger, attending the gradual oxidation of considerable quantities of a gaseous and spontaneously inflammable body like boric methide has prevented the author from following this compound into its products of oxidation, as was done in the case of boric ethide. With a graduated supply of oxygen, however, boric methide appears to comport itself like boric ethide, and the compounds formed are probably homologous with diethylate and dihydrate of boric dioxyethide.

In conclusion, it can scarcely be doubted that the action upon boracic ether of the zinc compounds of the remaining alcohol radicals would produce the homologues of the bodies described in the foregoing pages. It may also be remarked that the existence of bodies like boric dioxyethide, in which one-third of the oxygen in boracic anhydride is replaced by ethyl, altogether abolishes any supposed analogy between carbonic and boracic acids, whilst it proves that the composition of the latter acid is expressed by the formula BO_3, or some multiple of that formula.

V. "On the Constitution of Sea-Water, at different Depths, and in different Latitudes." By George Forchhammer, Ph.D., Professor of Mineralogy in the University of Copenhagen. Communicated by the President.

Professor Forchhammer was present at the Meeting, and, by request of the President, gave a statement of the principal results of his researches. He first, however, took occasion to express his great satisfaction in being allowed the opportunity of personally and gratefully acknowledging the liberality with which men of science in this country had entered into his views and supplied him with specimens requisite for carrying on his inquiries; and he particularly mentioned the name of a late distinguished Fellow of this Society, Sir James Clark Ross, who had kindly furnished various samples of sea-water procured in his Antarctic voyage.

The number of elements hitherto found in sea-water the author stated to be thirty-one, viz. *Oxygen, Hydrogen, Azote* in ammonia, *Carbon* in carbonic acid, *Chlorine, Bromine, Iodine* in fuci, *Fluorine* in combination with calcium, *Sulphur* as sulphuric acid, *Phosphorus* as phosphoric acid, *Silicium* as silica, *Boron* as boracic acid, discovered by the author both in sea-water and in sea-weeds, *Silver* in the *Pocillopora alcicornis, Copper* very frequent both in animals and plants of the sea, *Lead* very frequent in marine organisms, *Zinc* principally in sea-plants, *Cobalt* and *Nickel* in sea-plants, *Iron, Manganese, Aluminium, Magnesium, Calcium, Strontium* and *Barium,* the latter two as sulphates in fucoid plants, *Sodium, Potassium.* These twenty-seven elements the author himself had ascertained to occur in sea-water; the presence of the next four elements, viz. *Lithium, Cæsium, Rubidium,* and *Arsenic,* has been shown by other chemists.

Of these elements only a few occur in such quantity that their determination has any notable influence on the quantitative analysis of sea-water, viz. Chlorine, Sulphuric acid, Magnesia, Lime, Potash, and Soda. The others, as far as their existence has been determined in the sea-water itself, are found in the residue which remains after evaporation to dryness and redissolution of the salts in water.

The author next stated that in the water of the ocean far from the shores the principal ingredients always occur very nearly in the same proportions. If we assume chlorine=100, the mean proportion of the other leading constituents is as follows :—

	Mean proportion.	Maximum.	Minimum.
Sulphuric acid ..	11·89	12·09	11·65
Lime	2·96	3·16	2·87
Magnesia........	11·07	11·28	10·95
All salts	181·1	181·4	180·6

These proportions apply only to specimens obtained at a long distance from shores, or in the open ocean. In the interior of the Baltic, for instance, the proportion of chlorine to sulphuric acid is as 100 to 14·97—to lime as 100 to 7·48; and the proportion of chlorine to all salts as 100 to 223·0. This constant proportion of the different constituents in the ocean depends evidently not upon any chemical combination and affinity between the different substances, but upon the enormous quantity of salts in the whole ocean, which

renders imperceptible any difference that might otherwise arise from the different proportion in which salts are carried into the sea by rivers. It depends, besides, on the uniform action of the numberless organic beings inhabiting the ocean which abstract sulphuric acid, lime, potash, and magnesia from the water, and render them insoluble.

The mean quantity of solid matter in the water of the ocean generally, the author found to be 34·304 per 1000. To determine this mean quantity he has divided the ocean into regions, viz. :—

1st Region. Atlantic, from the Equator to 30° N. lat.; mean 36·169.

2nd Region. Atlantic, from 30° N. lat. to a line from the north of Scotland to the north of Newfoundland; mean 35·976.

3rd Region. From the northern boundary of region 2 to the south coast of Greenland; mean 35·556.

4th Region. Davis's Strait and Baffin's Bay; mean 33·167.

5th Region. Atlantic, between 0 and 30° S. lat.; mean 36·472.

6th Region. Atlantic, between 30° S. lat. and a line from the southernmost point of Africa to the southernmost point of America; mean 35·038.

7th Region. Between Africa and the East Indian Islands; mean 33·868.

8th Region. Between the East Indian and the Aleutic Islands; mean 33·506.

9th Region. Between the Aleutic and the Society Islands; mean 35·219.

10th Region. The Patagonian stream of cold water; mean 33·966.

11th. The Antarctic region; mean 28·563.

Besides these regions of the great ocean, the author enumerates some other regions, which are under the decided influence of the surrounding land. Such are the North Sea, with a mean quantity of solid matter of 32·806 per 1000; the Kattegat and Sound, with a mean of 15·126; the Baltic, mean 4·807; the Mediterranean, mean about 37·5; the Black Sea, mean 15·894. Of the proportion in the large bays of America the author had only one observation, viz. in water from the Caribbean Sea, in which the quantity of saline matter was found to be 36·104 per 1000.

The author then showed that the equatorial regions contain the

greatest percentage of saline matter, and that this peculiarity is owing to the evaporation under and in the neighbourhood of the line being greater than the quantity of water supplied by the rain falling on the sea and by the rivers flowing from the land; that the equilibrium is maintained by polar currents, which bring water with less saline matter to the equatorial regions. The mean quantity of saline ingredients in the equatorial regions of the ocean is about 36·2 per 1000, while in the polar regions it is about 33·5.

The North Atlantic Ocean contains much more salt than the South Atlantic, which the author explains by the prevailing influence of the Gulf-stream; and from his analyses of many samples of water taken in the current which flows from N.E. to S.W., between Iceland and the east coast of Greenland, he thinks it highly probable that this East Greenland current is in reality not a polar current, but a returning branch of the Gulf-stream, its mean quantity of salt being nearly the same as in the northern part of the Atlantic Ocean, viz. 35·5 per 1000.

The author then compared the Mediterranean with the Baltic, and stated that there is a double current at the entrance of the Baltic as well as in the Straits of Gibraltar; but with this difference, that the under-current of the Mediterranean runs out of, and the surface-current generally runs into, that sea; whereas the under-current of the Baltic is an entering one, and the surface-current of the Sound generally runs out into the Kattegat and North Sea. He showed, moreover, that the deep water in both seas is richer in salt than that from the surface, and consequently has a greater specific gravity.

In the Atlantic he found the reverse, viz. that the quantity of saline ingredients in the water *decreases* with the depth, if the samples are taken at some distance from the shore; and as his analyses are sufficiently numerous, and include specimens from great depths (12,000 feet), he considers this unexpected result to be tolerably well established. He thinks that this fact would prove the existence of a polar current in the depths of the Atlantic, as well as in some parts of its surface.

In the sea to the east of Africa he found the quantity of saline matter slightly increasing with the depth.

June 5, 1862.

The Annual Meeting for the Election of Fellows was held this day.

Major-General SABINE, President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Mr. Curling and Mr. Scott Russell were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected into the Society:—

George Bentham, Esq.
Henry William Bristow, Esq.
Alexander Ross Clarke, Captain R.E.
John W. Dawson, Esq.
Frederick J. Owen Evans, Esq., R.N.
John Braxton Hicks, M.D.
The Very Rev. W. Farquhar Hook, D.D.
George Rolleston, M.D.
Charles William Siemens, Esq.
Maxwell Simpson, Esq.
Balfour Stewart, Esq.
Thomas Pridgin Teale, Esq.
Sir James Emerson Tennent, LL.D.
Isaac Todhunter, Esq., M.A.
C. Greville Williams, Esq.

The Society then adjourned to Thursday, June 19.

June 19, 1862.

Major-General SABINE, President, in the Chair.

Frederick John Owen Evans, Esq., Sir James Emerson Tennent, Thomas Pridgin Teale, Esq., The Very Rev. Walter Farquhar Hook, D.D., Maxwell Simpson, Esq., Dr. J. Braxton Hicks, C. W. Siemens, Esq., Isaac Todhunter, Esq., and C. Greville Williams, Esq., were admitted into the Society.

César Mansuete Despretz, Franz Ernst Neumann, and Ernst Heinrich Weber, were balloted for, pursuant to notice, and elected Foreign Members of the Society.

The following communications were read :—

I. "Dissections of the Ganglia and Nerves of the Œsophagus, Stomach, and Lungs." By Robert Lee, M.D., F.R.S. Received May 13, 1862.

On the 17th of July 1861 I resolved to dissect the nerves of the human stomach immersed in alcohol, as I had done those of the uterus and heart, with magnifying powers of six and twelve diameters. Having procured from Dr. William Dickinson at St. George's Hospital a healthy stomach with three inches of the œsophagus, and having thoroughly washed away the contents, and the blood from the vessels, it was placed in a shallow vessel and covered with rectified spirit. With the help of my dissecting lens, a pair of small straight forceps, a pair of small curved forceps, and curved needle, I proceeded cautiously to remove all the white condensed cellular membrane in which the trunks and branches of the par vagum on the œsophagus were imbedded, and the sheath of slender fibres of cellular membrane closely investing all the nerves. Two glands situated near the termination of the œsophagus in the stomach were likewise removed.

The trunks and branches of the par vagum having thus been completely laid bare, the whole œsophagus appeared covered with ganglionic plexuses of nerves, and distinct ganglia formed on the nerves were clearly seen. Some of these ganglia had the usual appearance of ganglia of the great sympathetic, with nerves entering and nerves passing out from them, and these branches passing into other ganglia. Some of the ganglia formed on the trunks of the par vagum were long and thin, presenting the appearance which Mr. Joseph Swan has called gangliform membranes. Near the cardia, both trunks of the par vagum terminated abruptly in long solid ganglia of a reddish colour and firm texture ; and from these numerous small branches of nerves with ganglia were sent to the whole cardiac extremity of the stomach without being accompanied with arteries; and others were sent to the coronary artery, and accompanied this artery with all its ramifications to the lining membrane of the stomach.

On examining minutely the ganglia and nerves of the œsophagus, numerous branches were seen passing down between the strong longitudinal muscular fibres of the œsophagus to the circular muscular fibres of the middle coat, upon which plexuses of nerves with small

ganglia were formed. Some branches of nerves were seen passing from the middle coat of the œsophagus into the lining membrane.

After having completed the dissection of the œsophageal ganglia and nerves, the nerves continuous with them were then carefully traced throughout the walls of the stomach from the cardia to the pylorus. The peritoneal coat having been removed, a thin strong tendinous expansion was seen underneath, covering the whole convex border of the stomach and a great part of both the anterior and posterior surfaces; the removal of this fascia was necessary before the nerves could be traced.

If the preparation be now examined, numerous small nerves will be seen proceeding from the abrupt termination of the par vagum at the cardia, and distributed extensively over the cardiac extremity of the stomach, to the muscular coat. Numerous branches of nerves can be seen passing down between the muscular fibres of the outer to the subjacent muscular coat, and largely distributed over the fibres of this coat.

The two trunks of the par vagum, divided into numerous branches, can be seen passing forward to the coronary artery, which has been cut across, with all the nerves proceeding from the semilunar ganglion which united at the cardiac extremity of the stomach with these nerves continued from the par vagum.

The trunk and all the branches of the coronary artery are accompanied with nerves from the par vagum and semilunar ganglion, and in this dissection the nerves with the arteries have been traced extensively to the lining membrane of the stomach. The branches of the coronary artery are seen passing down through the muscular coats generally, about midway between the smaller and larger curvatures, accompanied with nerves upon which ganglia are formed; and numerous branches of nerves are seen passing to the muscular coats from the nerves which accompany the arteries.

I will not attempt further on this occasion to describe the ganglia and nerves of the œsophagus and stomach displayed in this dissection, from which it is obvious that there are two sets of nerves distributed throughout the walls of the stomach, one to the muscular coats, the other destined to supply the lining membrane.

I have made elaborate dissections of the ganglia and nerves of the whole œsophagus, stomach, alimentary canal, and lungs; but of these

I shall give no account to the Royal Society unless expressly requested by the Council to do so, and assured that my communication shall receive that treatment which I consider the importance of the subject to demand.

II. "Further Observations on the Distribution of Nerves to the Elementary Fibres of Striped Muscle." By LIONEL S. BEALE, M.B., F.R.S., Professor of Physiology and of General and Morbid Anatomy in King's College, London; Physician to King's College Hospital. Received June 19, 1862.

(Abstract.)

After referring to the views entertained on the mode of termination of the nerves in the tissues generally, the author proceeds to consider the arrangement of the nerves in muscle. The old view was that nerves terminate in loops or networks which are external to the sarcolemma. More recent researches had proved that these loops and networks are composed of coarse dark-bordered fibres, and from them finer fibres had been followed to the surface of the elementary fibres, and it was concluded that these terminate upon the sarcolemma in *free ends*. In the Philosophical Transactions for 1860, a paper by the author was published, in which it was shown that the distribution of nerve-fibres to the muscles of the mouse was much more extensive than was generally supposed, and that to each muscular fibre, pale nerve-fibres with nuclei are distributed throughout its entire length; that numerous fibres cross the elementary fibres at various angles, and thus the appearance of a network of nerve-fibres is produced. This network is upon the same plane as the capillaries, and can be stripped off the surface of the sarcolemma with these vessels. Last year Kühne published a memoir on the termination of the nerves upon the elementary muscular fibres of the frog, and supported his view expressed in previous papers, that the nerves penetrate the sarcolemma and terminate in close relation to the contractile tissue*. Kühne endeavoured to show that the white substance of the nerve ceases at the sarcolemma, and that a pale nucleated fibre, the continuation of

* Ueber die peripherischen Endorgane der motorischen Nerven, 1862.

the axis-cylinder of the nerve, perforates the sarcolemma and terminates in free extremities beneath. In connexion with these pale fibres he described special organs of an oval form and containing a nucleus. In the Croonian Lecture for the present year, Professor Kölliker stated that he had failed to demonstrate the peculiar organs described by Kühne, that Kühne's pale fibres are outside the sarcolemma, and that the nerves terminate in free ends, the sheath of the nerve being continued for some distance over the pale fibre*. He also described some nerve-fibres, which for the most part ramify over the surface of the muscle. These he regarded as sensitive fibres. Kölliker and Kühne agree that the muscle receives but a small supply of nerves, that their supply is limited to one part of the muscle, and that a comparatively very small portion of each elementary fibre is brought into relation with the nerves at all. The author's conclusions are quite at variance with these views. Although in many cases the fine pale nerve-fibres could not be followed for any great distance from their origin, in some instances this had been done. The pale fibres consist of a bundle of very fine fibres, which divides and subdivides into smaller bundles, and these, after being followed to the edge of the muscular fibres, can often be traced a long way amongst the fibres of connective tissue, and can sometimes even be followed to other trunks. The author had seen many fibres less than the $\frac{1}{50,000}$th of an inch in diameter, which had been proved to consist of at least two fibres. Many of the so-called connective tissue corpuscles, close to the sarcolemma, are really the nuclei of very fine pale nerve-fibres, which form, as in the mouse, networks on the surface of the muscular fibre; but the meshes are larger and the fibres much finer in the frog than in the mammal or bird.

The author showed that the distribution of the dark-bordered fibres to many muscles of the frog is by no means so limited as is generally supposed. The elementary fibres of the inferior muscle of the eye of the frog are crossed by dark-bordered fibres at intervals of the $\frac{1}{50}$th of an inch. The author showed that what appears to be the outline of a dark-bordered fibre *near its peripheral distribution,* really consists of a finer nerve-fibre in many instances.

* See Kölliker's 'Handbuch der Gewebelehre des Menschen.' Vierte Auflage, 1862, pp. 203, 286, 287, figs. 111, 157, 158. Also the Croonian Lecture delivered May 1st, 1862.

Fine nerve-fibres run in the same sheath with the dark-bordered fibres. Some of these fibres are the direct continuation of dark-bordered fibres. There are often also fine fibres to be demonstrated external to what appears to be the sheath of the fibre. Nuclei are connected with the dark-bordered fibre, with the fine fibres in, and with those external to the sheath. The pale fibres of Kühne and Kölliker are always compound, and consist of—

1. A very fine fibre prolonged from the dark-bordered fibre.

2. Very fine fibres continuous with those in the sheath of the nerves, or external to it.

The author concludes, from numerous observations upon the distribution of nerves in many different tissues, that the general disposition of the finest fibres is the same as that of the coarser trunks and fibres. In passing from the trunks towards the ultimate distribution of the nerves, it might be said we meet with finer and still finer networks and plexuses; the finest fibres visible with the highest powers (1700 diameters) being composed of more than a single fibre. It is therefore probable that in all cases complete circuits exist. The author maintains that the really important part of the peripheral nerve-fibres only commences at the point where the dark-bordered nerve-fibre seems to cease. Beyond this there is a most elaborate network, the fibres of which are compound and composed of very fine fibres. The meshes of this network and the fibres differ much in size in different tissues. The active elements of the tissues lie in or upon the meshes of this network.

The author then discusses the relation of the terminal branches of the nerve-fibres to connective tissue. His views are briefly expressed in the conclusions given below. In order to see the appearances described by the author, the tissue must be mounted in some fluid which reflects highly, like syrup or glycerine. The fine fibres he has seen cannot be demonstrated in specimens mounted in fluids composed mainly of water.

The paper is accompanied with upwards of forty figures copied from specimens magnified by a twelfth or by a twenty-sixth of an inch object-glass made by Messrs. Powell and Lealand, and magnifying respectively 700 and 1700 diameters linear.

Conclusions.

1. In certain muscles of the frog the distribution of dark-bordered nerve-fibres is pretty uniform in every part. Although in the case of the pectoral a greater number of nerve-fibres is distributed to the central part of the muscle, fibres may be traced from the large bundle almost to the extremities of some of the muscular fibres. Many branches which easily escape observation pass between the muscular fibres, and their subdivisions supply neighbouring fibres, or are gradually lost in the connective tissue.

2. Fine nerve-fibres are most easily demonstrated on the external surface of the sarcolemma near the nerve-trunks; but reasons have been advanced in favour of the conclusion that every elementary muscular fibre is more or less freely supplied with nerve-fibres throughout its entire length. Many of the fine nerve-fibres on the surface of the muscular fibres become gradually very faint, until from their extreme tenuity we are no longer able to follow them.

3. Fine nerve-fibres in direct continuation with the dark-bordered fibres, and less than the $\frac{1}{30,000}$th of an inch in diameter, have been seen to divide into finer branches which have nuclei in connexion with them.

4. The pale fibres delineated by Kühne and Kölliker, and by them considered terminal, consist of—

a. Fibres about the $\frac{1}{30,000}$th of an inch in diameter, or less, resulting from the subdivision of the dark-bordered fibre.

b. Fibres resulting from the subdivision of fine nerve-fibres ramifying in the sheath of the dark-bordered fibre, or situated external to it.

5. Nuclei are found in connexion with—

a. The dark-bordered fibre itself, near its terminal ramifications.

b. The fine fibres which are the direct continuation of the dark-bordered fibres.

c. The fine fibres in the sheath, or external to it.

6. The nuclei and delicate fibres above referred to are arranged so as to form networks, the meshes of which vary much in size, situated with the capillaries on the external surface of the sarcolemma. The fibres of this network are compound, and consist of finer fibres which are distinct from, and do not anastomose with, each other. The fine fibres continued from some of the dark-bordered fibres, as well as

those ramifying in the sheath of the nerves, may sometimes be followed over six or more elementary muscular fibres, and form, with other fine branches, networks, many of the meshes being as wide as a muscular fibre.

7. Fine nerve-fibres with nuclei connected with them exist (not unfrequently to the number of four or five) in the sheath of the dark-bordered nerve-fibres near their distribution; and some are also found external to what appears to be the outline of the sheath. Some of these result from the subdivision of a dark-bordered fibre.

These fine fibres and their nuclei have been hitherto included under the head of 'connective tissue.'

8. The connective tissue around the elementary muscular fibres, and in connexion with the nerve-fibres, is composed of—

a. Nuclei which might have taken part in the formation of the nerve-fibres, but which have degenerated, and a low form of fibrous tissue has alone been produced.

b. Fibres and nuclei which were once active, and formed an integral part of the nervous system, but which have grown old, and have been replaced by new nuclei and fibres.

c. The remains of altered and wasted vessels and nerve-fibres distributed to them, and wasted muscular fibres themselves.

9. The nerves distributed to the voluntary muscles of the frog do not terminate in free ends, but there is reason for believing that complete nervous circuits exist. In all cases the fibres resulting from the division of the ordinary nerve-fibres are so fine that many cannot be seen with a power magnifying less than 1000 diameters, and there is evidence of the existence of fibres which could only be demonstrated by employing a much higher magnifying power. It is by these very fine fibres alone, and their nuclei, that the tissues are influenced. The ordinary nerve-fibres are only the cords which connect this extensive peripheral system, which has been traced in different tissues far beyond the point to which the dark-bordered nerve-fibres can be followed, with the central organs of the nervous system.

10. The facts and conclusions above stated, with reference to the distribution of nerve-fibres to the voluntary muscles of the frog, are in accordance with the arrangement of the finest nerve-fibres demonstrated in many other tissues of the same animal, and agree with

many appearances observed by the author in connexion with the peripheral distribution of the nerves, not only in certain tissues of man and the higher mammalia, but also in invertebrate animals.

11. The distribution of the finest branches of the nerve-fibres can only be demonstrated in tissues which have been immersed in fluids which refract highly, as syrup or glycerine.

III. "Researches on the Development of the Spinal Cord in Man, Mammalia, and Birds." By JACOB LOCKHART CLARKE, Esq., F.R.S. Received May 20, 1862.

(Abstract.)

In the first stage of development the spinal cord consists simply of a canal surrounded by a single layer of small cells or nuclei, which are not distinguishable from each other in regard to size or structure, and are so closely aggregated as to appear in actual contact. After a time this homogeneous layer, while it increases in depth, separates irregularly into two strata, the inner stratum forming the epithelium, and the outer the grey substance. This differentiation of structure proceeds gradually, and is not at first marked by any definite line of separation, nor by any apparent difference in the structure of the component cells. At the same time there is gradually formed around the walls of the nuclei a granular substance, which unites into processes or fibres, and constitutes a continuous network, by which all the nuclei or cells of both layers are uninterruptedly connected. In the grey layer there is at first no apparent difference between the nuclei or cells of the anterior and posterior portion, although in each portion dark or more closely aggregated groups may be observed in connexion with roots of the nerves. As development advances, however, while the nuclei of the posterior grey substance remain for a long time but little altered, those of the anterior substance increase in size, become more granular, and are connected by thicker fibres united in a coarser network. At the same time, in the separate groups of the anterior grey substance, the granular network around the nuclei assumes a coarser and sponge-like structure, until it constitutes a number of large roundish or irregular and adjacent cells with thickening and nucleated walls. While these are in course of development, the contained nuclei are forming

within them and around themselves a gradually increasing layer of finely-granular substance. The thick cell-walls have all the appearance commonly assumed by connective tissue, and are continuous with that tissue in the grey substance of the cord.

In the intervertebral ganglia there is some difference in the manner in which the nerve-cells make their appearance. The small cells or nuclei gradually enlarge, assuming a variety of shapes; and it is not till they have increased to a considerable size that nuclei and granular contents are observable within them. These cells, like those of the cord, are connected, not only with each other, but with fibrous prolongations from the sheath of the ganglion, with the intervening connective tissue, and with the sheaths of blood-vessels, in one uninterrupted network. The fibres of the posterior roots of the nerves, as they pass through the ganglion, split up and subdivide into fibrillæ, which become successively continuous with processes of series of the nerve-cells, which for the most part are cup-shaped or pyriform.

The processes of the epithelium around the canal of the cord are directly continuous with the connective tissue and with the sheaths of blood-vessels and pia mater at the surface.

In the early stages of development, then, it appears that there are two kinds of nuclei in the grey substance of the cord: that one kind developes the general network of tissue which pervades the entire substance, but proceeds no further; that each of the other kind, while it is connected with this network, as well as with the true nerve-fibres, developes around itself a nucleated wall, which is still connected, and ultimately blended with the surrounding reticular structure, but, proceeding further, it again forms around itself and within the cell-wall an increasing layer of granules. The granular contents of the cells are connected with their walls, which they form around themselves; the walls of the cells are continuous with the connective tissue, and this again is continuous with the sheaths of the blood-vessels and pia mater of the surface, as well as with the processes of the epithelium: so that the connective tissue of the cord would appear to be intermediate in its nature between the nerve-tissue and the pia mater on the surface; but there is no reason to believe that the connective tissue could ever be developed into nerve-tissue.

IV. "Observations made on the Movements of the Larynx when viewed by means of the Laryngoscope." By JOHN BISHOP, Esq., F.R.S. Received June 5, 1862.

I had not contemplated any further investigation on the physiology of intonation by the human organs of voice, had not my attention been aroused by the facility afforded by the apparatus of Professor Czermak, of seeing what actually takes place in the larynx during the production of vocal sounds.

The tact of the Professor in applying the instrument in his own case, and the impunity with which he is able to bear its presence in the sensitive parts of the pharynx, are great advantages.

In many persons the presence of any foreign body so applied, usually produces, by reflex action, a sense of sickness in the stomach.

In ordinary breathing the glottis is wide open, and the arytenoid cartilages are thrown wide apart; but on the production of the most simple sound, these cartilages are suddenly and rapidly closed, and the edges of the vocal cords come into juxtaposition with each other so as to leave no interval between them in their entire length.

In the production of the lower tones of the voice the vocal cords may be seen to vibrate throughout their whole length, and even at their prolongations at the base of the arytenoid cartilages; they seem to vibrate also throughout their entire breadth. As the pitch of the tones rises in the scale, the length of the cords in a state of vibration diminishes, and they are pressed more closely against each other: as the tones become more acute, the pressure is increased, and the tension of the vocal cords augmented; the breadth of the cords is also diminished.

When the chest tones have arrived at the limit of the scale of acute range, and the falsetto tones commence, the glottis is seen to be more closely pressed together, and the edges only of the vocal cords are suffered to vibrate, as Garcia has already observed. On the other hand, while the chest tones are produced, a larger surface of the vocal cords is in a state of vibration. When the falsetto tones are produced, it appears that the very extreme edge only of the cord vibrates, and a much less expenditure of breath is required. While the highest notes of the voice are intoned, the vocal cords are so closely pressed together, that a small portion only of the glottis is

seen to yield to the pressure, which takes place nearly at its central portion.

From the inspection of the vocal organs now so easily obtained, it may be stated in general terms that, as the voice ascends from its lowest to its more acute tones, the lengths of the vibrating portions of the vocal cords are proportionally diminished, while at the same time their tensions are increased; and, in fact, they present the same phenomena as those of musical chords, and they appear to obey the same laws, as Ferrein so long since supposed, and which have since been confirmed by Müller and by myself.

Moreover, the vocal cords form a kind of valve, which is situated in a tube, and acts on the column of air in the manner of a reed.

It is observed that while the pitch of the tones of the voice becomes more grave, the epiglottis is depressed and the pharynx is relaxed; and, on the contrary, as the pitch becomes more acute, the epiglottis is raised and the pharynx becomes contracted: the depression of the epiglottis probably assists in deepening the pitch of the vocal tube in the same manner as the lid of an organ pipe does.

In the production and modulation of the voice, it is astonishing with what accuracy some persons are able to produce at will, sounds of a determinate pitch and of a quality which charm and captivate the ear of a musician. The muscles which are principally concerned in this faculty are the thyro-arytenoid and the lateral crico-arytenoid. The crico-thyroid is limited to stretching the vocal ligaments.

The mere turning of the vocal cords on their axes, out of the vocalizing position, does not afford sufficient space for ordinary breathing, as supposed by Mr. Willis, but we find that the arytenoid cartilages and vocal cords are widely separated during ordinary breathing.

With regard to the controversy as to whether the vocal organs are to be considered as a stringed instrument or as a reeded pipe, it has been thought by some physiologists that the same organs cannot possibly perform the offices of both. However, under the denomination of reeded pipes, we find a great variety of form and structure, and it is not difficult to conceive that while the time of an oscillation of the vocal ligaments obeys the same laws as musical strings, the

valve of the glottis in opening and closing the vocal tube performs an action resembling that of some of the musical reeds.

The human organs of voice have been considered by a great many distinguished philosophers as constituting a reeded instrument, and the relation in which they stand to instruments of that character has been already discussed in my paper in the 'Transactions' of the Royal Society for the year 1846; it only remains to remark that the phenomena brought to light by means of the laryngoscope tend to confirm the idea that the vocal organs really perform the double effect both of reed and string.

In ejaculatory sounds, such as the production of the syllables há, há, há in laughing, the glottis is opened at each intermission and closed at each intonation of sound, thus producing a rapid succession of opening and closing the glottis.

In a paper published in the 'Proceedings of the Royal Society *,' by Manuel Garcia, a great number of observations on the movements of the glottis are described; many of these have been verified both by Professor Czermak and by myself, and we cannot but be gratified by the advance which has been made in our knowledge of the action of the vocal organs during intonation, and that the speculations and controversies which have existed on some points are, by the application of the laryngoscope, now brought to a satisfactory conclusion.

The great differences which we find to exist in the quality of the sounds produced—those, for example, of the chest, and those of the falsetto character, the causes of which have excited so much speculation—are in reality effected by very simple changes in the mechanism of the larynx.

It would have been possible to extend this paper by pursuing the inquiry into the details of the special action of the muscles, and the distribution and functions of the nerves of the larynx, as well as the play of the several cartilages, but I have restricted myself to the actual phenomena presented to the eye, and to the acoustic deductions arising out of the movements of the larynx, more especially those of the thyro-arytenoid ligaments.

The waves of sound generated by the larynx in the column of air contained in the vocal tube, set the whole of the membranes surrounding the tube in a vibratory, reciprocating motion, and we know

* May 24, 1855.

from the researches of Savart, and from pathological data, that these movements are essentially necessary to the production of the most simple sounds; for when these membranes are incapable of being put into a state of vibration, the sounds of the voice are extinguished, and the result is aphonia.

V. "Anatomy and Physiology of the Spongiadæ."—Part III. By J. Scott Bowerbank, LL.D., F.R.S. Received June 18, 1862.

(Abstract.)

This paper is the third part of the Anatomy and Physiology of the Spongiadæ. The author, after pointing out the inefficiency, or rather the non-existence of a definite arrangement of species of sponges, proposes to establish a series of orders, suborders, and genera, the distinguishing characters of which are to be founded on the structural peculiarities of the various organs of the animals which have been described in detail and named in the first and second parts of the paper. The term Amorphozoa, proposed by De Blainville as a designation of the class, is rejected, as all sponges cannot be considered as shapeless, many genera and species exhibiting much constancy in their forms, while that of Porifera, proposed by Dr. Grant, is adopted, as the porous mode of imbibition of nutriment is universal in this class of animals. The author also agrees with Dr. Grant in dividing the class into three great orders, dependent on the nature of the substances of which the skeletons are constructed. These three great divisions are designated by Dr. Grant in the following order:—1st, Keratosa, having skeletons of horny structure, with few or no siliceous spicula; 2nd, Leuconida, the skeletons composed of calcareous spicula; and 3rd, Chalinida, the skeletons constructed of siliceous spicula. The author, for reasons stated in detail in the paper, proposes to change the order of this arrangement, placing the calcareous sponges first, under the designation of Calcarea. The siliceous sponges are placed second, and designated Silicea, while the first order of Dr. Grant, Keratosa, is placed last. With these exceptions of arrangement and designation, the orders are essentially those established by Prof. Grant in his "Tabular View of the primary divisions of the Animal Kingdom."

The first of these orders (Calcarida) has hitherto been represented by the genus *Grantia* only; but as the genus as established by Fleming contains sponges having very differently constructed skeletons, the author has divided the whole of the species of calcareous sponges that have been named and described into the four following genera, *Grantia, Leucolenia, Leuconia,* and *Leucogypsia,* in accordance with four distinct types of skeleton-structure which are found to exist among the sponges originally arranged under the genus *Grantia* of Fleming.

The second order, Silicea, is very much more extensive than that of Calcarea, and, from the striking varieties it affords in the construction of the skeletons, it allows of a subdivision into seven suborders. The first of these consists of sponges having spiculo-radiate skeletons, and contains thirteen genera, as follows:—*Geodia, Pachymatisma, Ecionemia, Alcyoncellum, Polymastia, Halyphysema, Tethea, Halicnemia, Dictyocylindrus, Phakellia, Microciona, Hymeraphia,* and *Hymedesmia.*

The second suborder consists of spiculo-membranous sponges; it consists of one genus, *Hymeniacidon.* The third has spiculo-reticulate skeletons; it contains four genera, *Halichondria, Hyalonema, Isodictya,* and *Spongilla.* The fourth suborder has spiculo-fibrous skeletons; it contains two genera, *Desmacidon* and *Raphyrus.* The fifth suborder has compound reticulate skeletons; it has but one genus, *Diplodemia.* The sixth suborder has solid siliceo-fibrous skeletons; it contains one genus, *Dactylocalyx.* The seventh suborder has canaliculated siliceo-fibrous skeletons, and contains one genus, *Farrea.*

The third order, Keratosa, is also divided into seven suborders. The first, consisting of solid non-spiculate kerato-fibrous skeletons, is represented by one genus, *Spongia*; the legitimate type of the genus being the cup-shaped and finest Turkey sponges of commerce. The second suborder has solid semi-spiculate kerato-fibrous skeletons; it contains at present but one genus, *Halispongia*; the type of which is the coarse massive sponges of commerce from the West Indian Islands. The third suborder has solid, entirely spiculated kerato-fibrous skeletons; it has one genus, *Chalina*: the type of this genus is one of the commonest of the British sponges, *Halichondria oculata* of Johnston. The fourth suborder is characterized by

having simple fistulo-fibrous skeletons; it contains one genus, *Verongia*. The fifth suborder contains sponges which have compound fistulo-fibrous skeletons, and is represented by the genus *Auleskia*. The sixth suborder consists of sponges having regular semi-areno-fibrous skeletons, and is represented by the genus *Stematumenia*. The seventh suborder has irregular and entirely areno-fibrous skeletons; it is represented by the genus *Dysidea*. The whole of these genera (those previously established as well as the new ones proposed by the author) have been characterized in accordance with their anatomical structures.

The author concludes his paper with a dissertation on the discrimination of species, and a general review of those portions of the organization that may be applied with advantage to their scientific description. The principal sources for this purpose being—1st. The spicula. 2nd. The oscula. 3rd. The pores. 4th. The dermal membrane. 5th. The skeleton. 6th. The interstitial membranes. 7th. The intermarginal cavities. 8th. The interstitial canals and cavities. 9th. The cloacal cavities. 10th. The sarcode; and 11th. The ovaria and gemmules. And, finally, directions for the examination and preservation are given, with a few examples of the mode of specific description proposed by the author.

VI. "On the Spectrum of Carbon." By John Attfield, Esq., F.C.S., Demonstrator of Chemistry at St. Bartholomew's Hospital. Communicated by Dr. Frankland. Received June 19, 1862.

(Abstract.)

The author has prismatically examined various flames containing carbon. He finds that certain rays of light are common to ignited oxycarbons, hydrocarbons, nitrocarbons, and sulphocarbons, and concludes that these common rays are those emanating from ignited carbon vapour. By special manipulation he obtains the carbon spectrum with olefiant gas, cyanogen, carbonic oxide, and bisulphide of carbon. Observed by the naked eye, the prevailing colour of ignited carbon is light blue.

VII. "On the Distorted Skulls found at Wroxeter (Salop), with a Mechanico-Chemical Explanation of the Distortion." By Henry Johnson, M.D., Shrewsbury. Communicated by Erasmus Wilson, Esq. Received June 19, 1862.

(Abstract.)

The author states that about twenty crania were brought from the excavations at Wroxeter. Of these, two were discovered at the bottom of a hypocaust, seven feet below the surface of the earth. Of the remaining nineteen, which were dug up in the Orchard some distance from the other excavation, nearly one-half, that is *nine*, were more or less deformed. As the deformed skulls were found lying under less than two feet of light earth, whilst those which were buried under, and pressed by, seven feet of rubble or heavy earth were not deformed, he thinks that the pressure theory alone will not satisfactorily account for the phenomena. The idea occurred to him that some chemical agency was at work in the former case which did not operate in the latter. He ascertained by experiment that the soil of the Orchard was acid, reddening litmus, whilst that of the hypocausts was neutral or alkaline.

The author goes on to show that the acidity of the soil of the Orchard, and of vegetable mould in general, is due to the presence of free carbonic and nitric acids, which are not to be detected in earth taken from some depth, such as that of the hypocaust or a deep pit. That carbonic acid is capable of dissolving bone (that is, carbonate and phosphate of lime) is abundantly proved by more than one experiment. A dried and weighed slip of bone was introduced into a bottle with distilled water highly charged with carbonic acid gas. In a month's time it had decidedly lost weight and become somewhat *flexible*.

The author's first impression was that *humic* acid was the solvent of bone in the earth. He believes that traces of alkaline humates may always be discovered in "the washings" of soil, but that this fact has nothing to do with the solution of buried bones, and therefore he does not pursue the subject.

The author draws, therefore, the following conclusions:—

1. That the distortion of the skulls found at Wroxeter is not *congenital*, but *posthumous*.

2. That pressure alone is not the cause of the deformity.

3. That besides the softening effect of continuous moisture acting for ages upon the cartilaginous or animal matter of the bones, there is proof of the presence of free carbonic and nitric acids very generally in soils, and more particularly in black mould, such as that of the Orchard at Wroxeter.

4. Nitric acid may also be discovered in small quantity. But carbonic acid is almost always present in soil where air and moisture come in contact with organic matters in a state of decomposition. He thinks that this is the principal cause of the solution of bone in the earth, rendering it softer, and more ready to bend or break.

5. That the distortion must occur at a comparatively early period after interment, because when all, or nearly all, the animal matter of the bones is destroyed, they cannot bend.

Lastly. That some of the apparently *bent* bones are really *broken*; Professor Wyville Thomson, of Belfast, having first pointed out to the author minute cracks or fissures in some of the distorted crania.

VIII. "Preliminary Researches on Thallium." By William Crookes, Esq., F.C.S. Communicated by Professor Stokes, Sec. R.S. Received June 19, 1862.

Having so recently been honoured by the Council of the Royal Society with a grant from the Donation Fund for the purpose of defraying some of the expenses of my researches on this new element, I should not have ventured to offer to the Society so incomplete a notice as the present one, had I not within the last week heard that a continental chemist, Professor Lamy, of Lille, has recently been fortunate enough to meet with a residue containing thallium in considerable quantities, and has isolated the element and prepared several of its compounds: it therefore appears advisable at once to place on record a description of several compounds of this body obtained since the date of my first announcement of its discovery in March 1861, but which I had purposely avoided publishing in order that it might form part of a more complete memoir on the subject which I had hoped at some future day to have the honour of sub-

mitting to this Society. I trust that, under these circumstances, I may be pardoned for bringing before the Royal Society an incomplete account of this new element.

The occurrence of a brilliant green line in some selenium residues, whilst examining them for tellurium, led me first to suspect the presence of a new element. This had been derived from a considerable quantity of the seleniferous deposit from the sulphuric acid manufactory at Tilkerode in the Hartz Mountains, which had been kindly placed at my disposal by Professor Hofmann; and the residue was that left behind on distilling the selenium which had been prepared from the deposit by appropriate chemical treatment. The processes through which it had passed limited the elements which could by any possibility be present to some half dozen; and as I was pretty confident that none of these presented in the spectroscope the phenomenon of a single bright-green line, it became of interest to investigate the subject further. In March 1861* I was enabled to announce definitely that the green-line substance was decidedly a new element, and that from some of its reactions it was probably a high member of the sulphur, selenium, and tellurium group, although I hesitated to assert this positively. The paper alluded to contained a sufficient number of the reactions of this body to enable me to prove chemically, as well as optically, that I was dealing with a new element possessing well-defined characters. Pursuing the investigation, I was enabled in the following May† to give a further account of this body, and to propose for it the name of *Thallium* (symbol *Tl*), from the Greek θαλλὸς, or Latin *thallus*, a budding twig,—a word which is frequently employed to express the beautiful green tint of young vegetation, and which I chose on account of the green line which it communicated to the spectrum recalling with peculiar vividness the fresh colour of early spring. In the same note I gave the localities and description of several minerals in which I had found the element, and also a method of extracting it from them in a pure state. Considering that I had sufficiently announced the discovery in these papers, which were republished in nearly every chemical journal in Europe, I turned my

* Philosophical Magazine, S. 4. vol. xxi. p. 301; and Chemical News, vol. iii. p. 194, March 30, 1861.

† Chemical News, vol. iii. p. 303, May 18, 1861.

attention towards procuring a source of thallium which would enable me to prepare this body on the large scale; my experiments having hitherto been confined to mineralogical specimens which I had difficulty in tracing to their source, and the whole amount of thallium which I had as yet obtained not exceeding three grains in weight.

After some delay, Mr. Thornthwaite was good enough to supply me with a considerable quantity of crude sulphur distilled from Spanish copper pyrites. In this I found thallium present to the extent of one or two grains to the pound, and up to within the last few months it has been from the element prepared from this source that I have been working. I have recently, however, succeeded in finding an ore containing thallium, which is worked in this country, and from which I hope to be able to prepare the metal in larger quantities.

I have found the following the most advantageous method for extracting the new element from sulphur or pyrites:—

Powder the ore very finely, and dissolve it as completely as possible in strong hydrochloric acid, with gradual addition of nitric acid until all solvent action ceases; then dilute with water, and filter. Evaporate down to drive off the excess of nitric acid, add a little sulphuric acid if necessary, and take care that the solution does not get dry, or even pasty. Then dilute with water, and heat gently, to be certain of getting all the soluble portion dissolved. Filter: if lead be present, the greater portion will be left behind in this operation in the form of insoluble sulphate. Dilute the filtrate considerably, and add a solution of carbonate of soda until the reaction is distinctly alkaline; then add an excess of solution of cyanide of potassium (free from sulphide of potassium). Heat gently for some time, and then filter. The precipitate contains the whole of the lead and bismuth which may be present as carbonates, whilst the thallium is in solution. A current of sulphuretted hydrogen now being passed through the liquid, precipitates all the thallium, whilst the copper, antimony, tin, and arsenic remain dissolved. If cadmium and mercury are present, they will accompany the thallium. The former can readily be dissolved out by warm dilute sulphuric acid, which has scarcely any solvent action on the sulphide of thallium, whilst this in its turn can be separated from the sulphide of mercury by being boiled in moderately dilute nitric acid, in which the sulphide of mercury is insoluble.

These two metals are, however, seldom present with thallium in the ores which I have examined. The nitric-acid solution is now to be evaporated to dryness, the residue dissolved in hot dilute sulphuric acid, and a piece of pure metallic zinc placed in the liquid; the thallium will be at once precipitated in the form of a deep-brown powder, which soon changes to a heavy black, granular precipitate. The metal can be obtained in the coherent form by fusion in hydrogen.

This method of analysis is given on the supposition that all the above metals are present. It may generally be much abridged, as the ore is seldom of so complicated a character. If there is a difficulty in procuring perfectly pure zinc for the reduction of the sulphate to the metallic state, this can be effected by passing a weak voltaic current through the liquid, using platinum poles; the metal will then be precipitated in the reguline, or spongy state, according to the strength of the current. I have not been very successful in reducing the oxide by hydrogen. The current of gas carries the volatile oxide away from the heated part of the tube before complete reduction takes place. It is, however, probable, from an observation made towards the conclusion of this experiment, that, with a longer tube in proportion to the quantity of material, kept at a good heat throughout its length, this plan might give good results, the metal being considerably less volatile than the oxide.

In many cases, when minute traces only of thallium accompany large quantities of other metals, it may be advisable to repeat the whole or some of the above operations, in order to purify this element from foreign metals which may have escaped complete removal.

I now pass on to a description of thallium and its chemical reactions.

Thallium in the pure state is a heavy metal, bearing a remarkable resemblance to lead in its physical properties. Its specific gravity is, however, higher—about 12. The freshly scraped surface has a brilliant metallic lustre not quite so blue in colour as lead, and it tarnishes more rapidly than this latter metal. It is very soft, being readily cut with a knife and indented with the nail; it may also be hammered out and drawn into wire, but has not much tenacity in this form. It easily marks paper. The fusing-point is below redness, and with care several pieces may be melted together and cast into one lump.

There is, however, generally a loss in this operation, owing to its rapid oxidation. The metal itself does not appear to be sensibly volatile below a red heat. I have made no special attempts at present to determine the atomic weight, although from two estimations of the amount of sulphur in the sulphide it appears to be very heavy. The figures obtained did not, however, agree well enough to enable me to speak more definitely on this point, than that I believe it to be above 100. I may mention that I obtained this element in the pure metallic state and exhibited it to several friends as early as January last *, and should then have published an account of it, had it not been for the reasons already mentioned. Thallium is soluble in nitric, hydrochloric, and sulphuric acids, the former attacking it with greatest energy, with evolution of red vapours.

Oxides of Thallium.—Thallium forms two, and probably three oxides: one possessing basic properties, which I shall call the oxide; another containing more oxygen, possessing acid properties, which may therefore be called thallic acid; and most likely a third, or suboxide, which forms the first portions of the precipitate formed by zinc in solutions of this metal; the first action being a darkening of the solution, and the production of a deep-brown powder, which by longer contact with zinc turns to a dense black precipitate.

Upon carefully evaporating the nitric-acid solution upon a water-bath, but not carrying it to dryness, a mass of deliquescent crystals is obtained on cooling, which are decomposed upon addition of water with separation of a white or pale-yellow precipitate, which appears to be a subnitrate, and an acid solution containing nitrate of thallium. If the liquid is evaporated quite to dryness and kept at a temperature of 100° C. for a little time, the nitric acid goes off, and leaves a residue of thallic acid.

Thallic Acid.—This acid is soluble in water, and may be obtained in the crystalline form from its aqueous solution. It then forms crystals, which are permanent in the air, and have an acid reaction to test-paper. The thallates of the alkalies are also soluble in water, and may be prepared by dissolving the acid in the alkali, or by fusing thallium or its oxide with a mixture of alkaline carbonate and nitrate. The method I originally published for extracting thallium was based

* Vide Chemical News, vol. v. pp. 349, 350.

upon the formation in this manner of an alkaline thallate soluble in water. This acid is also produced in solution when permanganate of potash is added to a soluble salt of oxide of thallium.

Chloride of Thallium.—If a current of dry chlorine is passed over precipitated thallium at a moderate heat, they combine with formation of a volatile chloride, which condenses in the cool part of the tube in the form of a pale-yellow crystalline powder, fusing together in parts to a crystalline lump. Water only partially dissolves this, with production of a white insoluble residue. Dilute hydrochloric acid added to the turbid solution immediately renders it clear; upon evaporating this solution over a water-bath, white crystals of the chloride are deposited. When the nitric-acid solution of thallium or its sulphide is evaporated with an excess of hydrochloric acid, and then more hydrochloric acid added and the evaporation repeated to a syrup, a residue is obtained which is apparently decomposed by water with production of a white precipitate: this is chloride of thallium; it is insoluble or nearly so in water, but readily soluble in dilute hydrochloric or nitric acid.

Sulphide of Thallium.—When sulphuretted hydrogen is passed through the acid solution of chloride of thallium, a partial precipitation of a reddish-brown powder takes place; this appears to be a combination of the chloride and sulphide, and the metal is never entirely removed from solution by this means. The best method of obtaining the sulphide is to precipitate it with sulphide of ammonium in an alkaline solution: unless a large quantity of thallium is present, no immediate effect is produced beyond the darkening of the liquid; it assumes a brown tint, which becomes rapidly more and more intense, especially upon gently heating it, until the sulphide of thallium separates in the form of a deep-brown heavy precipitate which shows a great tendency to collect together in clots at the bottom of the vessel: this formation of the sulphide is very characteristic of the metal. Sulphide of thallium is insoluble in an excess of sulphide of ammonium, ammonia, or cyanide of potassium. Its complete precipitation as sulphide from solutions containing an excess of cyanide of potassium affords a ready means of separating thallium from several metals with which it is frequently associated. It is difficultly soluble in hydrochloric or sulphuric acids, but readily

so in nitric acid. When dry, it is a deep-brown, almost black powder, fusing and volatilizing when heated: when pure, it is neither so fusible nor so volatile as sulphur; but when it occurs with an excess of this latter element, it is very difficult to separate from it by sublimation.

Carbonate of Thallium is precipitated upon adding an alkaline carbonate to the acid chloride solution; it is moderately soluble in an excess of carbonate of ammonia, and readily so in cyanide of potassium. This is a very definite reaction, and enables thallium to be separated with accuracy from lead and bismuth.

Sulphate of Thallium.—When the hydrochloric or nitric solution is evaporated down with sulphuric acid, the more volatile acid is driven off and the sulphate is left behind. It is crystalline and soluble in water.

Iodide of Thallium is precipitated as a yellowish-red powder upon cautious addition of iodide of potassium to a solution of thallium. It is readily soluble in excess of iodide of potassium, forming a colourless solution.

Phosphate of Thallium forms a white flocculent precipitate soluble in mineral acids, but sparingly soluble in acetic acid.

Ferrocyanide of Thallium is white and insoluble in water.

Cyanide of Thallium is precipitated as a white or light-brown powder upon the cautious addition of cyanide of potassium to a solution of thallium. It is readily soluble in an excess of the precipitant.

Chromate of Thallium is a pale-yellow precipitate soluble in acids and reprecipitated upon neutralization with ammonia.

No precipitates are produced when a solution of thallium is mixed with *protochloride of tin, oxalic acid, carbazotic acid, sulphurous acid,* or *protosulphate of iron.*

Most of these reactions have been independently verified by my friends E. O. Brown, Esq., and J. Spiller, Esq., of the Chemical Department, Woolwich Arsenal; and I am glad to be enabled to take this opportunity of expressing my obligations to them for their valuable aid. The reactions are sufficient to prove chemically that the body under examination is a new element. Its behaviour in the spectrum apparatus is perhaps the most conclusive test upon this

point. When a minute portion of the metal (the sulphide, chloride, or, in fact, any compound of thallium) is introduced into the flame of the spectroscope, it immediately produces a single green line, perfectly sharp and well defined upon a black ground, and of extraordinary purity and intensity, almost equal to the sodium-line in brilliancy. It is not, however, very lasting. Owing to its great volatility, a portion introduced at once into the flame merely shows the line as a brilliant flash, remaining only a fraction of a second; but if it be introduced into the flame gradually, the line continues present for a much longer time. If, also, a piece of metallic thallium be introduced into the flame on a platinum wire loop, they fuse together, and the alloy gives the green line rather more permanently, although of course fainter.

Working on a small scale, it is not easy to obtain these compounds free from soda; but when that is effected, and a tolerable quantity of substance is held on a loop of platinum wire in a flame, the green colour is most brilliant, and produces very extraordinary effects upon the appearance of surrounding objects. If thallium could be obtained in quantity, this ready means of producing an intense and homogeneous green light could not fail to be applicable to some useful purpose.

The green line of the thallium spectrum appears to be unaccompanied by any line or band in other parts of the spectrum. A flame of sufficient temperature to bring the orange line of lithium into view produces no addition to the one thallium-line; and an application of telescopic power strong enough to separate the two sodium-lines a considerable distance apart still shows the thallium-line single. I consider therefore that I am justified in stating that thallium produces *the simplest spectrum of any known element.* Theoretical inquiries into the cause of the spectrum lines, and their relation to other constants of an element, may be facilitated now we know a metal which gives rise to luminous vibrations of only one degree of refrangibility. The remarkable simplicity of the thallium spectrum offers a strong contrast to the complicated spectra given by mercury, bismuth, and lead—the metals to which it has the most chemical resemblance.

The position of the green line does not coincide with any definite line in the solar spectrum. According to Kirchhoff's theory, we must

therefore assume that thallium is not present to any great extent in the sun. Under the highest telescopic power of my apparatus, the line appears to be absolutely identical in refrangibility with a sharp well-defined line in the barium spectrum, to which Professors Bunsen and Kirchhoff have given the name Baδ. Want of material has hitherto prevented me from taking accurate measurements of the distance between the thallium-line and the principal lines of the solar spectrum.

This green line is an exquisitely delicate test for the presence of thallium, and shows it to be a somewhat widely distributed element. Many specimens of crude sulphur contain it (especially when rather dark-looking). In most cases it is only necessary to set fire to as large a piece of sulphur (less than a pea) as the platinum loop will hold, and when it has nearly burned away to blow it out, and then introduce it at leisure into the flame of the spectroscope, for the thallium to show its presence by a bright-green line which will flash for an instant into the field of view. Although the greater part of the thallium is left behind after burning off the excess of sulphur in this manner, some of it volatilizes, and consequently, if the specimen gives no indications of thallium by this treatment, it will be advisable to dissolve out as much of the sulphur as possible with bisulphide of carbon, and then to test the residue in the flame.

Thallium is a constituent of very many mineral ores. Upon examining a large collection of cupriferous pyrites from different parts of the world, I found it present in more than one-eighth. It is not confined to any particular locality; neither does it seem to bear any relation to the presence or absence of arsenic in the mineral. I have, however, very rarely met with it in pyrites in which copper was absent. In most cases it is only necessary to powder a small fragment of the mineral and ignite a little of it in the flame on a moistened platinum wire, for the green line to be distinctly visible.

If a thalliferous pyrites is finely powdered and then heated to redness in a glass tube, as much as possible out of contact with air, the sulphide of thallium, together with some free sulphur, sublimes from it and may be condensed by appropriate arrangement. This sublimate gives the thallium-line with great brilliancy.

Owing to the frequent occurrence of thallium in copper ores, it is very probable that this element may sometimes be present in commercial copper, and may give rise to some of the well-known, but unexplained, differences of its quality. I am at present engaged in investigating this subject, and have already found some indications of thallium in commercial products.

I have no hesitation in saying that in some of our large copper, sulphur, and sulphuric-acid works, thallium is at the present time being thrown away by the hundredweight: a very slight modification of the present arrangements of the furnaces and condensing flues, or even an examination of some of the residues, would enable nearly the whole of this to be saved. Bearing this in view, I am now in communication with several large consumers of thalliferous minerals. My applications have without exception been met with the utmost courtesy and most obliging offers of assistance, and there is therefore every probability that I shall soon have an opportunity of preparing this new element in considerable quantities, and thus be enabled to pursue the investigation with more comfort and accuracy than hitherto, when my stock of material has had to be counted by the grain.

IX. "On the Photographic Transparency of various Bodies, and on the Photographic Effects of Metallic and other Spectra obtained by means of the Electric Spark." By Prof. W. Allen Miller, M.D., LL.D., V.P. and Treas. R.S. Received June 19, 1862.

(Abstract.)

In this paper the author pursues an inquiry the commencement of which was communicated to the Chemical Section of the British Association last year. Owing to the employment of a prism of bisulphide of carbon, he was then led to believe that the photographic effects of the electric spectra produced by the different metals were in a great degree similar, if not identical. Subsequent investigations have, however, shown him that the absorbent effects of the bisulphide upon the chemical rays are so great, that the conclusions then drawn from observations made by this refracting medium require

very considerable modification. Notwithstanding the great length of the chemical spectra obtained by the aid of the bisulphide, not more than *one-sixth* or *one-seventh* of the true extent of the spectrum produced by the electric spark between various metals is procured, as may be shown by comparing the spectrum with one of the same metal furnished by the use of a lens and prism of rock-crystal.

Rock-crystal, however, possesses but a comparatively small refractive and dispersive power, whilst it almost always affords some trace of double refraction in one portion or other of the spectrum procured by its means.

In searching for some singly refracting medium which should possess sufficient refractive and dispersive power to enable it to be used advantageously in the construction of lenses and prisms suitable for this inquiry, the author was led to examine the photographic absorption of a variety of colourless substances which appeared perfectly transparent to the luminous rays. The experiments detailed in the first portion of the present paper refer to this absorbent action of various media upon the chemical rays of the spectrum; whilst the second portion of the paper is devoted to a description of the electric spectra of some of the more important elementary bodies, and the effect of varying the gaseous media in which the sparks producing these spectra are made to originate.

1. *The Photographic Transparency of Bodies.*—In the experiments upon the absorbent action of the different media, the source of light employed was the electric spark obtained between two metallic wires (generally of fine silver), connected with the terminals of the secondary wires of a ten-inch induction-coil. The light, after passing through a narrow vertical slit, either before or after traversing a stratum of the material the chemical transparency or *diactinic* quality of which was to be tested, was allowed to fall upon a quartz prism placed at the angle of minimum deviation for the mean of the refracted rays. Immediately behind this was a lens of rock-crystal, and behind this, at a suitable distance, the spectrum was received upon a collodion-film coated with iodide of silver; this was supported in the frame of a camera, and after an exposure, generally lasting for five minutes, the image was developed by means of pyrogallic acid, and fixed with cyanide of potassium.

The general results of these experiments were as follows:—

1. Colourless bodies which are equally transparent to the visible rays, vary greatly in permeability to the chemical rays.

2. Bodies which are photographically transparent in the solid form, preserve their transparency in the liquid and in the gaseous states.

3. Colourless transparent solids which exert a considerable photographic absorption, preserve their absorptive action with greater or less intensity both in the liquid and in the gaseous states.

Whether the compound is liquefied by heat or dissolved in water, these conclusions respecting liquids are equally true. The perfect permeability of water to the chemical rays, conjoined with the circumstance that in no instance does the process of solution seem to interfere with the special action upon the incident rays of the substance dissolved, renders it practicable to submit to this test a great number of bodies which it would otherwise be impossible to subject to this species of experiment on account of the extreme difficulty of obtaining them in crystals of sufficient size and limpidity.

Glass vessels cannot be employed to contain the liquids during the trial. Flint-glass, crown, hard white Bohemian, plate-glass, window-sheet, and Faraday's optical glass, all, even in thin layers, shorten the spectrum by from three-fifths to four-fifths or even more of its length. Mica produces a similar effect. Indeed, the only substance which the author found could be employed with advantage is rock-crystal cut into thin slices and polished. The value of this material in researches upon the more refrangible end of the spectrum was pointed out by Prof. Stokes and M. E. Becquerel several years ago. In order to hold the liquids for experiment, a small trough was prepared by cutting a notch in a thick plate of plate-glass, the sides being completed by means of thin plates of quartz, which were pressed against the ground surfaces of the plate-glass by the aid of elastic bands of caoutchouc; a stratum of liquid of 0·75 inch in depth was thus obtained for each experiment.

The substances which, after atmospheric air and certain other gases, are most perfectly diactinic, are rock-crystal, ice, as well as pure water, and white fluor-spar. Rock-salt is scarcely inferior to them, if at all. Then follow various sulphates, including those of baryta, and the hydrated sulphates of lime and magnesia, as well as those of the alkalies. The carbonates of the alkalies and alkaline

earths, as also the phosphates, arseniates, and borates, are likewise tolerably transparent, though saturated solutions of phosphoric and arsenic acids exerted considerable absorbent power; so also did those of the alkalies, potash, and soda, possibly from the presence of a trace of some foreign colouring matter, as those liquids had an extremely faint greenish tinge.

The soluble fluorides, as well as the chlorides and bromides of the metals of the alkalies and alkaline earths, are freely diactinic, but the iodides are much less so, and exhibit certain peculiarities. All the organic acids and their salts which were tried by the author exerted a marked absorptive action upon the more refrangible rays. Amongst those subjected to experiment were the oxalates, tartrates, acetates, and citrates, those mentioned first in order having the greatest absorptive action. It is, however, much more difficult to obtain organic compounds in a state of purity sufficient to furnish trustworthy results, than is the case with the salts of the inorganic acids. The author, therefore, expresses himself with more reserve upon some of these organic bodies, particularly the acetates, than in other cases. The different varieties of sugar are freely diactinic.

Amongst the salts of inorganic acids, the nitrates are the most remarkable for their power of arresting the chemical rays. A solution of each of these salts, in all the instances tried, cut off all the more refrangible rays, and reduced the spectrum to less than a sixth of its ordinary length. The chlorates, however, do not participate in this absorptive power to nearly the same extent.

Although the sulphates, as a class, are largely diactinic, the sulphites are much less so; and the hyposulphites cut off about three-fourths of the length of the spectrum, leaving only the less refrangible portion.

Of eighteen different liquids tried by the author, two only can be regarded as tolerably diactinic, viz. water, which is eminently so, and absolute alcohol, which, however, exhibits a considerable falling off. The liquids which follow are mentioned in the order of their chemical transparency, those most transparent being mentioned first: —Dutch liquid, chloroform, ether; then benzol and distilled glycerin, which differ but little; then fousel oil, wood-spirit, and oxalic ether, which are also nearly alike; acetic acid, oil of turpentine, glycol, carbolic acid, liquid paraffin, boiling at 360° F., and bisul-

phide of carbon. Finally, terchloride and oxychloride of phosphorus, although perfectly colourless and limpid, arrest all the chemical rays.

The experiments upon aëriform bodies yielded important results; they show but little coincidence with those of Tyndall on the absorptive power of the gases for radiant heat. These experiments were made by interposing in the track of the ray between the vertical slit and the quartz prism, a brass tube two feet long, closed at each end air-tight by means of a plate of quartz. Each gas or vapour in succession was introduced into the tube, and the results compared with those produced by causing the rays to traverse the tube when filled with atmospheric air.

Amongst the colourless gases, oxygen, hydrogen, nitrogen, carbonic acid, and carbonic oxide exhibit no absorptive power.

Olefiant gas, protoxide of nitrogen, cyanogen, and hydrochloric acid exert a slight but perceptible absorbent effect. But in the case of coal-gas the absorptive action is extremely marked, the more refrangible half of the spectrum being cut off by it abruptly. The absorption exerted by sulphurous acid is still more powerful and as sharply defined; sulphuretted hydrogen and the vapour of bisulphide of carbon exhibit a still more decided absorbent action; the effect of the terchloride and oxychloride of phosphorus is not less marked. This absorbent action of these different compounds of sulphur and phosphorus is very striking.

Coal-gas appears to owe its remarkable power of arresting the chemical rays to the presence of the vapour of benzol and other heavy hydrocarbons; since the vapour of benzol at 65°, diffused to saturation through a column of atmospheric air two feet long, exerts a still more powerful absorptive effect than coal-gas.

On the other hand, the effect of a similar arrangement, in which the vapour of ether, of chloroform, and of oil of turpentine was substituted for that of benzol, gave effects which, though perceptible, were much less marked. An arbitrary scale is laid down, by which a comparative estimate of the absorptive power of each compound, whether solid, liquid, or gaseous, may be effected with tolerable accuracy.

With a view of facilitating the production of a spectrum on a flat field, at a uniform distance at all points from the prism, the author instituted a series of experiments, in which a small metallic speculum

was substituted for the lens of rock-crystal; but the loss of chemical power in the reflected rays was so considerable, and this loss occurred so unequally at different points, that the method was abandoned. The results of the photographic action of light reflected at an angle of 45° from the polished surface of several of the principal metals is given. The reflexion from gold, although not very intense, was found to be more uniform in quality than that from any other metal that was tried. Burnished lead also gave very good results. The reflexion from silver is singularly deficient in some portions of the less refrangible rays, although in most other parts the reflexion is tolerably perfect, except for rays of extremely high refrangibility.

2. *The Electric Spectra of the Metals.*—The author proceeds then to detail his experiments upon the spectra obtained by causing the sparks caused by the secondary current from the induction-coil to pass between electrodes composed of various elementary substances, and he gives photographs of the impressions obtained from collodion negatives of a considerable number of different elementary bodies. The spectra were procured by arranging a quartz-train in the manner already described. Among the elements so examined are the following:—

Platinum.	Arsenic.	Copper.
Palladium.	Tellurium.	Aluminum.
Gold.	Tungsten.	Cadmium.
Silver.	Molybdenum.	Zinc.
Mercury.	Chromium.	Magnesium.
Lead.	Manganese.	Sodium.
Tin.	Iron.	Potassium.
Bismuth.	Cobalt.	Graphite, and
Antimony.	Nickel.	Gas-coke.

The commencement of each spectrum in its less refrangible portion is similar in nearly all cases; and as it is this portion only which is transmissible through bisulphide of carbon, this circumstance explains the similarity of all the spectra procured by the author from different metals in his earlier experiments, already laid before the British Association. In the more refrangible parts of the spectrum great and characteristic differences between the results obtained with the different metals are at once manifest. In some cases, as in those of copper and nickel, the action is greatly prolonged in the more refrangible extre-

mity, whilst the intense and highly characteristic spectrum of magnesium is much shorter.

In many cases metals which are allied in chemical properties exhibit a certain similarity in their spectra. This occurs, for example, with the magnetic metals, iron, cobalt, and nickel, and with the group embracing bismuth, antimony, and arsenic. The more volatile metals exhibit generally the most strongly marked lines. Cadmium, for instance, gives two intense groups. Zinc, two very strong lines near the less refrangible extremity, three near the middle, and four nearly equidistant lines towards the termination of the more refrangible portion, whilst in the spectrum of magnesium the chemical action is almost suddenly terminated near the middle by a triple group of very broad and strong lines.

It will be observed, on examining the photographs of these spectra of the various metals, that the impressions, particularly in the more refrangible portions, consist of a double row of dots, running parallel with the length of the spectrum, and forming the terminations of lines rather than lines themselves, as though the intense ignition of the detached particles of metal, necessary to furnish rays capable of exciting chemical action, had ceased before the transfer of these particles to the opposite electrode had been completed.

If each electrode be composed of a different metal, the spectrum of each metal is impressed separately upon the plate, as is evident on examining the photographs.

When alloys are employed as electrodes, the spectrum exhibited is that due to both the metals; but if the metals made use of are approximatively pure, the spectrum is hardly to be distinguished from that of the pure metal. In the case when alloys are used as electrodes, it is not always the more volatile metal which impresses its spectrum most strongly. A specimen of brass, for example, containing 38 per cent. of zinc, gave a spectrum which could not be distinguished from that of pure copper, though an alloy of three parts of gold and one of silver gave a spectrum in which the lines due to silver predominated.

The author then proceeds to describe a number of experiments upon the transmission of sparks between electrodes of different metals in a current of several different gases. The apparatus employed consisted of a glass tube; into the side an aperture was drilled,

which could be closed by a plate of quartz; the ends of the tube were closed by ground brass plates, each supporting a pair of brass forceps, into which the electrodes were fitted; through the axis of the tube a current of each gas was transmitted at the ordinary atmospheric pressure.

Among the gases thus tried were hydrogen, protoxide of nitrogen, carbonic acid, carbonic oxide, olefiant gas, marsh-gas, cyanogen, sulphuretted hydrogen, sulphurous acid, nitrogen, and oxygen. The spectrum obtained from the same metal varied considerably in these different media. In hydrogen the intensity of the spectrum was greatly reduced, and the more refrangible rays were wanting, but no new rays made their appearance. In carbonic acid, carbonic oxide, olefiant gas, marsh-gas, and cyanogen, the special lines due to the metal were produced, but in each a series of identical lines appeared, and these new lines were referable to the carbon contained in each of these gases. Each gas exhibits special lines which are continued across the spectrum, and are never interrupted like those of the metals.

The author observed that many of these gases, such as protoxide of nitrogen, hydrochloric and sulphurous acid, presented a considerable obstacle to the passage of the sparks from the induction-coil.

X. "On the Long Spectrum of Electric Light." By Professor George G. Stokes, M.A., Sec. R.S. &c. Received June 19, 1862.

(Abstract.)

The author's researches on fluorescence had led him to perceive that glass was opaque for the more refrangible invisible rays of the solar spectrum, and that electric light contained rays of still higher refrangibility, which were quite intercepted by glass, but that quartz transmitted these rays freely. Accordingly he was led to procure prisms and a lens of quartz, which, when applied to the examination of the voltaic arc, or of the discharge of a Leyden jar, by forming a pure spectrum and receiving it on a highly fluorescent substance, revealed the existence of rays forming a spectrum no less than six or eight times as long as the visible spectrum. This long spectrum, as formed by the voltaic arc with copper electrodes, was exhibited

at a lecture given at the Royal Institution in 1853; but the author, for reasons he mentioned, did not then further pursue the subject. Having subsequently found that the spark of an induction-coil with a Leyden jar in connexion with the secondary terminals yielded a spectrum quite bright enough to work by, he resumed the investigation, and examined the spectra exhibited by a variety of metals as electrodes, as well as the mode of absorption of the rays of high refrangibility by various substances. The spectra of the metals may be viewed at pleasure by means of fluorescence, and the mode of absorption of the invisible rays by a given solution may be at once observed; but there are difficulties attending the preparation in this way of sufficiently accurate maps of the metallic lines; and the great liability of the rays of high refrangibility to be absorbed by impurities present in very minute quantity renders the certain determination of the optical character, in this respect, of substances which are only moderately opaque a matter of considerable difficulty. Having found that Dr. Miller had been engaged independently at the same subject, working by photography, the author deemed it unnecessary to attempt a delineation of the metallic lines (for which, however, he has recently devised a practical method that was found to work satisfactorily), or to examine further the absorption of rays of high refrangibility by solutions of metallic salts, &c.

The present paper contains therefore mainly results obtained in other directions in the same wide field of research. Among the metals examined, the author had found aluminium the richest in invisible rays of extreme refrangibility; and accordingly aluminium electrodes were employed when the deportment of such rays had to be specially examined. As the bright aluminium lines of high refrangibility do not appear to have been taken by photography, a drawing of the aluminium spectrum is given, with zinc and cadmium for comparison.

The author has also described and figured the mode of absorption of the invisible rays by solutions of various alkaloids and glucosides. Bodies of these classes, he finds, are usually intensely opaque, acting on the invisible spectrum with an intensity comparable to that with which colouring matters act on the visible. This intensity of action causes the effect of minute impurities to disappear, and thereby increases the value of the characters observed. It very often happens that at

some part or other of the long spectrum a band of absorption, or maximum of opacity, occurs; and the position of this band affords a highly distinctive character of the substance which produced it.

Among natural crystals, besides the previously known yellow uranite, the author found that in adullaria, and felspar generally, a strong fluorescence is produced under the action of the rays of high refrangibility, referable not to impurities, but to the essential constituents of the crystal. A particular variety of fluor-spar shows also an interesting feature, though in this case referable to an impurity, exhibiting a well-marked reddish fluorescence under the exclusive influence of rays of the very highest refrangibility. This property renders such a crystal a useful instrument of research.

With some metals broad, slightly convex electrodes were found to have a great advantage over wires, exhibiting the invisible lines far more strongly, while with some metals the difference was not great.

The blue negative light formed when the jar is removed, and the electrodes are close together, was found to be exceedingly rich in invisible rays, especially invisible rays of moderate refrangibility. These exhibited lines independent of the electrodes, and therefore referable to the air. This blue light has a very appreciable duration, and is formed by what the author calls an arc discharge.

The paper concludes with some speculations as to the cause of the superiority of broad electrodes, and of the heating of the negative electrode.

XI. "On the Reflexion of Polarized Light from Polished Surfaces." By the Rev. SAMUEL HAUGHTON, M.A., F.R.S., Fellow of Trinity College, Dublin. Received June 9, 1862.

(Abstract.)

When a plane-polarized beam of light is incident on a polished surface at a certain angle of incidence, and polarized in a certain azimuth, the reflected beam of light is circularly polarized.

The tangent of this angle of incidence is called by the author the Coefficient of Refraction, and upon it appears to depend the *brilliancy* of a polished surface.

The cotangent of the azimuth of incident polarization is called the Coefficient of Reflexion, and upon it appears to depend the rich *lustre*, strikingly exhibited in polished copper and gold.

The paper contains an account of the experiments made to determine, with precision, these constants for the following substances:—

A. *Transparent Bodies.*

1. Munich glass (*a*).
2. Munich glass (*b*).
3. Paris glass.
4. Fluor-spar.
5. Glass of antimony.
6. Quartz crystal.

B. *Pure Metals.*

1. Silver.
2. Gold.
3. Mercury.
4. Platinum.
5. Palladium.
6. Copper.
7. Zinc.
8. Lead.
9. Bismuth.
10. Tin.
11. Iron and steel.
12. Aluminium.

C. *Alloys.*

1. Copper and tin (speculum metal).
2. Copper and zinc ($10\,Cu+Zn$).
3. " " ($9\,Cu+Zn$).
4. " " ($8\,Cu+Zn$).
5. " " ($7\,Cu+Zn$).
6. " " ($6\,Cu+Zn$).
7. " " ($5\,Cu+Zn$).
8. " " ($4\,Cu+Zn$).
9. Copper and zinc ($3\,Cu+Zn$).
10. " " ($2\,Cu+Zn$).
11. " " ($Cu+Zn$).
12. " " ($Cu+2\,Zn$).
13. " " ($Cu+3\,Zn$).
14. " " ($Cu+4\,Zn$).
15. " " ($Cu+5\,Zn$).

The determination of the optical constants of these substances leads to many interesting conclusions; among which the following may be stated:—

1. That transparent bodies, as well as metals, possess a coefficient of reflexion, which is sometimes very sensible, although there are bodies in which it is very small.

2. That *Silver* is the only substance which possesses the qualities of *brilliancy* and *lustre*, represented by the coefficients of refraction and reflexion, in a high degree.

3. Of the metals which have high *brilliancy* and little *lustre* may be named *Mercury*, *Palladium*, *Zinc*, and *Iron*.

4. Of the metals which have high *lustre* and little *brilliancy* there are only two, *Gold* and *Copper*.

5. Results of the highest interest appear from an examination of

the optical constants of the alloys of copper and zinc, which cannot be given in an abstract.

6. In the details of the several experiments, the author calls attention to several remarkable laws, or indications of laws, which appear to him to require some notice from theorists.

a. When the azimuth of the incident beam is less than the circular limit, the axis major of the reflected ellipse, at the principal incidence, lies in the plane of incidence; but when the azimuth is greater than the circular limit, it is perpendicular to the plane of incidence, and as the incidence varies, the axis major twice approaches to a minimum distance from that plane.

b. There appears to the author to be some indication in the experiments on metals, that the quantity known to theorists as $\left(\frac{J}{I}\right)$ is not a function of the incidence only; a conclusion which, if correct, would require the intervention of a third wave suppressed, or some such theoretical supposition, to account for it.

XII. "On the Loess of the Valleys of the South of England and of the Somme and the Seine." By Joseph Prestwich, Esq., F.R.S. Received June 19, 1862.

(Abstract.)

In this paper the author takes up and discusses a point connected with the former inquiry, but postponed in the paper he read before the Royal Society in March last, a recent visit to France having led him to form a conclusion with regard to the origin of the Loess sooner than he then expected.

On that occasion he referred the loam and brick-earth, with land and freshwater shells, which occurs in the valleys and on many of the hills in the South of England and North of France, to temporary inundations of the old rivers. On the present occasion he shows that this deposit is intimately connected with the origin of the river-valleys and with the fluviatile high- and low-level gravels described in his last paper.

Reference is first made to the Loess of the valley of the Rhine, and the author accepts Sir Charles Lyell's explanation that it is the result of a river-deposit; but he does not agree in the explanation as to the mode which led to the actual results, so far as the present district is concerned.

One difficulty in understanding the spread of the loess in England and France has always been the greatly different levels on which it occurs, being present in the bottom of the valleys, and occurring on ground 100, 200, and 300 feet higher. This evidently places it beyond the reach of inundations with the valleys formed as they are at present, and the prior origin of which the common covering of loess might lead one at first to infer. But if, instead of starting at the present low levels, the valleys be taken at the level the author showed them to have had at the period of the upper high-level gravels, it will give a base for the original river-levels of 100 to 200 feet above the existing valleys, and therefore it will reduce the difference of level of the higher deposits of loess to be accounted for, to 100 or 150 feet. In many cases it is less, but it is still considerable. It thus brings the whole of the loess within the possible range of inundations of the old Post-pliocene rivers at different periods of their age; the higher beds of loess having been deposited during floods at an early period, and before the excavation of the present river-valleys, and the lower beds having been deposited after the excavation of the valley, and while some of the old meteorological conditions still prevailed.

The author shows that the loess is, in fact, like the high- and low-level gravels, always connected with river-valleys, although it extends much beyond the limits of these beds, rising to much higher levels, and extending far beyond their limits. He then shows that in the valley of the Somme, the difference between the highest levels of the loess and the upper gravels thus becomes reduced to 60 or 80 feet; in the Oise, to apparently about 50 feet; in the Seine valley, to about 120 to 150 feet; and in the Bresle valley to 70 feet.

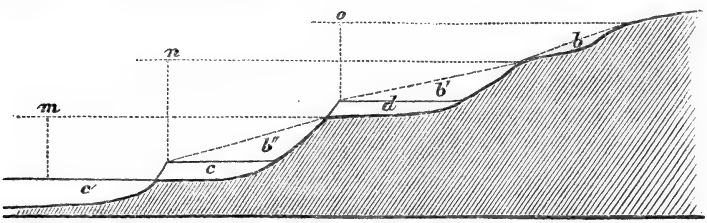
The loess contains the same mammalian remains and the same species of land mollusca as the gravels. Of freshwater mollusca it contains hardly any.

Notwithstanding the extension of the loess over the higher grounds flanking the river-valleys, still such grounds are always bounded by

higher hills, which seem to have formed barriers to its further spread. It seems, therefore, that although the connexion of these several and distinct deposits is, owing to their irregular and wide spread, not always apparent, it is probable that they are related to the same phenomena, and that they present two phases of causes having a common and contemporaneous origin.

In all rivers subject to floods, three forms of sediment will be deposited: first, gravel and shingle in the more direct channel; secondly, sand in the more sheltered places; thirdly, fine silt where the flood-waters are at rest out of the direct channel.

In such manner the author conceives the high- and low-level gravels and the loess of all the levels to have been formed.



b' b' b''. Representing the Loess.
d. A high-level gravel.
c' c'. Lower-level gravel.
m, n, o. The levels to which the river rose during inundations at different periods.

If, therefore, the flood-water origin of the loess be admitted, it follows that, as it is found rising from 50 to 100 feet above the highest bed of the fluviatile gravels which mark the channels of the old rivers, it gives a measure of the magnitude of the floods of that period, showing that they rose at times 50 to 100 feet above their summer low levels; like, in fact, the rivers in arctic regions, but to a greater extent. Such conditions show their great erosive power, and furnish the evidence wanting on the former occasion to prove that such greater power had existed. Though a greater rainfall was inferred from other causes, this more direct evidence was wanting.

The author mentions his discovery, on the occasion of his last visit to Paris, of freshwater shells of the genera *Limneus* and *Valvata* at two places in the low-level gravels of Paris, and again at Rouen. He also gives a section of some remarkable contortions, which he refers to ice-action, in the high-level gravel of Charonne.

The fluviatile origin of the different gravels, as well as the greater action of ice at the higher levels, is therefore confirmed, as is also the suggestion that the volume of water carried down at the period in question by the rivers was infinitely greater than it now is. At the same time the view now given both explains the origin of the loess, so long an unsettled problem, and harmonizes with the hypothesis before advanced in explanation of the accompanying general phenomena.

XIII. "On the Simultaneous Distribution of Heat throughout superficial parts of the Earth." By Professor H. G. Hennessy, F.R.S. Received June 19, 1862.

(Abstract.)

The principal object of this memoir is to develope the laws of the distribution of temperature in the portion of the atmosphere in contact with the earth, and to point out the connexion between the phenomena of aërial temperature and those of soil and oceanic temperature. The author maintains that hitherto no perfect physical representation of the distribution of heat over the earth's surface has been obtained. Humboldt's luminous method of representing the distribution of mean temperatures necessarily presents us with the temperatures of places at those hours of local time when the temperature happens to be equal to that of the entire day. But such hours occur at different places not at the same moment of absolute time, and therefore the isothermal lines traced by the aid of their results alone, are not true isothermal lines in the same sense as we understand an isothermal line or surface within crystals, or other definite geometrical solids which have been recently the subjects of thermological inquiry.

The distribution of sunshine at the outer limits of the atmosphere and at its base is first considered, and the nearly circular shape of the lines of equal sunshine is pointed out. After showing the connexion between these lines and the simultaneous isothermals for the air, land, and water, the author proceeds to more particularly discuss the aërothermal lines. As the term isothermal line has become universal in the sense of a line joining places possessing the same mean temperatures, the author proposes to designate the true lines of si-

multaneous equal temperature as synthermal lines. If any number of places have the same temperature at a given hour corresponding to the mean time of any one meridian, these places will be synthermal, and a line joining them will be a synthermal line. For this purpose the meridian of Greenwich has been selected, and a series of synthermal Tables have been calculated for different places corresponding to the Greenwich hours. For the construction of these Tables, the hourly observations of temperature made at the British Home and Colonial Observatories, the observations of Russia, Austria, Prussia, and Central Europe, as well as those of the United States, have been employed. The few series of hourly observations made by Arctic and African travellers have been also applied; and in addition to the Tables thus directly constructed, others have been deduced by interpolation for stations whose geographical position rendered it desirable to bring them into the general view of temperature-distribution. All results expressed in Centigrade and Reaumur degrees have been reduced to the Fahrenheit scale. A fresh set of Tables has been formed from those corresponding to local time, with hours corresponding to the meridian at Greenwich.

The synthermal Tables thus obtained show, as might be *à priori* expected, still greater differences between the temperatures of places in the same parallels of latitude than the Tables of mean temperature. Thus Rome and Tiflis differ in latitude by only 13′, and the mean temperature of Rome is 5°·1 in excess of that of Tiflis. At 8 A.M. Greenwich time, they are synthermal, both possessing the temperature of 59°·1, while at 7 A.M. Tiflis surpasses Rome by 0°·6, and at all other times besides these Rome surpasses Tiflis. At 4 A.M. this excess amounts to 9°·5. Although Pekin is situated in the isothermal line which passes close to the Isle of Wight, it is synthermal at 5 A.M. (Greenwich) to some place 6° warmer than Rome, and probably therefore on the north coast of Africa, and is synthermal with a point north of the Orkneys at between 8 and 9 in the evening. Similar comparisons of distant places in both hemispheres lead to similar results. It appears that during certain periods of the day, alternately hot and cold spaces exist in the interior of the continents compared to the surrounding oceans. In the southern hemisphere the rising of synthermal temperatures appears to be a little inferior to what it is in the northern, if we compare together stations

with nearly corresponding latitudes and differences of longitude in both hemispheres.

From the results tabulated in his synthermal Tables, the author has projected on an equatorial map of the world, the synthermal lines of 4 A.M. and 2 P.M. Greenwich time. This map clearly exhibits the risings of the synthermals, and the existence of spaces of maximum temperature. The synthermals in both hemispheres rise towards the poles opposite these spaces, and converge towards the space of minimum tropical temperature. In islands circumstanced like the British Isles, the synthermals may be represented by two systems of closed curves, one for the day with an interior space of maximum temperature, and the other for the night with an interior space of minimum temperature. These groups would be connected somewhat in the way of the magnetic curves delineated by Gauss in his Theory of Terrestrial Magnetism (Taylor's Scientific Memoirs, vii.). The shapes of these groups would closely resemble the isothermals already published by the author*, and which, from the small differences of longitude in our islands, may be conceived to represent very closely the synthermals of 9 A.M. and 8 P.M.

The probable shapes of the lines of equal soil temperature, or syngeothermals as the author calls them, are next considered; and it is shown that they must not only present far more remarkable deviations from equatorial parallelism than the synaërothermals, but also that their diurnal rising must be very considerable.

The author points to the connexion between some of his results and the diurnal law of the wind force discovered by Mr. Osler; and he also shows how the abnormal regressions of temperature in the latter months of spring may be partly explained by the circumstance that, although the isothermals of mean temperature during these months do not deviate widely from equatorial parallelism, the synthermals not only swing to a greater extent than during most of the other months of the year, but that they are also more closely crowded together.

These results are most strikingly developed during the month of May.

* Proceedings, vol. ix., and Atlantis, vol. i.

XIV. "On the Differential Coefficients and Determinants of Lines, and their application to Analytical Mechanics." By A. Cohen, Esq. Communicated by Professor Stokes, Sec. R.S. Received May 8, 1862.

(Abstract.)

1. The object of this paper is to develope a new method of proving and extending the formulæ of analytical mechanics, and at the same time to show how the different steps *themselves*, in the analytical work by which those formulæ are generally arrived at, exactly correspond to mechanical or geometrical facts, just as it is shown in modern geometry that the various equations of analytical geometry are capable of important interpretation.

2. If OA and OB be two straight lines drawn from the origin O, then for well-known reasons AB may be called "the complete difference" of OA and OB, and may be denoted by (OB)−(OA). Similarly, if OA and OB represent two successive states of a variable line P at times t and $t+at$, then the line whose direction is the limiting direction of AB, and whose magnitude is the limit of $\frac{\mathrm{AB}}{at}$, will be called "the complete differential coefficient" of P, and will be denoted by $\mathrm{D}_t(\mathrm{P})$.

3. It is easy to see that the line which represents, in magnitude and direction, a particle's velocity is the complete differential coefficient of the particle's radius vector, and that, similarly, the line representing the particle's acceleration is the complete differential coefficient of the line representing the velocity, and is therefore the second differential coefficient of the radius vector.

In this case, therefore, Kinematics may be considered as the calculus of the first and second differential coefficients of lines.

4. The following is the fundamental theorem concerning the differential coefficients of lines: the differential coefficient of a line is compounded of what would be the differential coefficient if the *length alone* varied, and of what would be the differential coefficient if the *direction alone* varied. Supposing the line to move in one and the same plane, it is easy to deduce from that theorem the expression for a line's differential coefficient, and by applying to the component

parts of that expression the same theorem again, we arrive at the elements of which a line's second differential coefficient is composed. Moreover, since those first and second differential coefficients are the representatives of a particle's velocity and acceleration, we are led by these investigations at once to all the analytical formulæ for motion in one plane relatively to moving axes, and are at the same time enabled to give to those formulæ a very simple interpretation.

5. I conclude the first chapter by indicating how all the results thus obtained may be made to flow from the ordinary mode of representing lines by means of imaginary quantities. For instance, by differentiating the expression $re^{\theta\sqrt{-1}}$ twice successively, we obtain

$$\frac{d^2r}{dt^2}-r\left(\frac{d\theta}{dt}\right)^2+\sqrt{-1}\,\frac{1}{r}\frac{d}{dt}\left(r^2\frac{d\theta}{dt}\right),$$

which expression is evidently compounded of the radial and transversal accelerations of a particle.

6. On passing to the general case of a line moving in space, a new conception has to be introduced—one, however, which presents itself, more or less disguised, in almost all the ordinary formulæ of statics and dynamics. Let OA and OB be two straight lines; draw OD perpendicular to and equal to twice the area of the triangle AOB; then OD may be called "the determinant" of OA and OB, inasmuch as its projections on three axes of coordinates are the simplest *determinants* that can be found with the coordinate projections of OA and OB. Moreover, if OD be drawn in such a direction that to an eye looking along DO the rotation from OA to OB appears a positive rotation, OD will be called the determinant of OA to OB, and may be denoted by

det (OA, OB).

7. The connexion of the determinant, as above defined, with the axis of a couple and with statics in general is self-evident. Nor is its connexion with dynamics less intimate; for if OA represent in magnitude and direction the angular velocity with which, and the instantaneous axis about which, OB is revolving at time t, then the linear velocity of a particle at B, the extremity of OB, is represented in magnitude and direction by det (OA, OB).

8. Having thus defined the determinants of lines, I proceed to prove a few fundamental propositions concerning them, which will often be found very useful in abbreviating and giving a clear meaning to complicated analytical work. Moreover, those propositions indi-

cate a remarkable symbolical resemblance between det (P, Q) and the product PQ. For instance, it may be proved that if P, P′, Q be any three straight lines, then the resultant of det (P, Q) and det (P′, Q) is det (P+P′, Q), where P+P′ denotes the resultant or complete sum of P and P′.

Again, it may be shown that the complete differential coefficient of det (P, Q) is exactly similar in form to the differential coefficient of the product P, Q.

9. Furnished with these propositions, it is easy to extend the formulæ of the first chapter to lines moving in space of three dimensions.

Let Λ denote the instantaneous axis about which, and the angular velocity with which, a line R, whose length at time t is r, revolves, then the complete differential coefficient of R is the resultant of $\frac{dr}{dt}$ in the direction of R, and the determinant of Λ to R, or

$$D_t(\mathrm{R}) = \left(\frac{dr}{dt} \parallel \text{to } \mathrm{R}\right) + \det(\Lambda, \mathrm{R}).$$

This is the fundamental proposition concerning the differential coefficient of a line. Applying it to the component parts of the last formula, we obtain a somewhat remarkable expression for the second differential coefficient of a line, and therefore also for a particle's acceleration. That expression easily leads to the formulæ for the acceleration of a particle relatively to any moving axes, and also to a very simple proof of Corioli's beautiful theorem concerning relative motion. The use of this method is illustrated by showing how it enables us at once to write down the equations for the motion of the simple pendulum, taking the earth's rotation into account.

10. I now pass to the dynamics of a rigid body.

Compounding the momenta of the different particles of a body as if they were forces, they may be reduced to a single momentum at O and a couple of momenta. The former I call the body's single momentum, and denote it by U; the latter I call the body's momentum couple, and denote it, or rather denote its axis, by H. If now the external forces acting on the body be similarly reduced to a force P at O, and a couple whose axis is G, it may be shown that D'Alembert's principle is contained in the proposition "that P and G are respectively the complete differential coefficients of U and H."

11. U and H, the body's single momentum and momentum couple, may be found without difficulty in the ordinary way. In the case of a rigid body moving about a fixed point O, if A, B, C be the moments of inertia, and ω_x, ω_y, ω_z be the angular velocities of rotation about the principal axes O_x, O , O_z, then H is the resultant of $A\omega_x$, $B\omega_y$, $C\omega_z$. Let then Λ denote the instantaneous axis and angular velocity of rotation, then D_t (H) is, by one of our fundamental theorems, equivalent to det (Λ, H), together with

$$\frac{d}{dt}(A\omega_x) \parallel \text{to } O_x, \quad \frac{d}{dt}(B\omega_y) \parallel \text{to } O_y, \quad \frac{d}{dt}(C\omega_z) \parallel \text{to } O_z,$$

and by D'Alembert's principle D_t (H) is the axis of the resultant couple formed by transferring to O all the external forces.

12. The last theorem includes Euler's equations, and the different extensions which those have of late received. I have attempted to show that, in solving mechanical problems, the above theorem will be generally found more useful than any of those equations. Moreover, it serves to explain the geometrical reason, independently of Euler's equations, why the results are so much simplified in the case of two of the principal moments of inertia being equal to one another.

13. In order to illustrate how advantageously and how completely the most complicated formulæ of dynamics may be interpreted, I have given a direct analytical proof of Euler's equations, and have then shown that the consideration of each step of the analytical work leads to an extremely short demonstration of the same equations by means of the theory of the differential coefficients and determinants of lines.

XV. "On the Theory of Probabilities." By George Boole, Esq., F.R.S. Received May 21, 1862.

(Abstract.)

This paper has for its object the investigation of the general analytical conditions of a method for the solution of questions in the Theory of Probabilities, which was published in a work entitled "An Investigation of the Laws of Thought, &c."*

The application of the method to particular problems has been

* London, Walton and Maberly, 1854.

illustrated in the work referred to, and more fully in a Memoir published in the Transactions of the Royal Society of Edinburgh, entitled "On the Application of the Theory of Probabilities to the Question of the Combination of Testimonies or Judgments." The latter contains also the foundations of the general analytical theory of the method. But the complete development of that theory is attended with mathematical difficulties which I have only just succeeded in overcoming.

A correspondence on the subject between Mr. Cayley and myself also appears in the current (May) Number of the 'Philosophical Magazine,' and I owe it to Mr. Cayley that these further researches, of the results of which an account will here be given, were undertaken.

I shall make but few remarks here upon the *à priori* grounds of the method. Generally it may be said that the solution of a question in the Theory of Probabilities depends upon the possibility of mentally constructing the problem from hypotheses which appear, whether as a consequence of our knowledge or as a consequence of our ignorance, to be simple and independent. When the data are the probabilities of simple events, and no conditions are added, the problem is in theory sufficiently easy, the sole difficulty consisting in the calculation of complex combinations. But when the data are the probabilities of compound events, or when the events are connected by absolute conditions expressible in logical propositions, or when both these circumstances are present, the difficulty of the required mental construction becomes greater. If we assume the independence of the simple events from which the compound events according to their expression in language are formed, we meet, first, the difficulty that the number of equations thus formed may be greater or less than that which is requisite to obtain a solution; and, secondly, the far more fundamental difficulty that the conditions under which the solution, supposing it to be obtained, is analytically valid may not coincide with the conditions under which the data are possible. It seems indeed likely—at any rate the evidence of particular examples points uniformly to the conclusion—that any attempts to construct the problem upon hypotheses which, while not involved in the actual data are of the same nature as those data (*i. e.* which might conceivably have resulted as facts of observation from the same experience from which

the data were derived), *limit* the problem, and lead to solutions which are analytically valid under conditions narrower than those under which the data are possible.

But the processes of mathematical logic enable us, without any addition to the actual data, to effect the required construction of the problem formally—formally because the hypotheses which are regarded as ultimate and independent in that construction refer to an ideal state of things. The nature of the conceptions employed, and their connexion with the conceptions involved in the actual statement of the problem, are discussed in the paper. It is sufficient to say here that, whatever difficulty there may be in these conceptions as conceptions, there is nothing arbitrary in the formal procedure of thought with which they are connected. The probabilities of the ideal events enter as auxiliary quantities into the process of solution, and disappear by elimination from the final result, but they are throughout treated as probabilities, and combined according to the laws of probabilities. I will only say here that the difficulty which has been felt in the conception of the ideal events appears to me to arise from a misdirected attempt to conceive those events by means of the events in the statement of the problem—the true order of thought being that the events in the statement of the problem are, not indeed in their material character, but as subjects of probability and of relations affecting probability, to be conceived by means of the ideal events.

Now the probabilities which constitute the actual data will in general be subject to conditions in order that they may be derived from actual experience. Those conditions admit of mathematical expression.

Generally, if the events in the data are all or any of them compound, and if $p_1, p_2, \ldots p_n$ represent their probabilities, those quantities will be subject to certain conditions, expressible in the form of linear equations or *inequations*, beside the condition that, as representing probabilities, they must be positive proper fractions. All such conditions of either kind are ultimately expressible in the general form

$$b_1 p_1 + b_2 p_2 \ldots + b_n p_n + b \gtreqless 0,$$

the coefficients $b_1, b_2, \ldots b_n, b$ differing in the different conditions so as to indicate that each of the quantities $p_1, p_2, \ldots p_n$ varies between

a system of inferior limits expressed by linear functions of the other quantities, and a system of superior limits also so expressed.

Thus, if A, B, C represent any simple events, and if p_1 represent the probability of the concurrence of B and C, p_2 that of the concurrence of C and A, p_3 that of the concurrence of A and B, then p_1, p_2, p_3 must, in order that they may be derived from experience, satisfy the conditions

$$p_1 \overline{\overline{>}} p_2+p_3-1, \quad p_2 \overline{\overline{>}} p_3+p_1-1, \quad p_3 \overline{\overline{>}} p_1+p_2-1,$$

as well as the conditions implied in their being positive proper fractions.

On the other hand, the ideal events being by hypothesis simple and independent, the auxiliary quantities which represent their probabilities will be subject to no other condition *à priori* than that of being positive proper fractions—to no other condition *à priori*, because their actual values are determined in the process of solution.

Now the most general results of the analytical investigation are—

1st. That the auxiliary quantities representing the probabilities of the ideal events admit of determination as positive proper fractions, and, further, of a single definite determination as such, precisely when the original data supply the conditions of a possible experience.

2ndly. That as a consequence of this the probability sought will always lie within such limits as it would have had if determined by actual observation from the same experience as the data.

The proof of these propositions rests upon certain general theorems relating to the solution of a class of simultaneous algebraic equations, and, auxiliary to this, to the properties of a functional determinant.

The following are the principal of those theorems :—

1st. If the elements of any symmetrical determinant are all of them linear homogeneous functions of certain quantities $a_1, a_2, \ldots a_r$ —if the coefficients of these quantities in the terms on the principal diagonal of the determinant are all positive—and if, lastly, the coefficients of any of these quantities in any row of elements are proportional to the corresponding coefficients of the same quantity in any other row, then the determinant developed as a rational and integral function of the quantities $a_1, a_2, \ldots a_r$ will consist wholly of positive terms.

And, as a deduction from the above,

2ndly. If V be a rational and entire function of any quantities $x_1, x_2, \ldots x_n$, involving, however, no powers of those quantities, and all the coefficients being constant, and if in general V_i represent the sum of those terms in V which contain x_i as a factor, and V_{ij} the sum of those terms in V which contain the product $x_i x_j$, then the determinant

$$\begin{vmatrix} V & V_1 & V_2 & \ldots & V_n \\ V_1 & V_{11} & V_{12} & \ldots & V_{1n} \\ V_2 & V_{21} & V_{22} & \ldots & V_{2n} \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ V_n & V_{n1} & V_{n2} & \ldots & V_{nn} \end{vmatrix}$$

will on development consist wholly of positive terms.

3rdly. The definitions being as above, and the function V being in form complete, *i. e.* containing all the terms which by definition it can contain, the system of simultaneous equations

$$\frac{V_1}{V}=p_1, \quad \frac{V_2}{V}=p_2, \ldots \frac{V_n}{V}=p_n,$$

in which $p_1, p_2, \ldots p_n$ represent positive proper fractions, will admit of one, and only one, solution in positive integral values of $x_1, x_2, \ldots x_n$.

4thly. The function V being incomplete in form, *i. e.* wanting some of the terms which it might by definition contain, the system of equations

$$\frac{V_1}{V}=p_1, \quad \frac{V_2}{V}=p_2, \ldots \frac{V_n}{V}=p_n$$

will admit of one, and only one, solution in positive integral values of $x_1, x_2, \ldots x_n$, provided that $p_1, p_2, \ldots p_n$, beside being proper fractions, satisfy certain conditions depending upon the actual form of V.

These conditions are expressible by linear equations or inequations of the general form,

$$b_1 p_1 + b_2 p_2 \ldots + b_n p_n + b \geqq 0.$$

In the application to the theory of probabilities, the form of the function V depends upon the explicitly determined logical connexion of the events in the data; the equations or inequations of condition correspond to the conditions of possible experience as a source of the data.

It appears, therefore, that, quite independently of any question of the validity of the logical, and I ought perhaps to add philosophical,

grounds of the method, it is a perfect method of interpolation. The analytical investigation, however, shows that, for the mere purpose of interpolation, the process might be modified by altering the coefficients of V without affecting its form; but it indicates at the same time that such modifications have no definite analogy with that process by which weight is assigned to astronomical observations, and, from their arbitrary character, lead to results which cannot properly be regarded as expressions of probability in any sense.

XVI. "On Simultaneous Differential Equations in which the number of Variables exceeds by more than unity the number of the Equations." By GEORGE BOOLE, Esq., F.R.S. Received June 19, 1862.

(Abstract.)

This paper contains the proof, with some applications, of a method described in a paper bearing nearly the same title which was published in the 'Proceedings of the Royal Society' for March 6, 1862.

XVII. "On the Calculus of Symbols."—Third Memoir. By W. H. L. RUSSELL, Esq., A.B. Communicated by Professor STOKES, Sec. R.S. Received June 18, 1862.

(Abstract.)

The following paper is a continuation of the two preceding Memoirs on the same subject. It has a fourfold object. In the first place, I calculate the general values of the coefficients in the Binomial Theorem given in the first Memoir. In the next place, I give an expression for the form of the coefficient of the general term of the multinomial theorem as previously explained. I then give a theorem for the multiplication of symbolical factors emanating from each other after a given law; and lastly, I investigate a binomial theorem, reciprocal to the binomial theorem already considered.

XVIII. "On the Properties of Electro-deposited Antimony" (concluded). By George Gore, Esq. Communicated by Professor Stokes, Sec. R.S. Received May 24, 1862.

(Abstract.)

In this communication the author has described two additional kinds of electro-deposited antimony possessing the property of evolving heat; one of them is obtained from a solution of bromide, and the other from a solution of iodide of antimony; there is also given additional information respecting the peculiar heating-antimony obtained from the aqueous terchloride.

The following is a brief statement and comparison of some of the properties of the three kinds of thermically active antimony. The specific gravity of the chloride deposit is 5·8, the bromide one 5·44, and the one from the iodide 5·25. The amount of heat evolved is greatest with the one from the chloride solution, and least with that from the iodide; the former evolves all its heat at 60° Fahr., by contact with a red-hot wire, the bromide one at 280° Fahr., whilst the iodide one requires a temperature of 340° Fahr.; the latter also acquires a reddish-brown colour by exposure to solar light.

The chloride deposit contains about 6·3 per cent. of saline matter, the bromide one about 20, and the one from the iodide liquid about 22·2. The quantities deposited by a single equivalent of zinc were about 42·5 in the chloride, 50 in the bromide, and 51 in the iodide solution.

The explanation proposed of their formation is, that the antimony in depositing, being in the "nascent" state, combines chemically in a feeble manner with the saline ingredients of the electrolyte; but the complete sources of the evolved heat remain undecided.

XIX. "On the Sulphur-Compounds in Purified Coal-Gas, and on Crystallized Hydrosulphocarbonate of Lime." By the Rev. W. R. Bowditch, B.A., F.C.S., Wakefield. Communicated by Professor William Thomson, M.A. Received June 14, 1862.

The following facts relative to sulphur in what is called purified coal-gas, are additional to those already submitted to the Royal Society.

When I first made known the action of heated lime upon coal-gas, chemists accounted for the phenomena observed by two assumptions:—I. That the sulphur-compound decomposed was free bisulphide of carbon. II. That the decomposition was due to a reaction between water and bisulphide according to the following equation:

$$CS_2+2HO=2HS+CO_2.$$

It was easy to show that these assumptions were erroneous, but exceedingly difficult to demonstrate the truth.

That the sulphur in gas does not usually exist as free bisulphide of carbon is proved thus.

Gas which has been purified at the gas-works by lime, and which contains 20 or 30 grains of sulphur in 100 cubic feet, may be passed for a considerable time through a tube containing cold slaked lime without producing discoloration; but if the same gas be charged with a minute quantity of bisulphide-of-carbon vapour and passed through the same lime-tube, the lime becomes yellow and green from decomposition of the bisulphide of carbon.

If instead of passing the gas through lime it be passed through triethylphosphine, the beautiful red crystals which this base gives with bisulphide of carbon are not formed; but if the base be dissolved in alcohol or ether, and the gas passed through this solution, the red crystals are formed, as Dr. Hofmann first proved. The alcohol or ether dissolves out the bisulphide of carbon from the hydrocarbon compounds of which it forms a part; and when it is thus dissolved it reacts with triethylphosphine.

Naphthalin, benzole, and other fluid hydrocarbons condensed from purified gas yield sulphide of hydrogen and other sulphuretted compounds by simple distillation, yet these do not produce the well-known red crystals with triethylphosphine. They may, moreover, be digested for weeks in an alkaline solution of oxide of lead without producing any sulphide of lead. Under similar treatment bisulphide of carbon yields hydrosulphocarbonate and sulphide of lead in a few hours.

If the sulphuretted fluid hydrocarbons condensed from gas be mixed with ammoniacal alcohol and heated, and an alcoholic solution of acetate of lead be then added, a black precipitate is formed after some time, which evolves sulphide of hydrogen upon addition of an acid. In this case neither hydrosulphocarbonate nor hydro-

sulphocyanide of ammonia is formed; yet it is well known that both are formed when bisulphide of carbon is added to ammoniacal alcohol. The erroneous view of the action of heated hydrate of lime upon the sulphur-compounds in gas arose, I think, from the generally received opinion that the blackening of lead-salts by a gas is a proof that that gas is sulphide of hydrogen—joined to the fact that sulphide of hydrogen is one of the compounds produced by the action of the heated lime. If the sulphide of hydrogen had been separated from the mixture of gaseous compounds produced, the truth would have been apparent; but as, I believe, all experimenters have failed to separate them, the subject was obscure. After having failed in many processes devised by myself and suggested by others, I at last removed the sulphide of hydrogen, and showed that the blackening of lead-salts is no proof of the presence of sulphide of hydrogen. Ordinary purified gas was passed over heated hydrate of lime, then through a considerable quantity of well-washed hydrated peroxide of iron, over lead-paper, and subsequently through moist slaked lime. The peroxide of iron was slightly blackened, and withdrew every trace of sulphide of hydrogen: the lead-paper became black, and the slaked lime yellow. This yellow lime gave a primrose-coloured solution with water, which precipitated lead- and silver-salts brownish red, thus showing the presence of impure hydrosulphocarbonate of lime.

To be certain of the absence of sulphide of hydrogen, some of the yellow lime was treated with hydrochloric acid, and the gases evolved thereby were conducted into a solution of potash. The potash solution gave no reaction with nitroprusside of sodium, showing the absence of sulphide of hydrogen, and when boiled with nitric acid gave no precipitate with a salt of baryta.

The hydrochloric solution of the lime contained a sulphur-salt, which was obtained as sulphate of lime when nitric acid was added and the whole was boiled.

The blackening of the lead-paper in this case could not be due to sulphide of hydrogen, as that compound was absent. Nor I think is it due to the hydrosulphocarbonic acid which passed over, and in contact with, the lead-paper, and was arrested by the lime. The red compound which this acid produces with lead-salts is said to turn black rapidly; and the red compound produced by a lead salt, and those of Berzelius and Zeise, undoubtedly does so blacken, as also

does that produced by a salt of lead and an alkaline pentasulphide. I have, however, obtained a red lead-salt by the reaction of crystallized hydrosulphocarbonate of lime and basic acetate of lead, which remains red after drying in the air at ordinary temperatures and exposure for weeks to the free atmosphere. I therefore conclude that the blackening of the lead-paper in the above case was not due to hydrosulphocarbonic acid, but to some unknown or unsuspected compound.

In order to understand this matter fully, I commenced some investigations into the reactions of bisulphide of carbon with metallic oxides and other compounds, a portion of which I have now the pleasure of submitting.

Red Crystallized Lime-Salt.—Slaked lime and bisulphide of carbon are mixed in a close vessel, and allowed to stand for three or four days. The lime at first becomes of a pale primrose-colour, which gradually deepens to a fine lemon-yellow. Water added to the yellow lime gives a solution of a gold-colour, which precipitates salts of lead and silver reddish brown, and salts of mercury brown. These precipitates become black upon standing a short time. If the yellow solution be allowed to remain for a few days in contact with the lime, crystals are formed which will be very small, and so distributed through the lime as merely to give it a fine salmon-colour.

This lime-salt is of a bright ruby colour, and by the following process it may be obtained in beautiful crystals. Freshly slaked lime is to be mixed with so much water that, when stirred, it will aggregate into small lumps about the size of peas. These lumps are dried upon a sand-bath till they are hard, and will bear handling without producing powder. The lumps (quite free from dust) are placed in tubes *not more than an inch in diameter*. When the lime is cold, bisulphide of carbon is poured upon it in sufficient quantity to saturate the lime, and leave a few drops in each tube. The tubes are corked, and allowed to stand for three or four days. Water is then poured in carefully, so as not to remove any powder from the lumps of lime, and the tubes are closed and allowed to stand as long as the crystals increase in size. To obtain the crystals, the mother-liquor is poured off, the mass of lime, &c. is dried at ordinary temperatures, and the crystals are picked out. For success, it is absolutely necessary that the lime should be in lumps, the vessel in which the reaction takes

place of small diameter, and that undecomposed bisulphide of carbon be present.

Crystals so prepared appear to be perfectly permanent; they do not undergo alteration from several weeks' exposure to atmospheric changes. I have not yet obtained them free from adhering lime, as all acids which will dissolve the lime decompose the crystals. They are soluble in water, but cannot be recrystallized from their solution. All the solutions I have yet tried decomposed without crystallizing. Other processes have equally failed to furnish a salt which can be obtained dry and pure; and I am therefore unable at present to furnish reliable concurrent analyses which will establish its formula.

I have not ventured an explanation of the reaction, because I am not yet acquainted with the compounds produced and their relative proportion. To ascertain whether an explanation based upon a reaction between the elements of water and bisulphide of carbon was tenable, I caused caustic lime to be heated to whiteness in a platinum crucible for two hours, and cooled out of contact with air. When cold, this anhydrous lime was saturated with dry bisulphide of carbon; and in a short time the lime became of a greenish-yellow colour, showing the progress of a reaction between the dry lime and the bisulphide of carbon.

Some of the reactions of this salt are remarkable; and more than one will exhibit the liability to error from the use of any but the most perfectly crystalline, dry, and clean specimens. Baryta-water added in excess throws down from an aqueous solution of the *pure* salt an amorphous, red, insoluble precipitate quite as brilliant in colour as vermilion. If this be washed directly after precipitation, the colour is retained for a considerable period; but if left in the mother-liquor, it soon darkens. The washed salt dries a brickdust red.

If, instead of the perfectly pure lime-salt, a solution of the salmon-coloured compound, which is formed when the salt is prepared in large vessels and with powdery lime, be taken, a brownish-yellow crystalline double salt of baryta and lime is formed, which is very soluble in water.

Nitrate of baryta gives lemon-yellow crystals in a red-brown mother-liquor with solution of the salmon-coloured mass, but an amorphous dirty grey precipitate in a yellowish mother-liquor with solution of the pure salt.

As soon as a quantity of the latter can be prepared, I hope to isolate the acid.

Salts of the oxides of barium, magnesium, strontium, silver, zinc, manganese, and chromium have been prepared by the direct action of bisulphide of carbon. Some of these differ considerably from the salts prepared by Berzelius by the action of the bisulphide of carbon upon the sulphides of the metals. The process which furnishes the lime-salt well crystallized will be tried with other compounds, and the results submitted to the Society.

A very offensive suffocating gas is evolved during the decomposition of bisulphide of carbon by lime, which is injurious, if not poisonous; and having suffered severely from breathing this and other noxious compounds derived from the same source, I think it right to call attention to it. I have formed a gas of similar properties by passing bisulphide of carbon and hydrogen together through heated lime, and should not be surprised if it prove to be the long-sought simple sulphide of carbon.

Slightly ammoniacal alcohol breathed from a cloth appears to be the best restorative for the severe depression caused by respiring the offensive gases and vapours above named.

XX. "On the Geometrical Isomorphism of Crystals." By the Rev. W. Mitchell. Communicated by Dr. Frankland. Received June 12, 1862.

In a paper "On the Geometrical Isomorphism of Crystals," published in the Philosophical Transactions for 1857, by H. J. Brooke, F.R.S., it was shown that all the substances crystallizing in the various forms of the pyramidal and rhombohedral systems might be regarded to be as isomorphous as those belonging to the cubical system.

This isomorphism was shown by so taking the arbitrary primitive pyramid of the one system, or the rhomboid of the other, as to bring these forms nearly isomorphous for every substance in the one system or the other. In this way tables were formed showing that the same notation for any form would be not strictly isomorphous, but plesiomorphous for any other form of another substance bearing the same notation.

It is the object of the present paper to show that not only the forms of the pyramidal and rhombohedral systems, but also those of the prismatic, are as strictly isomorphous as those of the cubical system. This is effected by demonstrating all the forms of these three systems to be but partial developments of the cubical system. Consequently every form can be indicated by the symbols of the cubical system. In other words, instead of having distinct axes and parameters for each system, and for every substance in that system, all are referred to the rectangular axes and equal parameters of the cubical system.

The pyramidal system is regarded as a tritohedral development of the cubical system, the faces so developed being all symmetrically taken with respect to one of the cubical axes. Thus, adopting the notation for the cubical system in the last edition of Phillips's 'Mineralogy,' the two faces of the cube $0\,0\,1$ and $0\,0\,\bar{1}$ will form the basal pinacoids, while the remaining faces $1\,0\,0$, $0\,1\,0$, $\bar{1}\,0\,0$, and $0\,\bar{1}\,0$ will give the direct square prism.

The faces of the rhombic dodecahedron $1\,1\,0$, $\bar{1}\,1\,0$, $\bar{1}\,\bar{1}\,0$, and $1\,\bar{1}\,0$ will give those of the inverse square prism.

There will be two groups of square pyramids derived from the four-faced cubes, the first in the development of the faces indicated by the symbols

$$k\,0\,h,\quad 0\,k\,h,\quad \bar{k}\,0\,h,\quad 0\,\bar{k}\,h$$
$$k\,0\,\bar{h},\quad 0\,k\,\bar{h},\quad \bar{k}\,0\,\bar{h},\quad 0\,\bar{k}\,\bar{h};$$

the second by

$$h\,0\,k,\quad 0\,h\,k,\quad \bar{h}\,0\,k,\quad 0\,\bar{h}\,k$$
$$h\,0\,\bar{k},\quad 0\,h\,\bar{k},\quad \bar{h}\,0\,\bar{k},\quad 0\,\bar{h}\,\bar{k};$$

the poles of both of these forms always lying in the zone $0\,0\,1$, $1\,0\,0$.

The remaining eight faces of the four-faced cube, when developed symmetrically with respect to the cubic axes, viz.

$$h\,k\,0,\quad k\,h\,0,\quad \bar{k}\,h\,0,\quad \bar{h}\,k\,0,\quad \bar{h}\,\bar{k}\,0,\quad \bar{k}\,\bar{h}\,0,\quad k\,\bar{h}\,0,\ \text{and}\ h\,k\,0,$$

give the octagonal prism.

The inverse square pyramids are derived from the tritohedral development of the faces of the twenty-four-faced trapezohedron and

three-faced octahedron,—the one group being derived from the faces

$$k\,k\,h,\quad \bar{k}\,k\,h,\quad \bar{k}\,\bar{k}\,h,\quad k\,\bar{k}\,h$$
$$k\,k\,\bar{h},\quad \bar{k}\,k\,\bar{h},\quad \bar{k}\,\bar{k}\,\bar{h},\quad k\,\bar{k}\,\bar{h},$$

the other from

$$h\,h\,k,\quad \bar{h}\,h\,k,\quad \bar{h}\,\bar{h}\,k,\quad h\,\bar{h}\,k$$
$$h\,h\,\bar{k},\quad \bar{h}\,h\,\bar{k},\quad \bar{h}\,\bar{h}\,\bar{k},\quad h\,\bar{h}\,\bar{k}.$$

The tritohedral development of the form $h\,k\,l$ gives the ditetragonal pyramid; of these there are three groups.

1st. $k\,l\,h,\quad l\,k\,h,\quad \bar{l}\,k\,h,\quad \bar{k}\,l\,h,\quad \bar{k}\,\bar{l}\,h,\quad \bar{l}\,\bar{k}\,h,\quad l\,\bar{k}\,h,\quad k\,\bar{l}\,h$
$k\,l\,\bar{h},\quad l\,k\,\bar{h},\quad \bar{l}\,k\,\bar{h},\quad \bar{k}\,l\,\bar{h},\quad \bar{k}\,\bar{l}\,\bar{h},\quad \bar{l}\,\bar{k}\,\bar{h},\quad l\,\bar{k}\,\bar{h},\quad k\,\bar{l}\,\bar{h}.$

2nd. $h\,l\,k,\quad l\,h\,k,\quad \bar{l}\,h\,k,\quad \bar{h}\,l\,k,\quad \bar{h}\,\bar{l}\,k,\quad \bar{l}\,\bar{h}\,k,\quad l\,\bar{h}\,k,\quad h\,\bar{l}\,k$
$h\,l\,\bar{k},\quad l\,h\,\bar{k},\quad \bar{l}\,h\,\bar{k},\quad \bar{h}\,l\,\bar{k},\quad \bar{h}\,\bar{l}\,\bar{k},\quad \bar{l}\,\bar{h}\,\bar{k},\quad l\,\bar{h}\,\bar{k},\quad h\,\bar{l}\,\bar{k}.$

3rd. $h\,k\,l,\quad k\,h\,l,\quad \bar{k}\,h\,l,\quad \bar{h}\,k\,l,\quad \bar{h}\,\bar{k}\,l,\quad \bar{k}\,\bar{h}\,l,\quad k\,\bar{h}\,l,\quad h\,\bar{k}\,l$
$h\,k\,\bar{l},\quad k\,h\,\bar{l},\quad \bar{k}\,h\,\bar{l},\quad \bar{h}\,k\,\bar{l},\quad \bar{h}\,\bar{k}\,\bar{l},\quad \bar{k}\,\bar{h}\,\bar{l},\quad k\,\bar{h}\,\bar{l},\quad h\,\bar{k}\,\bar{l}.$

Ditetragonal pyramids may also be developed from the faces of the forms $h\,h\,k$ and $k\,k\,h$, whose poles lie in the zones $0\,1\,1,\ 1\,0\,0$, and $1\,0\,1,\ 0\,1\,0$.

The forms of the hexagonal system are regarded as tetartohedral developments of the cubical system, the groups of faces being always taken symmetrically with respect to the diagonal of the cube or one of the octahedral axes.

The two faces of the octahedron $1\,1\,1$ and $\bar{1}\,\bar{1}\,\bar{1}$ thus form the basal pinacoids.

The six faces of the rhombic dodecahedron

$$1\,0\,\bar{1},\quad 0\,1\,\bar{1},\quad \bar{1}\,1\,0,\quad \bar{1}\,0\,1,\quad 0\,\bar{1}\,1,\quad \text{and}\quad 1\,\bar{1}\,0$$

give one hexagonal prism, while the other is derived from six faces of the twenty-four-faced trapezohedron, whose symbols are

$$2\,\bar{1}\,\bar{1},\quad 1\,1\,\bar{2},\quad \bar{1}\,2\,\bar{1},\quad \bar{2}\,1\,1,\quad \bar{1}\,\bar{1}\,2,\quad 1\,\bar{2}\,1.$$

The dihexagonal prisms are derived from those faces of the form $h\,k\,l$ which lie in the zone $1\,0\,\bar{1},\ 2\,\bar{1}\,\bar{1}$.

There are three groups of positive rhomboids, two derived from the three-faced octahedron, and one from the twenty-four-faced trapezohedron.

The following are the symbols of these groups :—

$$\begin{array}{lll} h\,h\,k, & k\,h\,h, & h\,k\,h \\ \bar{h}\,\bar{h}\,k, & \bar{k}\,\bar{h}\,\bar{h}, & \bar{h}\,\bar{k}\,\bar{h} \\ & & \\ h\,h\,\bar{k}, & \bar{k}\,h\,h, & h\,\bar{k}\,h \\ \bar{h}\,\bar{h}\,k, & k\,\bar{h}\,\bar{h}, & \bar{h}\,k\,\bar{h} \\ & & \\ k\,k\,\bar{h}, & \bar{h}\,k\,k, & k\,\bar{h}\,k \\ \bar{k}\,\bar{k}\,h, & h\,\bar{k}\,\bar{k}, & \bar{k}\,h\,\bar{k}. \end{array}$$

The last group is restricted to those forms of $k\,k\,h$ whose poles lie between those of $1\,1\,\bar{1}$ and $1\,1\,\bar{2}$, &c.

The direct rhomboids are all derived from the form $h\,k\,k$, and consist of two groups,

$$\begin{array}{lll} h\,k\,k, & k\,h\,k, & k\,k\,h \\ \bar{h}\,\bar{k}\,\bar{k}, & \bar{k}\,\bar{h}\,\bar{k}, & \bar{k}\,\bar{k}\,\bar{h}, \end{array}$$

and

$$\begin{array}{lll} h\,\bar{k}\,\bar{k}, & \bar{k}\,h\,\bar{k}, & \bar{k}\,\bar{k}\,h \\ \bar{h}\,k\,k, & k\,\bar{h}\,k, & k\,k\,\bar{h}. \end{array}$$

The form $h\,k\,l$ furnishes four groups of hexagonal scalenohedrons.

$$\begin{array}{lllllll} \text{1st.} & h\,k\,l, & h\,l\,k, & k\,l\,h, & l\,k\,h, & l\,h\,k, & k\,h\,l \\ & \bar{h}\,\bar{k}\,\bar{l}, & \bar{h}\,\bar{l}\,\bar{k}, & \bar{k}\,\bar{l}\,\bar{h}, & \bar{l}\,\bar{k}\,\bar{h}, & \bar{l}\,\bar{h}\,\bar{k}, & \bar{k}\,\bar{h}\,\bar{l}. \\ & & & & & & \\ \text{2nd.} & h\,k\,\bar{l}, & h\,\bar{l}\,k, & k\,\bar{l}\,h, & \bar{l}\,k\,h, & \bar{l}\,h\,k, & k\,h\,\bar{l} \\ & \bar{h}\,\bar{k}\,l, & \bar{h}\,l\,\bar{k}, & \bar{k}\,l\,\bar{h}, & l\,\bar{k}\,\bar{h}, & l\,\bar{h}\,\bar{k}, & \bar{k}\,\bar{h}\,l. \\ & & & & & & \\ \text{3rd.} & h\,l\,\bar{k}, & h\,\bar{k}\,l, & l\,\bar{k}\,h, & \bar{k}\,l\,h, & \bar{k}\,h\,l, & l\,h\,\bar{k} \\ & \bar{h}\,\bar{l}\,k, & \bar{h}\,k\,\bar{l}, & \bar{l}\,k\,\bar{h}, & k\,\bar{l}\,\bar{h}, & k\,\bar{h}\,\bar{l}, & \bar{l}\,\bar{h}\,k. \\ & & & & & & \\ \text{4th.} & h\,\bar{l}\,\bar{k}, & h\,\bar{k}\,\bar{l}, & \bar{l}\,\bar{k}\,h, & \bar{k}\,\bar{l}\,h, & \bar{k}\,h\,\bar{l}, & \bar{l}\,h\,\bar{k} \\ & \bar{h}\,l\,k, & \bar{h}\,k\,l, & l\,k\,\bar{h}, & k\,l\,\bar{h}, & k\,\bar{h}\,l, & l\,\bar{h}\,k. \end{array}$$

The forms of the prismatic system are then shown to be formed by an analogous development of faces about the rhombic axes; making two of the faces of the rhombic dodecahedron the basal pinacoids.

In examining the Tables accompanying the paper, in which all the known faces of crystals are expressed in the notation of the cubical system, it will be seen that the indices are necessarily somewhat large.

This inconvenience is more than compensated for by the fact that the angular elements of every face, and its relation to another face, can at once be calculated from the symbols of the faces by very easy formulæ. The magnitude of the indices are also shown to be much diminished by using approximations bringing every pole to its place on the sphere of projection within 5 or 6 minutes—an approximation not greater than that constantly used to make observations tally with the calculated symbols.

XXI. "On the Forces concerned in producing the larger Magnetic Disturbances." By Balfour Stewart, Esq., M.A., F.R.S. Received June 14, 1862.

(Abstract).

The author begins by alluding to a previous communication made to the Royal Society, containing an account of the great magnetic storm of August 28–September 7, 1859, in which he had shown that the first effect of this great disturbance was to diminish in intensity both components of the earth's magnetic force at Kew, during a period of about six hours. Such an effect, he argues, can scarcely be supposed due to any combination of earth-currents, of which the period is only a few minutes.

But another appearance is noticeable on the photographic curves which regard the progress of this great disturbance.

While the great wave of force had a period of about six hours, there were superimposed upon it smaller disturbances having a period of a few minutes, and therefore comparable in this respect with earth-currents.

These smaller disturbances are of very frequent occurrence, and show themselves in the Kew magnetograph curves as serrated appearances, occasionally magnified into peaks and hollows.

Two hypotheses may be entertained regarding them.

1st. They may be conceived to represent small and rapid changes in the intensity of the whole disturbing force which acts upon the magnet; and since (as stated above) this force cannot be supposed due to earth-currents, so neither can its variations be caused by these.

2nd. The peaks and hollows may be supposed due to the direct action of earth-currents upon the magnets.

The following argument is advanced to show that the second of these hypotheses is untenable.

Let us compare together the two magnetic disturbances of August and September 1859 and August 1860; and suppose the peaks and hollows of the disturbance curves of these dates to be caused by earth-currents. This would require that currents of the same name should have simultaneously travelled between Margate and Ramsgate, and between Ramsgate and Ashford during the latter disturbance, whereas during the former these currents should have been of different names, that is to say, the one positive and the other negative. According to Mr. Walker's observations however, on both these occasions a current between Margate and Ramsgate was simultaneous with one of the same name between Ramsgate and Ashford.

Thus, if we adopt the second hypothesis, it would appear that these lines ought to have been affected differently on these two occasions, whereas by observation they were affected in the same manner; the conclusion is that this hypothesis does not represent the truth.

The author then shows that earth-currents observed simultaneously with a very abrupt disturbance which commenced about $11^h\ 17^m$ A.M., September 1, 1859, would lead us to infer that the former are induced currents due to sudden and rapid changes in the magnetism of the earth.

Referring now to the first hypothesis, which asserts that the peaks and hollows represent small and rapid changes in the intensity of the whole disturbing force which acts upon the magnet, it would follow that these peaks and hollows should in this case comport themselves with regard to the three elements of the earth's magnetism in the same way as the whole disturbing force of which they represent the changes. Thus, if the tendency of the great body of the disturbing force is to raise the curves for the three elements simultaneously, then a small peak in one element should correspond to a peak, and not to a hollow, in the other two. But if, on the other hand, the tendency of the disturbing force is to raise one of the curves and lower the other two, then a peak in the first should correspond to a hollow in the others.

This is shown to be the case in the disturbances extending from the beginning of 1858 to the end of 1860; and the author therefore

concludes that peaks and hollows represent small and rapid changes in the intensity of the whole disturbing force which acts upon the magnet.

It is then shown that use may be made of these peaks and hollows, if we wish to analyse the forces concerned in producing disturbances. Let us suppose that several independent forces are concerned. It is very unlikely that a small and rapid change takes place at the same instant in more than one of these. The measurement therefore of simultaneous abrupt changes for the three elements may enable us to determine the character of one of the elementary disturbing forces at work.

It is not even necessary to confine ourselves to very rapid changes, provided we take peaks or hollows which present a similar appearance for all the elements, as such can only be produced by the action of a single force.

The author then shows that a peak of the horizontal force always corresponds to a peak of the vertical force, and not to a hollow, and that, when similar peaks are compared together, the horizontal-force peak is always as nearly as possible double in size that of the vertical force.

This curious fact would imply that the resolved portion of the disturbing force which acts in the plane of the magnetic meridian is always in nearly the same direction. The dip of this resolved portion will be about $17\frac{1}{2}^{\circ}$.

It is also found that a declination peak corresponds to a peak of either force, except in the case of the great disturbance of August to September 1859, during the most violent portion of which a peak of the declination corresponded to a hollow in either force. The length, however, of a declination peak does not bear an invariable ratio to that of a force peak—this ratio varying much from one disturbance to another, but not much from one part to another of the same disturbance. In this last case, however, the variation of the ratio, although not great, is yet greater than that of the ratio between the two force peaks; so that it is somewhat difficult to obtain similar peaks when comparing the declination curve with that of either force.

It thus appears that the force which acts upon the magnets does not vary much from one part to another of the same disturbance,

and it therefore becomes possible to give the elements of this force, which will thus characterize the disturbance.

The author then attempts, by means of comparing similar appearances, to represent the force at work for each disturbance between the beginning of 1858 to the end of 1860. The great disturbance of August to September 1859 is here remarkable as one in which two independent disturbing forces seem to have acted at once,—one of these being of the normal type, in which all the elements were raised or depressed together, while in the other the declination was raised when both elements of the force were depressed.

It will be observed that this method of analysis does not completely determine the disturbing force, but merely fixes the line of its resultant action, along which the force itself may be either positive or negative; or, again, there may be two nearly opposite forces acting against one another, the visible disturbance denoting merely the difference in strength between the two; and there is some reason to think that this last supposition represents the true state of the case.

For while the definite relation which exists between the peaks of the two force-components shows that all disturbing forces affect these in nearly the same way, yet sometimes, though very rarely, in the general progress of the curve one of the elements will be above the normal while the other is below it. Now, this may be accounted for in the following manner. Suppose we have a disturbance producing an elevation in the horizontal force represented by $+40$, and one in the vertical force represented by $+20$. This will be of the normal type. Suppose now that at the same time we have another force nearly similar, whose action on the two force-elements is represented by -39 -21. This is also sufficiently near the normal type. The result of these two disturbances superimposed will be $+1$ and -1, showing that the one element is raised above its normal position, while the other is depressed below it. This idea of two opposite forces acting simultaneously in disturbances is that entertained by General Sabine from other considerations.

XXII. "Experimental Researches on the Transmission of Electric Signals through Submarine Cables."—Part I. Laws of Transmission through various lengths of one Cable. By Fleeming Jenkin, Esq. Communicated by Prof. Wheatstone. Received May 20, 1862.

(Abstract.)

Professor W. Thomson has in various papers stated and developed the mathematical theory of the transmission of signals through long submarine cables. The present paper contains an experimental research into the same subject. The conclusions arrived at by theory are confirmed by the experiments, and some new facts of considerable importance are established.

All the observations in this part of the paper were made on the Red Sea cable, when coiled in iron tanks at Birkenhead.

By observation on a reflecting galvanometer, an arrival-curve was obtained for various lengths of cable with various arrangements of battery. By arrival-curve is meant the curve representing the gradual rise of the current at the remote end of the cable when the near end is put in permanent connexion with the battery.

The analysis of the various arrival-curves led to the following conclusions :—

1. "The electromotive force has no appreciable effect on the velocity with which the current is transmitted.

2. "The rate of decrease in the current at the remote end, after contact has been made for a given time with earth at the near end, is the same as the rate of increase observed after making contact with the battery at the near end for an equal time."

With reference to the use of alternate positive and negative currents as compared with alternate connexion with the positive or negative pole of a battery and earth,

3. It was found that the "reversals in no way modified the arrival-curve during its increase, nor did they modify the curve showing the decrease of the current."

The effect of ordinary morse signals was next observed on the galvanometer through various lengths of cable.

The changes in the received current, caused by repeated dots, by repeated dashes, by dots and dashes alternately, and by dots and dashes separated by a pause, were observed at different speeds.

Repeated dots, when represented graphically, give an even wavy line with large amplitudes of oscillation for slow speeds or through short lengths, but rapidly approaching a straight line as the speed of transmission or the length of the cable was augmented.

If the maximum permanent deflection caused by the battery be called 100, dots sent at the rate of 15 per minute through 2192 knots of cable caused oscillations in the received current of 12·7 per cent.; and sent at the rate of 50 per minute, this caused an oscillation of less than 1 per cent.

4. From this it was concluded that "on all submarine cables there is a limit to the number of signals which can be sent per minute, a limit which cannot be exceeded by any ingenious contrivance."

If we continue to call the maximum deflection due to permanent contact 100, the mean height of the current observed during dots is below 50, on account of time lost between the two contacts while moving the sending-key.

When dashes or lines are sent, *i. e.* long contacts with the battery followed by short earth-contacts, an even wavy line is obtained, the mean height of which is above 50; and when dots and dashes are combined, the curves representing the changes of the current become very irregular, sometimes flying above 50, sometimes falling below this line; and when long pauses, or a succession of long battery-contacts are introduced, the curves become hopelessly confused, especially at the higher speeds, so that the signals cannot be disentangled, even when the change of current can be continually followed. From this it is concluded that,

5. "There is a wide margin between the limit set to the speed of transmission by the gradual diminution of the received signals, and that set by their interference."

Reverse currents have been recommended as a means of accelerating the rate of speaking through submarine cables. Their effect was tested; the arrival-curves and signal-curves obtained by their use differed in no way from those obtained by simple currents and earth-contacts. Hence it was concluded that,

6. "The use of reverse currents does not alter the limit set by the gradual diminution of the received signals, nor that set by their interference."

It occurred to the author that, if by any means the current could invariably after each signal be brought to one constant strength and

maintained at that strength between the signals, the confusion of interference would be avoided. He considered that, if the second or earth-contact of each signal bore a fit proportion to the first contact, this object might be effected; and he considered that a succession of very short pairs of contacts of a certain relative length, would maintain the current at the constant final strength during any pause separating signals. He therefore prepared a paper band with openings cut so as to make pairs of equal battery- and earth-contacts for dots, long battery-contacts, followed by nearly equal earth-contacts, for a dash, and a succession of pairs of very short contacts wherever a pause was required, the battery contacts being rather the shorter of the two.

The success of this plan was such that the signals were distinctly recorded, not only by the galvanometer, but by a relay when the total variations caused by the shortest signals were invisible on the galvanometer, *i. e.* even less than 1 per cent. of the maximum final current.

7. Hence it was concluded that by the means adopted, or by analogous means, "signals can be sent without confusion at any speed which will allow the shortest signal used to cause a sensible variation in the received current."

These experiments were tried on dry cable coiled in iron tanks, and might therefore not be applicable to extended and submerged cables.

The author has, however, proved that the retardation and insulation of an iron-covered cable are very little affected by the mere presence or absence of water; and wherever the conclusions obtained from the experiments agree with the deductions of theory, it is clear that the experiments and theory confirm one another, and the conclusions may be safely applied to the practical case of a submerged and extended cable; for it is impossible to suppose that results due only to an accidental arrangement of the cable should by chance coincide with the deductions from a defective hypothesis.

The experimental arrival-curves do not exactly agree with the curve given by Professor W. Thomson (Proceedings of the Royal Society, 1855. Phil. Mag. 1856).

The experimental curve approaches its maximum much more slowly than the mathematical curve, and continues to rise 1 or 2 per cent. long after all effects from retardation as given by theory would cease.

Some of this effect may be due to the mutual influence of the coils of the cable*; but the greater part of the discrepancy is due to the

* *Vide* paper read by Professor W. Thomson at the British Association, Aber-

change of the insulation due to continued electrification, first published by the author in a paper read before the Royal Society in 1859–60*.

The identity of the arrival-curve during increase and decrease shows that,

8. "The apparent increase of resistance of the gutta percha is rather due to an absorption of electricity which is again given out, than to a real change in the conductivity of the material."

The theoretical and practical conclusions on the effect of repeated signals were next examined. Little change of insulation could take place during the repeated signals, because the greater part of the cable remained continually electrified; and greater coincidence between the experiments and the theory was therefore to be expected.

The curve expressing the rate at which the amplitude of oscillation in the received current diminishes as the number of signals increases, was constructed from Professor W. Thomson's equations†; and the experimental amplitudes with 1500, 1802, and 2192 knots of cable in circuit, were found to coincide in the most accurate manner with this curve—establishing completely the soundness of the mathematical theory.

9. These results prove beyond all question that "the rate of transmission varies as the square of the length, whether by rate of transmission be meant that speed at which repeated signals fail to produce any sensible effect, or the rate producing so great an amplitude that common hand-signals can be received without confusion."

It is also found (when small compared with the total resistance) that,

10. "The resistance of the battery and receiving instrument produces nearly the same effect as the addition of an equal length of submarine cable."

If the amplitude of oscillation in the received current caused by dots at any one speed through any one straight cable were known, the amplitude through any other cable at any other speed could immediately be taken from the curve, now verified by experiment.

Unfortunately this one fact is wanting. The author hopes to be able to supply the want in the second part of this paper.

deen, 1859. Also paper by Professor W. Thomson and F. Jenkin, Phil. Mag. 1861; also a letter from Mr. F. C. Webb in 'The Engineer,' August 1859.

* Published in full in Appendix to the Report of the Committee of the Board of Trade on the Construction of Submarine Cables.

† *Vide* Proceedings of the Royal Society, 1855. Phil. Mag. 1856.

XXIII. "On the Thermal Effects of Fluids in Motion."—Part IV. By J. P. Joule, LL.D., F.R.S., and Professor W. Thomson, F.R.S. Received June 19, 1862.

(Abstract).

A brief notice of some of the experiments contained in this paper has already appeared in the 'Proceedings.' Their object was to ascertain with accuracy the lowering of temperature, in atmospheric air and other gases, which takes place on passing them through a porous plug from a state of high to one of low pressure. Various pressures were employed, with the result (indicated by the authors in their Part II.) that the thermal effect is approximately proportional to the difference of pressure on the two sides of the plug. The experiments were also tried at various temperatures, ranging from 5° to 98° Cent., and have shown that the thermal effect, if one of cooling, is approximately proportional to the inverse square of the absolute temperature. Thus, for example, the refrigeration at the freezing temperature is about twice that at 100° Cent. In the case of hydrogen, the reverse phenomenon of a rise of temperature on passing through the plug was observed, the rise being doubled in quantity when the temperature of the gas was raised to 100°. This result is conformable with the experiments of Regnault, who found that hydrogen, unlike other gases, has its elasticity increased more rapidly than in the inverse ratio of the volume. The authors have also made numerous experiments with mixtures of gases, the remarkable result being that the thermal effect (cooling) of the compound gas is less than it would be if the gases after mixture retained in integrity the physical characters they possessed while in a pure state.

XXIV. "On the Spectra of Electric Light, as modified by the Nature of the Electrodes and the Media of Discharge." By the Rev. T. R. Robinson, D.D., F.R.S. Received June 19, 1862.

(Abstract.)

The author, after referring briefly to the researches of previous inquirers, and the hypothesis now generally adopted, that the bright lines observed in these spectra depend so absolutely on the chemical nature of the substances present that their occurrence is an unerring test of that presence, expresses his belief that it cannot be admitted

in its full extent without much more decisive proof than has yet been afforded. It assumes,

1. That each substance has a set of lines peculiar to itself.

2. That those lines are not produced or modified by any molecular agent except heat.

3. That the spectrum of one substance is in nowise modified by the presence of another; and in such cases both spectra coexist independently, and are merely superposed.

4. As may be inferred from 2, that electricity does not make matter luminous directly, but only by heating it; so that the electric spectrum differs in nothing from that produced by heat of sufficient intensity.

His attention was directed to this subject several years ago by the difference of colour of discharges in carbonic oxide at common and diminished pressure; and the results of his experiments appear to show that none of these four points is universally true.

His apparatus consisted of a powerful induction machine, with which a Leyden jar was connected; of prisms, first one of 45°, afterwards one of 60° (whose deviations were reduced to the scale of the first); and of an optical theodolite, in which a collimator with a variable slit gives the beam whose spectrum is observed. He points out an important defect of this arrangement, and discusses the probable liabilities to error proceeding from the graduation reading only to minutes, and from other sources of uncertainty.

The media of discharge were air, nitrogen, oxygen, hydrogen, and carbonic oxide, to which were added in some instances the vapours of mercury, phosphorus, and bisulphuret of carbon. For electrodes, 23 metals and graphite were used—15 with each of the five gases at common pressure and at one of $0^{\circ}\cdot 2$, the others only with some of them. In all, 185 *different* spectra were measured, of which 93 were at common pressure.

At common pressure the spectra show a number of bright lines on a coloured ground, the light of which is in general stronger towards the red than the violet end, and strongest in the green. In some this ground is so bright as to efface all but the most luminous lines: this is especially the case with hydrogen. Of the lines, some are very brilliant; but they range in light down to the very lowest degree of faintness, such that (at least with the author's apparatus) they can only be seen when the room is entirely dark, and are bisected with

great difficulty. They vary also in width, from a mere hair's breadth to six or seven times the apparent width of the slit.

On exhausting the tube in which the discharge is made, at first the only change is that the brilliant lines lose a little of their lustre, till at pressures varying from 3° to $0^\circ{\cdot}5$ the spectrum rather suddenly fades away, sometimes leaving only a suspicion of one or two lines; with others the least-refrangible rays vanish, while the violet remain, though very faint, especially with aluminium. In hydrogen spectra the three bright bands of this gas vanish at unequal densities; and it is remarkable that this occurs when the gas is diluted to the same proportions by mixing air with it.

Exhausting yet further, this transition spectrum becomes again bright; fresh lines appear, and it is changed into a new one, which, however, is never as splendid as that at common pressure, especially at the red end, and in which the very brilliant lines are less frequent. This want makes the difference between the two kinds of spectra seem greater than it really is. Fewer lines are visible in the rarefied media, and of these about four-tenths are not found in the spectra of common pressure.

If the tables in which the measures are given be examined in reference to the points alluded to as doubtful, it will be obvious,

1. That many lines are found in *all the gases*, and in *many, perhaps all* the metals: the existence of such lines must be independent of the chemical nature of electrodes or media; it is otherwise with their brightness, which may be intense with one substance and feeble with another. This unchemical origin is still more clearly shown by a modified experiment of Plücker, where the discharge is made by the induction of glass without the presence of any metal. When *the same* glass vessel was filled in succession with nitrogen, oxygen, and hydrogen, though not above twenty-three lines were seen in its capillary tube, and those very faint, yet more than half the number were common to two of the gases, or to the three. These might perhaps be referred to soda or lead detached from the glass; but some of them are not found in those spectra.

2. The difference between the common-pressure, transition, and rarefied spectrum shows that the character and even the existence of certain lines depend on the mere density of the medium, the chemical circumstances remaining unchanged.

3. That the spectra are not merely superposed without change is

evident from several facts. The spectra of air do not in every case show all the lines of oxygen and nitrogen, and occasionally have some not visible in either of them: the spectrum of graphite in oxygen is quite different from those of carbonic oxide. There is even reason to believe that for certain lines the actions of bodies may be antagonistic. The spectrum of mercury electrodes and mercury vapour showed 48 lines, and the author expected that the spectra for any gas with mercury electrodes would add to those of mercury the peculiar lines of that gas, which could thus be certainly determined. In the nitrogen spectrum, however, 20 of the mercurial lines had disappeared, in the hydrogen 18, and in the carbonic oxide 13.

4. The brilliancy or visibility of the lines is very little increased by greatly augmenting the heating power of the discharge. The two halves of the induction machine can be made to act either consecutively for tension, or collaterally for quantity. In the latter case the quantity is doubled, and therefore the heating power quadrupled. When the apparatus is so used, the violet bands are something brighter, but not so much so as to be noticed by an unpractised observer. The red and green show no appreciable difference; but the author is inclined to think the change may be greater in the ultra-violet part. He proposes, however, to repeat the experiment with coils of much greater power as to quantity. If electricity can produce thermic vibrations by its transmission, there seems no *à priori* reason why it cannot produce luminous ones; and no evidence that it cannot is known to him.

It seems to follow from these observations that the tendency to show such lines belongs to matter in general, but that different forms of it have different powers of manifesting that tendency, and that those powers may sometimes interfere. If this be confirmed by further research, the result will be that, though the *electric* spectrum may give useful indications to the analyst, it should never be his sole dependence, or be trusted without full cognizance of the conditions which may affect its indications.

XXV. "On Fermat's Theorem of the Polygonal Numbers."—Second Communication. By the Right Hon. Sir Frederick Pollock, F.R.S., Lord Chief Baron. Received June 19, 1862.

(Abstract.)

The object of this paper is to show the result of combining the three series (which have been the subject of previous communica-

tions) in a square, in such manner that the division into 4 squares of *certain* terms in each series, may produce a division into 4 squares of *every* term of other series, and thus each term in the whole square will at last be divided into 4 squares, and the first term will be so divided into 4 square numbers that two of the roots will be equal to each other; two of them will differ by 1, and the algebraic sum of all the roots will be equal to 1.

It is not offered (at present) as a *proof* that it *must* be so, but as a *method* by which that result may always (in fact) be obtained.

If any odd number $2n+1$ be increased by 2, 4, 6, 8, 10, &c., the $(2n+1)$th term will be $(2n+1)^2$; other terms will have a distinct arithmetic relation to n, and $n+1$, and the whole series will be such that, if the pth term can be divided into square numbers whose roots shall equal $2p-1$, then every term of the whole series can be so divided that the roots of the $(p+1)$th term will be $2p+1$, and so on through the whole series.

Let $\overset{1}{27}, \overset{3}{29}, \overset{5}{33}, \overset{7}{39}, \overset{9}{47}, \overset{11}{57}$, &c. be such a series, with the odd numbers as indices of the sums of the roots, $39={}^{2}\overline{-2\overset{3}{+}1\overset{2}{+}3\overset{2}{+}5}$, and the sum of the roots is 7, and the differences of the roots, placed in arithmetic order, will be 3 . 2 . 2; then 29 will have roots with the same differences, the sum being 3, ${}^{2}\overline{-3\overset{3}{,}\ 0\overset{2}{,}\ 2\overset{2}{,}\ 4}=29$, and 57 will have roots ${}^{2}\overline{-1\overset{3}{,}\ 2\overset{2}{,}\ 4\overset{2}{,}\ 6}=57$. The other numbers in the series will have the differences reversed, but the sums of the roots will be respectively as the odd numbers placed as the index of each.

If any odd number be increased by 4, 8, 12, 16, &c., so as to form a series, $2n+1$, $2n+5$, $2n+13$, $2n+25$, pth term $(2n+(p-)^2, +p^2)$, it will have in the first term 4 roots, 2 of which differ by 1; in the 2nd term, 4 roots, 2 of which differ by 3; in the 3rd term 4 roots, 2 of which differ by 5; in the nth term 4 roots, 2 of which differ by $2n-1$; the other two roots will be common to all the terms. If these odd numbers, 1, 3, 5, &c., be made indices of the 1st, 2nd, 3rd, &c. terms, and any one term can be found having 2 roots differing by the index of that term, then the roots of all the other terms may be found. Let 27 increase by 4, 8, 12, &c.,

$$\overset{1}{27}\ \ \overset{3}{31}\ \ \overset{5}{39}\ \ \overset{7}{51}\ \ \overset{9}{67}, \text{ \&c.};$$

but $39 = \overline{2, 3, 1, 5}^{2}$, and $-2, 3$ differ by 5. Then the terms of the whole series may be divided into 4 squares, 2 of which will be common to all the terms, and the other 2 will have the difference pointed out by the index. The roots are placed below each term, and the middle roots are common to all the terms :

1	3	5	7	9	11
27	31	39	51	67	87, &c.
0, 1, 5, 1	−1, 1, 5, 2	−2, 1, 5, 3	−3, 1, 5, 4	−4, 1, 5, 5	−5, 1, 5, 6

If an odd number $(2n+1)$ be increased by 2, 6, 10, 14, &c., and $2n+1, 2n+3, 2n+9, 2n+19, 2n+(p-1)^2, (p-1)^2, 1$ (pth term) be the resulting series, then, if the even numbers (beginning with 0) be made indices, and any term in the series can be divided into 4 squares, 2 of them having their roots with the algebraic difference pointed out by the index, then the other 2 roots will be common to all the terms, and in a similar manner all the terms will have roots corresponding with the index of each term.

The series $2n+1$, $2n+5$, $2n+13$, $2n+25$, &c. will have for its nth term $n^2, +n^2+1$, for its nth term is obviously $2n+(n-1)^2 +n^2=n^2+n^2+1$; the $(n-1)$th term will be $(n-1)^2, +(n-1)^2+ 3$; and going backwards to the first term, the roots $(n-1)$, $(n-1)$ decrease by 1, and the arithmetic number increases by 2; but this obtains beyond the first term into a continuation of the series backwards ; thus,

23	15	11	11	15	23	35	51
2 15 2,	1 13 1	0 11 0	1 9 1	2 7 2	3 5 3	4 3 4	5 1 5.

Instead of this mode of continuing the roots and arithmetic numbers, they may be applied thus :—

1	3	5	7	9	11	13	17
0 11 0	1 13 1	2 15 2	3 17 3	4 19 4	5 21 5	6 23 6	7 25 7
11	15	23	35	51	71	95	123
1 9 1	2 7 2	3 5 3	4 3 4	5 1 5			

And whenever the arithmetic number is of the form $2a^2+2a+2b^2+1$, (that is) is the sum of 2 triangular numbers multiplied by 2 and increased by 1, then, by altering the even squares, the term may be made to consist of 4 squares, as to which the roots of 2 of them will differ by 1.

As an example, $51=4^2+4^2+19$

$$=3^2+5^2+(19-2)\ 17$$
$$=2^2+6^2+(19-8)\ 11$$
$$=1^2+7^2+(19-18)\ 1^2.\ 0^2.$$

The series $2n+1$, $2n+3$, $2n+9$, $2n+19$, &c., has similar proper-

	1	3	5	7	9	11
0	1 11	3 15	5 23	7 35 −1, 0, 3, 5	9 51 0, 1, 5, 5	11 71
2	1, 3 13	1, 5 17	3, 7 25 −2, −1, 2, 4	5, 9 37 1, 2, 4, 4	7, 11 53 0, 1, 4, 6	9, 13 73
4	3, 5 19 0, 1, 3, 3	1, 7 23 −3, −2, 1, 3	1, 9 31 2, 3, 3, 3	3, 11 43	5, 13 59 0, 1, 3, 7	7, 15 79
6	5, 7 29 −4, −3, 0, 2	3, 9 33 3, 4, 2, 2	1, 11 41	1, 13 53	3, 15 69 0, 1, 2, 8	5, 17 89
8	7, 9 43 4, 5, 1, 1	5, 11 47	3, 13 55	1, 15 67	1, 17 83 0, 1, 1, 9	3, 19 103
10	9, 11 61 5, 6, 0, 0	7, 13 65	5, 15 73	3, 17 85	1, 19 101 0, 1, 0, 10	1, 21 121

ties, not necessary to be stated in detail in order to understand the effect of placing the series in a square.

If an odd number, as 11 (see diagram, No. 1), be placed in the upper line on the left-hand square of a larger square divided into small squares, and be made the first term of a series, increasing by 4, 8, 12, &c., and then each term of that series be made the first term of a series increasing by 2, 6, 10, 14, &c., the square will be completed, and every odd number in the small squares will be a term in both series; but if a diagonal be drawn from 11 to 121, and parallel from

13 to 101, and beginning with 13, a number be taken from each line alternately, a series of the first kind will be discovered, viz. one increasing by 2, 4, 6, 8, 10, &c., and every term in that series will become the first term of another similar series; so that every odd number in the small squares (except those in the top row) will be terms in 2 such series, and the indices will be as marked in the right-hand upper corner of each square; the numbers in the margin 1, 3, 5, 7, &c., and 0, 2, 4, 5, &c., are the indices of all the series, parallel to them.

Now $51 = \overline{0, 1, 5, 5}^{\,2}$. No two roots differ by 9; but $5=5$; and $5+5-1=9$. As $5=5$, all the numbers below 51, that is 53, 59, &c., are divided into 4 square numbers, whose roots appear in the diagram. Again, $35=4^2$, $4^2+3=3^2$, 5^2+1, and $35=\overline{-1, 0, 3, 5}^{\,2}$, which gives 7, the index of 35, as a term in the series increasing by 2, 4, 6, &c.; and therefore every term may be resolved into 4 squares; but as one series crosses a set of series, it at length furnishes the index, thus, $\underset{-0,\,-1,\,5,\,5}{51}$, $\underset{-1,\,-2,\,4,\,4}{37}$, $\underset{-2,\,-3,\,3,\,3}{31}$, on arriving at 31. $2+3=5$, the index of 31 as a term in the series increasing by 4, 8, 12, &c.; and 19 therefore equals 0, 1, 3, 3; and -1, $3=4$ (the index of 19 as a term in the 3rd series); $11=0, 3, 1, 1$.

The method by which the division of certain numbers into 4 squares is here accomplished applies to all numbers of the form $2n+1$; but as the first term increases, the methods also multiply, so as to afford increasing means of division, which must be the subject of a future communication.

XXVI. "On the Oxidation and Disoxidation effected by the Alkaline Peroxides." By B. C. Brodie, Esq., F.R.S., Professor of Chemistry in the University of Oxford. Received June 19, 1862.

(Abstract.)

A preliminary notice containing an abstract of the greater portion of this paper has already appeared*.

Having shown that the alkaline peroxides are capable of acting either as agents of oxidation or reduction, the author proceeds to

* See 'Proceedings,' vol. xi. p. 442.

connect the double function of this class of peroxides with the peculiar catalytic decompositions which they undergo. It is shown that the catalytic decomposition may be regarded as a combination of these two actions, an oxidation and a reduction simultaneously occurring. Thus in an alkaline solution of the peroxide of hydrogen, protoxide of manganese is oxidized to peroxide. In the acid solution the peroxide of manganese is reduced to protoxide, the results being expressed in the following equations:—

$$Mn_2O + H_2O_2 = H_2O + Mn_2O_2,$$
$$Mn_2O_2 + H_2O_2 = Mn_2O + H_2O + O_2;$$

while the result of the catalytic decomposition effected by the peroxide of manganese is given in the equation derived from the above by elimination,

$$2H_2O_2 = 2H_2O + O_2,$$

the result being the same as though the peroxide of manganese were alternately reduced and oxidized by the alkaline peroxide. We are thus enabled to analyse the catalytic action into its constituent decompositions.

That in numerous cases the catalytic change is brought about by the intervention of intermediate compounds, which are alternately formed and destroyed during the action, is shown in various examples. For instance, the addition of a solution of peroxide of sodium to an excess of a solution of a protosalt of copper causes the formation of a precipitate of a yellow peroxide of copper. If, on the other hand, a few drops of the salt of copper be added to an excess of the alkaline peroxide, the same yellow body is formed, but the whole of the peroxide is ultimately decomposed; after the decomposition hydrated protoxide of copper remains. Similar phenomena occur with an ammoniacal solution of the copper-salt. If a few drops of this solution be added to an ammoniacal solution of the peroxide of hydrogen, the solution becomes of a yellow colour, and the catalytic action is set up. This action may continue, in dilute solutions, for several hours; during the whole of this time the yellow colour is permanent; but ultimately, when the peroxide is entirely decomposed, the blue colour of the ammoniacal solution of the protoxide reappears. The ammoniacal solution of the protoxide of copper decomposes the peroxide of hydrogen into water and oxygen, precisely as sulphuric acid decomposes alcohol into ether and water. But in this case the colour of the solution gives actual evidence of the

presence of the intermediate compound by the agency of which the catalytic action is effected, and which is formed, but disappears from the final result.

XXVII. "On the Relative Speed of the Electric Wave through Submarine Cables of different lengths, and a Unit of Speed for comparing Electric Cables by bisecting the Electric Wave." By CROMWELL F. VARLEY, Esq. Communicated by Professor STOKES, Sec. R.S. Received June 19, 1862.

(Abstract.)

The present paper gives the results of some experiments which were undertaken to determine, first, the relative speed of the electric wave through cables of various lengths; secondly, the retarding effect of the iron covering of the cable; and thirdly, methods for the increase of the speed of the electric wave.

When a long submarine cable or subterranean wire is connected at one end through a galvanometer to the earth, and the other end is connected with a battery, a current flows through it, deflecting the galvanometer-needle.

If the needle be made very light and small, so as to have but a small amount of inertia, and the cable be long, the current will be seen to arrive after the lapse of a short but appreciable interval of time, and will gradually augment in intensity approaching to, but never attaining, the maximum.

Professor Thomson has investigated this subject mathematically, and arrived at the conclusion that in submarine cables of different lengths the speed is inversely as the square of the distance.

Through the Atlantic Cable, the conducting wire of which weighed 93 lbs. to the statute mile, and the length of which was rather more than 2300 statute miles, the electric current did not show itself on Thomson's sensitive reflecting galvanometer until more than one second after contact had been made with the battery at the other end.

In experiments made by the author in 1854 upon 1600 miles of wire between London and Manchester, connected up in one continuous circuit, the current was not visible upon the chemical recording instruments then in use until after the lapse of about three seconds.

These experiments were repeated by Professor Faraday; and he has made known the results.

From the imponderable nature of electricity (considered for a moment as a fluid), from its incompressibility, and other circumstances, the author infers that the electric current commences flowing out at the one end at the very instant that contact is made with the battery at the other end; but it is a considerable time before it reaches an appreciable strength; it then goes on augmenting in strength, approaching to, but never absolutely attaining, its maximum force.

As the first part of the wave commences to appear instantly, and as the top of the wave would require an indefinitely long period of time to be reached, it will be evident that the part of the wave best suited for investigation is half the maximum, as at that period the changes of its intensity in a given time are more rapid than at any other.

When attempting to measure by means of a galvanometer the arrival of the wave at half its maximum, the weight and momentum of the magnet of the galvanometer were found to interfere so much that, excepting through very long lengths of cable, nothing approaching to an accurate determination of the speed could be obtained. The use of electro-magnets was equally, if not more, objectionable, as they require a very appreciable but uncertain time to be magnetized.

The following method, however, of *bisecting the electric wave* has obviated these difficulties, and admits of the determination of the relative rates of transmission through cables of different lengths with very great accuracy.

The machine used consisted of an axle carrying two "commutators." This axle was driven by clockwork, governed, in one case, by a fly rotating in mercury, and in the second experiment by means of a fly in the air, together with a friction spring.

The commutator consists of two wheels, each wheel being in two halves. Upon the broad edge of the wheel rest two springs, one of which is connected with one pole of the battery, and the other with the other pole. The two halves of the wheel were constantly connected, by means of two other springs, the one with the cable wire, and the other with the earth, so that when the wheel was turned round, during one half of the revolution a positive current was flowing through the cable wire, and during the other a negative. The other commutator on the same axle was precisely similar in construction; but the two springs resting on the edge of the wheel

were connected with two wires of a galvanometer, and one half of the wheel was connected with the receiving end of the cable wire tested, and the other half of the commutator was connected with the earth.

The two commutators were so arranged that when, by the rotation of the wheel, the current of electricity from the battery was reversed, the connexions of the galvanometer were reversed also; and therefore, if the speed of the electric wave through the cable were indefinitely great, the currents would flow through the galvanometer in one direction, no matter how fast the currents in the cable are reversed. As, however, a given amount of time elapses before these waves reach their maximum at the distant end of the circuit, and as also a given time elapses after the battery has been reversed at the one end before the current is reversed at the other or distant end, it is clear that by gradually augmenting the rate of rotation of the commutator until the wheel is a quarter of a revolution in advance of the wave, a point is arrived at when the galvanometer's connexions are reversed precisely at the moment that the wave reaches its maximum strength, and consequently the wave is bisected, one half of it flowing through the galvanometer in one direction, and the other half in the other. At this rate of rotation the galvanometer falls to zero; because, the wave being exactly bisected, the one half tends to deflect the needle to the right, and the other to the left, but, owing to the weight of the needle and the rapidity of the reversals, it (the needle) stands nearly steadily at zero. The galvanometer used consisted of a rather heavy astatic pair of needles suspended by a silk fibre. The wire acting upon the needles was about the twentieth part of an inch in thickness, in order that it should offer no serious resistance to the electric current. Its resistance was less than one Varley unit (1 mile copper wire $\frac{1}{16}$ inch in diameter).

The rate of rotation necessary to obtain the first zero is the point recommended for comparing the relative speeds of the electric waves through submarine cables of different dimensions.

By augmenting the speed beyond that necessary to produce the first zero, the needle becomes deflected in the opposite direction and gradually approaches a maximum; that is to say, when the electric wave is half a revolution behind, the currents all flow through the galvanometer in one direction again. This is termed the second

maximum (the first maximum being that obtained when the wheel is not rotating at all); and by augmenting the speed still more, until the wave is three-quarters of a revolution behind, the wave is again bisected and a second zero is obtained, and so on.

The great variation of speed necessary to give these and other results was such that the means then at the author's disposal in the first experiments were not sufficiently regular to admit of very accurate readings.

The experiments now communicated were made upon two cables, one containing six conducting wires, a portion of which was laid in the Mediterranean. This cable had been lying exposed to sun and weather in the East India Docks for some years, and the gutta percha had become deteriorated to a considerable extent; its exact length was not known; and from these combined causes it could not be used for determining the rate at which the wave travels through given lengths, but it has served to demonstrate that Thomson's "law of the squares" is substantially correct in practice.

In the experiments made on this cable, the resistance of the galvanometer was equal to one mile of the cable. The battery power used averaged from 12 to 36 cells of Daniell's battery, each cell offering a resistance of one-sixth of a mile of the cable.

The first experiment was made upon two wires forming a loop of about 150 miles in length; and when the currents were reversed at the rate of 15·16 per second, the needle came to zero.

The second experiment was made through three wires, that is to say, 225 miles of cable. The speed then obtained was 6·57. Through four wires (*i. e.* double the length of first experiment) 3·78 reversals per second were obtained.

Through six wires, or three times the length of the first experiment, 1·75 per second were obtained, or inversely as the square of the length.

In the foregoing experiments the current was made to pass up one wire and down the second, up the third and down the fourth, and so on; but in experiment No. 5, the current was made to pass through all the six wires, one after the other, in the same direction, the object being to determine, if possible, what amount of retardation was attributable to the magnetization of the iron covering. On the current through the first wire ceasing, a magneto-electric current is produced in the opposite direction to the first magneto-electric

current; and consequently, when the wires were so connected that the current went up one wire and down the second, up the third and down the fourth, as in experiment No. 4, the magneto-electric action upon No. 2 wire is counterbalanced by the magneto-electric action upon No. 3, and so on; but in experiment No. 5 the magneto-electric current was in full force on all the wires. The result, however, did not show any appreciable difference in the speed of the wave, as the machine then used could not be governed with sufficient accuracy.

Experiments were made to determine the effect of applying resistance to one end of the cable. For instance, a telegraphic instrument, when applied to the cable, augments the resistance of the circuit; and when a resistance equal to half that of the cable was applied at one end, the rate of the electric wave through it was decreased to three-quarters. When a resistance equal to the whole of the cable was added at one end, so as to double the resistance of the whole circuit, the speed was reduced to about three-fifths; and when resistance double that of the cable was added, the speed was reduced rather more than one-half.

Variations in the electromotive force produced no sensible variation in the speed of the waves.

The second series of experiments were tried upon the Dunwich and Zandvoort cable, after it was submerged, and consequently in a straight line, and not, as in the previous experiment, in a coiled mass; it was therefore exposed to much less magneto-electric induction. The insulation of this cable was very high indeed.

The experiments on this cable, among other results, show that doubling the length of the circuit reduced the speed nearly four times. The experiments on the Mediterranean cable showed that, with three times the length, the speed was reduced nearly nine times. With twice the length the speed was reduced nearly four times, or inversely as the square of the distance nearly.

The mean of the experiments through 270 miles of cable are 4·76 revolutions of the wheel per second, or 9·52 reversals of the current per second.

In the experiments through 540 miles, or twice the length of cable, the speed was 1·326 revolution of the wheel, or 2·65 reversals per second. The reason why they do not follow the law of the squares exactly, is probably to be attributed to the resistance of the

battery used on this occasion, and also to the fact of the magneto-electric induction of the iron exterior.

Experiment 9 shows that, on the introduction midway in the circuit of an escape (circuit dérivé), the resistance of which is equal to half the circuit, the first zero is obtained at the rate of 2·78 revolutions per second, or 5·56 reversals per second; the introduction of this escape about doubles the speed of transmission; and thus, by the establishment of a series of escapes judiciously along the cable, the speed may be augmented to a very high degree without weakening the current too much for the purposes of telegraphy. Experiments were then tried with currents of various durations; and the results of these experiments are very important, the highest speed being obtained when the cable was connected to the battery for a very short interval of time and immediately afterwards put to earth. In this way, through the 540 miles, the speed of the wave was increased from 1·326 to 3·7.

In the experiments in which resistances of various amounts were added to one end of the cable, the consequent retardations agree very nearly with the results obtained upon the Mediterranean cable. It was found to be immaterial at which end of the circuit the resistance was added: this, however, can only hold good with highly insulated wires; for it is evident, upon a little consideration of the matter, that, where the line is imperfectly insulated, the resistance added at the sending end will produce more retardation than if applied to the receiving end.

In the experiments on the second zero and second maximum, it is shown that, if the speed required to produce the first zero be taken as unity, double that speed is necessary to produce the second maximum, and four times the speed to get the second zero.

Notwithstanding the difficulties under which these experiments were made (from the necessity of using a machine the rates of whose motion could not be very accurately governed), the results are still sufficiently accurate for all "*practical*" purposes of submarine telegraphy; but such nice points as the retarding influence of the iron covering cannot be inferred with any precision from these experiments. It is certain, however, that in long cables the retarding influence of the external iron covering is so small, compared with the retardation due to electrical induction, that it may be neglected in estimating the speed of the electric wave.

"On the Production of Vibrations and Sounds by Electrolysis."

By George Gore, Esq. Communicated by Professor Tyndall. Received November 12, 1861. Read December 12, 1861*.

1. Under some circumstances, which I have already briefly recorded (Proceedings of the Royal Society, No. 44. p. 177), vibrations of singular beauty, accompanied by definite sounds, are produced at the surfaces of mutual contact of a liquid metal and electrolyte by the passage of an electric current.

2. The most convenient mode of obtaining the vibrations and sounds is as follows. Take a circular disk of thin sheet glass about 2 or 3 inches in diameter, and procure a thin hoop of glass about 1 inch wide and of the same diameter as the disk, by cutting off the end of an ordinary glass shade. Coat one edge of the hoop with melted sealing-wax, and place the hoop with that edge downwards upon the disk in an oven so that the two may become securely united. Procure another hoop about $\frac{1}{4}$th of an inch wide, and of a diameter about $\frac{1}{4}$th of an inch less than the previous one, and fix it to the same side of the disk concentric to the other in a similar manner, and make the junctions water-tight. A circular vessel surrounded by an annular space or vessel will thus be obtained, as represented in the annexed figure 1, which gives a perspective view of the complete vessel supposed to be cut in two.

Fig. 1.

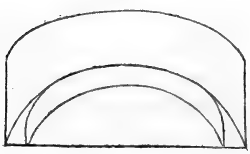
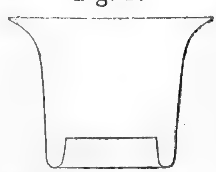


Fig. 2.



A second form of vessel may be made of the shape represented in fig. 2, open at each end; and a bottom formed to it by sticking on with sealing-wax, either a flat disk of glass or an inverted watch-glass. An equally suitable vessel may be made in one piece of glass by forming what is termed by glass-blowers a "chevril" or raised edge in the bottom of a cup (see figs. 3 and 4); or by sticking a

* An abstract of this Paper has already appeared in the 'Proceedings,' vol. xi. p. 491. It is now printed in full by order of the Council.

thin globe of glass upon the form of fig. 2 while in a molten state, and cutting off the superfluous portion when cold (see fig. 5).

The most useful form of vessel is that of fig. 5. In some cases I have employed vessels similar to that of fig. 1, with a *series* of

Fig. 3.

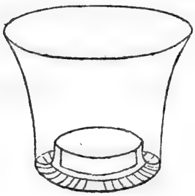


Fig. 4.

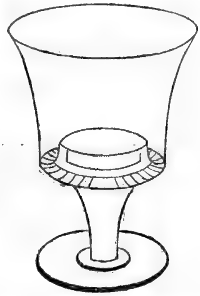
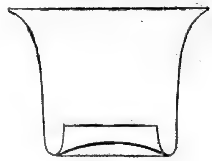


Fig. 5.



annular spaces within each other, to as many as twelve, and to a size of $8\frac{1}{2}$ inches diameter*.

3. Nearly fill the inner circular space and the annular groove with pure mercury, and cover the mercury to about half an inch or more in depth with a filtered solution composed either of 10 grains of cyanide of mercury and 100 grains of hydrate of potash dissolved in $2\frac{3}{4}$ ounces by measure of hydrocyanic acid containing 5 per cent. of anhydrous acid; or of 10 grains of cyanide of mercury dissolved in a mixture of 3 ounces of hydrocyanic acid and $\frac{1}{4}$th of an ounce of strong aqueous ammonia: the latter solution must be used directly after it is made, because it soon decomposes and loses its vibrating power.

4. Take either two or three of Grove's cells, containing platinum plates about 6 inches long and 4 inches wide, connect them intensity fashion, and immerse the positive terminal platinum wire in the circular pool of mercury, and the negative wire in the annular ring of mercury; or five Smee's cells, the acting surface of each silver plate being equal to 30 square inches, charged with one measure of sulphuric acid and nine of water, will do equally well. The polar wires should be guarded from contact with the electrolyte by small tubes of glass melted upon them, and reaching nearly to their extreme points, to prevent dissolving of the platinum and evolu-

* I beg to acknowledge the assistance I have received from Messrs. Osler, glass manufacturers, and their talented manager, Mr. Percivall, in the construction of these and other glass vessels of novel shapes. The above cups may be also obtained of Messrs. Elliott, Brothers, 30 Strand, London.

tion of hydrogen. It is essential that the electric power be within proper limits, otherwise the phenomena will not occur, and I have therefore found it very convenient to employ a wind-up battery, the plates of which can be instantly immersed to any desired depth in the exciting liquid. The one I have used in nearly all the experiments described in this paper consists of ten Smee's elements, each plate being $7\frac{1}{2}$ inches wide and $11\frac{1}{2}$ inches deep, single zincs and single silvers, and capable of being immersed about 9 inches in the exciting liquid; it is sufficiently large to produce the phenomena in rings of mercury $8\frac{1}{2}$ inches diameter and $\frac{1}{6}$th of an inch wide: with small rings of mercury the plates are immersed only $\frac{3}{4}$ths of an inch deep. It is advisable in all cases to produce the sounds by nearly the minimum of electric power, because they are then generally more uniform and more prolonged. The introduction of a coil of stout copper wire (containing a soft iron core) into the circuit will considerably assist the production of sounds if the cells are few in number.

5. Instantly on passing the electric current, a series of small elevations, which I shall term vibrations or crispations, occur upon the surface of the annular portion of mercury, and impart to it the appearance of a series of transverse ridges, all radiating from the centre of the vessel*; and these elevations or crispations are accompanied by definite sounds very similar to the humming of a large bee, and capable of being heard under favourable circumstances at a distance of upwards of 50 feet. The sounds are evidently produced by the vibrations.

6. These elevations are exceedingly definite and uniform in size; apparently all of them are at exactly the same distance asunder at any given moment if the mercury is undisturbed by other causes; the distance varies under different circumstances from about $\frac{1}{6}$th of an inch to a distance so small as to be scarcely distinguishable by the unassisted eye. The apparent distance asunder of each elevation is probably half the distance of the actual vibrations, because the mercury is raised and depressed with great rapidity at each of those spots: while one spot is in a state of elevation, the next adjoining one is depressed; and while the former spot is depressed, the latter is elevated, and so on alternately with such great rapidity as to pro-

* The appearances and their beauty cannot be satisfactorily represented by sketches.

duce upon the eye at any moment the impression of double the number of elevations that actually exist at that moment. Elevations that appeared to be about $\frac{1}{8}$th of an inch asunder emitted the note F.

7. The *position* of the elevations in very narrow rings of mercury is generally uniform, *i. e.* in lines radiating toward the centre of the vessel when the mercury is undisturbed; but with rings $\frac{1}{4}$th of an inch wide or upwards the elevations of different portions of the ring frequently fluctuate in their positions; and under much rarer conditions the whole ring of elevations revolves (and sometimes rather quickly) round an imaginary axis in the centre of the vessel, sometimes in one direction and sometimes in the other; but what the conditions are that determine the rotation or its direction I have not investigated further than to ascertain that the vibrations are not influenced by a strong magnet. The surface of the negative or vibrating mercury never exhibited *nodal* points.

8. In addition to the elevations which emit sounds, there are other movements of greater amplitude, *i. e.* undulations about $\frac{1}{4}$th of an inch in length; and also other motions of a more violent kind in negative *pools* of mercury, consisting of upheavals of mercury in large masses at irregular distances from each other, as if large bubbles of gas were beneath (but no gas was observed), especially in a solution composed of $2\frac{3}{4}$ ounces of hydrocyanic acid and 100 grains of hydrate of potash, on first electrolysing it. Both these motions, like the previous ones, occur at the cathode, but neither of them are attended by appreciable sounds. On rare occasions, with the full power of the battery exerted upon the solution given (3), undulations have also been observed in the positive mercury surface, but they emitted no perceptible sounds.

9. I have not given a full description of all the numerous appearances of the various movements, as similar phenomena (though produced by other causes) have been minutely described by Mr. Faraday in the Philosophical Transactions of the Royal Society, 1831; and my remarks will be almost entirely confined to the vibrations that produce sounds, because the other electrolytic movements have already been to a great extent observed and examined by previous investigators.

10. The phenomenon of definite sound is limited to those crispations whose widths lie within certain limits; when the widths between the elevations enlarge beyond $\frac{1}{6}$th of an inch, the sounds cease. Fre-

quently feeble sounds are heard a few seconds before crispations appear, and sometimes also a short time after they disappear, especially in a solution composed of $2\frac{3}{4}$ ounces of hydrocyanic acid, 100 grains of hydrate of potash, and 20 grains of cyanide of mercury. In a weak solution of cyanide of potassium the sounds sometimes cease or become inaudible before the elevations disappear; and in other cyanide solutions I have occasionally observed the sounds and crispations continue a short time after the electric current has been stopped; there is probably, therefore, a liquid metal and electrolyte which together will emit sounds without the aid of a battery, and I have made a few experiments (with Hg alloyed with K) to discover them, but without success. In some cyanide solutions the sounds and crispations cease very suddenly; but in the one I have given (3) they remain nearly uniform about ten or fifteen minutes with a feeble electric current, and then diminish gradually as a film is formed upon the anode. The crispations are more steady with a moderate number of large voltaic elements than with a large number of small elements. On some occasions, especially with many elements of small surface, two sets of crispations, one about half the width of the other, will coexist and succeed each other alternately.

11. In every case the PITCH of the sound varied with the *width* of the crispations; the more base the sound, the greater was the distance between the elevations. The loudness of the sound appeared to depend partly upon the depth of the crispations; but this point I have not determined; if the specific gravity of mercury was less, or that of the solution greater, the crispations would probably be deeper and the sounds louder. Shallow crispations formed upon mercury in a solution of iodide of potassium evolved no perceptible sound, although their width was about $\frac{1}{12}$th of an inch. No loud sounds have been obtained. Rings of mercury $8\frac{1}{2}$ inches diameter gave no louder sounds than rings $2\frac{1}{2}$ inches diameter, although the quantity of electricity circulating was much larger, the plates of the battery referred to (4) being immersed 9 inches deep in the former case and only 1 inch in the latter case. A certain amount of *time* is occupied in the development of the vibrations, and is distinctly observable where the electric current is feeble; the vibrations and sounds being gradually developed as the mechanical resistances opposed by inertia, gravity, cohesion, adhesion, friction, &c. of the liquids are gradually overcome. The vibrations are attended by movements in the *mass* of

the liquids, consisting of currents of the electrolyte which flow from the positive to the negative electrode, and of the mercury which flow in an opposite direction; these I shall leave out of consideration, as they are only *secondary* results.

12. Nearly all the experiments described in this paper were made with the solution of cyanide of potassium and mercury already described (3).

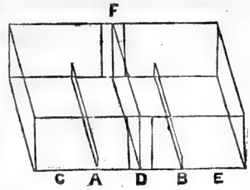
Influence of the Electrodes.

13. The crispations and sounds never (or very rarely, and under conditions which I have not recently met with) occur at the anode; nor had the mass, size, form, or position of surface, distance, chemical composition, or physical condition of the anode any effect upon the direction of the rows of elevations, nor any *direct* effect (their *indirect* effects will be described hereafter) upon the sounds. The elevations and sounds occurred with anodes of all sizes and shapes, and in all positions; also with anodes of platinum wire and sheet. A ring cathode of mercury within a ring anode of mercury gave the sounds equally well. By using a straight and narrow strip of mercury as anode inside a narrow ring cathode, no difference in the size of the crispations at different parts of the ring occurred.

14. With regard to the influence of the cathode, the sounds occurred as well with a very shallow layer of mercury as with a deep mass. Sounds were obtained with nearly as great facility in the central pool (if not too large) by making that the cathode, as in the annular portion. The direction of the rows of elevations was dependent upon the form of the boundary outline of the negative mercury surface; a rectangular surface gave rectangular rows, and a circular surface gave circular rows, and in each case the rows were parallel to the boundary of the mercury surface: a small shallow ring of gutta percha held upon the surface of a large rectangular cathode of mercury caused the rows of crispations to be circular inside the ring, whilst the rows outside remained rectangular. A convenient apparatus, though a temporary one, for examining the direction of the rows of crispations was made by joining together rectangular pieces of thin sheet-glass by sealing-wax in the form shown in fig. 6. The vessel is 3 inches long, 2 inches wide, and 1 inch deep; its lower part is divided into three equal portions by two strips of glass, A and B, about $\frac{1}{4}$th of an inch wide, so as to form spaces C, D, and E for three pools of mercury; a vertical slide of glass, F, rests by its lower edge upon the

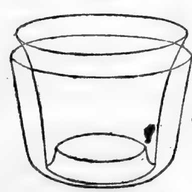
central pool of mercury and separates the supernatant electrolyte into two equal portions, and causes the central portion of mercury to form two connected pools, and to act as a cathode on one side and as an anode on the other, the polar wires being immersed in the outer pools. The sounds and crispations were more definite with narrow strips of mercury $\frac{1}{8}$th of an inch wide than with wider ones, and with annular ones than with circular or rectangular pools: the annular strip may be of any moderate diameter; the best width of mercury to give a definite sound is about $\frac{1}{10}$th of an inch, because there is then only room for two rows of moderate-sized elevations; when the rows are numerous, as in a circular or rectangular pool, the vibrations (and the sounds) are frequently irregular, and interfere with each other. On several occasions fine white sand was sprinkled upon variously shaped negative surfaces of vibrating mercury, but no signs of nodal points were detected. The *liquid* state of the cathode appeared to be essential, as no sounds were produced on substituting a fine stretched platinum wire for the negative mercury. The anode nearly always contracts, and the cathode lengthens during the electrolysis, as may be easily verified by employing *segments* of rings of mercury for the electrodes.

Fig. 6.



15. To ascertain the influence of the chemical nature of the cathode, the glass vessel, fig. 2, open at its lower end, was placed in an outer vessel A, fig. 7, and portions of Dr. Wood's fusible alloy, composed of 1·5 part cadmium, 2 parts tin, 4 parts lead, and 7·5 parts bismuth, were placed in the central cavity and in the annular space; the vessels were then filled about one inch deep, in one experiment with a filtered mixture of 1 ounce of hydrocyanic acid, $\frac{1}{2}$ an ounce of water, and 25 grains of hydrate of potash; and in another experiment with 1 ounce of hydrochloric acid, 1 ounce of water, and 30 grains of chloride of potassium, and the liquid heated by means of a sand-bath to about 180° F. to liquefy the alloy*;

Fig. 7.



* I found by experiment that this alloy solidified at 150°·8 F., and in solidifying

various degrees of battery power were then applied in the usual manner, occasionally reversing the electric current, but no crispations or sounds, nor even undulations, occurred in either case.

Influence of the Electrolyte.

16. The *mass* of the electrolyte is not essential to the sounds, it only increases them by enabling a larger quantity of electricity to circulate; the sounds were readily produced with the thinnest film of liquid upon the surface of the mercury. Several experiments were made to ascertain if the depth of crispations and loudness of sounds would be increased by increasing the specific gravity of the electrolyte by addition of various salts; but no definite effects of that kind were found on gradually adding portions of saturated solutions of carbonate of potash and bromide of potassium to a good phonetic liquid.

17. With regard to the influence of the chemical composition of the electrolyte upon the vibrations and sounds, upwards of one hundred liquids, including organic and inorganic acids, concentrated and dilute—aqueous solutions of caustic alkalies—alkaline carbonates, bicarbonates, borates, hypophosphites, phosphites, phosphates, pyrophosphates, hyposulphites, sulphites, sulphates, bisulphates, iodides, iodates, bromides, bromates, chlorides, chlorates, perchlorates, fluorides, nitrites, nitrates, silicates, tungstates, chromates, bichromates, and manganates—salts of alkaline earths, of alumina, chromium, uranium, manganese, and of various organic acids, all of various degrees of dilution, and with different degrees of battery power, were tried, to ascertain if the property of evolving sounds by electrolysis with mercury electrodes was a general property of electrolytes, or only of certain liquids; but the only ones in which the sounds occurred were moderately dilute aqueous solutions of the cyanides of ammonium, potassium, sodium, barium, strontium, calcium, and magnesium, and best in the cyanides of ammonium and potassium; no sounds occurred in aqueous hydrocyanic acid, nor in a solution of cyanide of mercury. In nearly all the liquids, movements of the mercury, more or less strong, were produced; and in solutions of alkaline hyposulphites, iodides, and bromides, especially the iodides, definite undulations ap-

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evolved sufficient heat to raise its own temperature to 155°·3 Fahr. = 4·5 Fahr. degrees. I also previously found that the solution of cyanide of potassium named gave sounds (after a little use) readily at 180° Fahr. with mercury electrodes.

proaching in size and appearance to the crispations were produced, but in no instance did the sounds occur. Solutions of iodide of ammonium and iodide of potassium did not yield sounds with an electric current made intermittent by means of a contact-breaker. In dilute phosphoric acid the anode enlarged, *i. e.* fell more flat, and the cathode contracted*.

18. Numerous experiments were made to determine the most suitable composition of the electrolyte, also to ascertain the effects of deficiency or excess of each of the ingredients. 1st. With regard to the hydrocyanic acid and hydrate of potash, the best proportions were found to be their chemical equivalents (or a slight excess of acid); if either the acid or alkali was in much excess, the latter in particular, films soon formed upon the mercury anode and stopped the action; a very large excess of the acid made the solution liable to become brown and lose its phonetic capacity. 2nd. With regard to the cyanide of mercury, if it was omitted, or if it was present in too small a quantity, the sounds did not occur immediately upon applying the electric current, but only took place after the current had circulated some time (being occasionally reversed in direction), and had thereby caused sufficient cyanide of mercury to be formed; and if it was in excess, the sounds were irregular and feeble. And 3rd. If the solution was diluted with water, the loudness of the sounds diminished, and by further dilution the sounds were altogether prevented; if the solution contained less than 10 or more than 50 grains of cyanide of potassium per ounce, the sounds rarely occurred. An equivalent quantity of strong aqueous ammonia was substituted for the hydrate of potash with equal advantage, except that the solution was much more liable to decompose and become brown; and it is probable that the other alkalies might also be substituted for the potash with success. When a solution becomes brown, it loses its phonetic power; but this decomposition may be prevented, or at least greatly hindered,

* In examining these liquids, I observed that if mercury was shaken strongly in a slightly decomposed mixture of 2 ounces of strong hydrocyanic acid, 1 ounce of water, and 30 grains of calcined magnesia, it remained in a finely-divided state during several weeks, and might be washed many times with water without the globules reuniting; a similar quantity of alumina, or 10 grains of hydrate of potash, was substituted for the magnesia with similar but less effectual results, which, with the potash liquid, were observed only after it was partly decomposed and of a brown colour.

particularly in the cyanide of potassium solution, by not employing too great an excess of hydrocyanic acid, and electrolysing the mixture with mercury electrodes as soon as it is made. From the various results obtained, it is evident that the sounds only occur under very special, limited, and probably complex conditions.

19. The presence of certain impurities in the electrolyte did not prevent the vibrations or sounds; for instance, chloride of ammonium, nitrate of ammonia, or bromide of potassium, added in moderate quantities to the cyanide of potassium solution, produced no conspicuous effect.

20. After the current has been passed a long time in one direction and is then reversed, the crispations and sounds do not occur at the cathode for a little time, partly because a layer of liquid containing an excess of cyanide of mercury has been formed at the surface of that electrode, and a layer of liquid with a deficiency of cyanide of mercury has been formed at the other electrode by the previous electrolytic action; diffusion of these layers by stirring the liquid hastens the reproduction of the crispations and sounds.

21. By repeating the phonetic experiment many times with unguarded wires (4), the cyanide solution gradually loses its power of producing the sounds, probably from acquiring an excess of cyanide of mercury by electrolysis, hydrogen being evolved and mercury replacing it: the loss of power does not result from an alteration in the electrodes, because with the same mercury and a new solution the power was restored, whilst with fresh mercury and the same liquid the power was not restored. The mercury remains unaltered, except absorbing a trace of alkali metal, which is readily removed by washing, and it is therefore not necessary to take fresh mercury for fresh experiments.

Influence of Mechanical circumstances and Temperature.

22. In some solutions (or with a feeble battery), where the crispations did not spontaneously occur, a slight blow with the end of a glass rod against the negative ring induced them to commence, and also a similar blow sometimes stopped them. Frequently, also, when the vibrations have stopped, stirring the liquid reproduces them by mixing the layers of altered liquid that accumulate about the electrodes.

23. Raising the temperature of the solution to about 200° Fahr. appeared to have no *direct* effect upon the vibrations, it only affected them indirectly by allowing more electricity to circulate. The apparatus employed for hot solutions was that shown in fig. 7 (15).

Influence of the Electric Current.

24. In nearly all cases the vibrations and sounds were essentially dependent upon the passage of the electric current, and ceased the instant the current was stopped; the exceptional cases being those already mentioned (10), in which the sounds continued a short time after the current was stopped: it is probable that in those cases local electric currents were produced by the action of the water upon a minute quantity of potassium contained in the mercury, and which had been deposited by the previous electrolysis.

25. With regard to the influence of the *direction* of the electric current; the vibrations and sounds only occurred where the current passed from the electrolyte into the metal, never (or very rarely, and under conditions which I have not recently met with) where it passed from the metal to the solution. No direct effect of the direction of the current in the mass of the electrolyte upon the crispations was detected by passing the current either vertically or horizontally through the solution into the cathode.

Influence of Size and Number of the Voltaic Elements.

26. Twenty Smee's elements, $2\frac{1}{2}$ inches wide, and immersed $\frac{1}{4}$th of an inch deep, with a mercury ring $1\frac{3}{4}$ inch diameter and $\frac{3}{16}$ths of an inch wide, gave crispations $\frac{1}{7}$th of an inch wide, and a coarse base sound; whilst four Smee's elements, $7\frac{1}{2}$ inches wide, and immersed 9 inches deep in the same acid mixture, gave, with the same ring, fine crispations about $\frac{1}{32}$nd of an inch wide, and a high tone; and one Grove's cell, with a platinum plate 12 inches wide and 18 inches deep, and the same ring, gave very faint sounds of high tone without visible crispations in the negative ring, and with fine crispations if the *pool* of mercury formed the cathode. The crispations produced by the twenty small Smee's cells (charged either with acid 1 to 8 or 10 of water or 1 to 24) were less regular than those produced by ten elements of the same surfaces, or five pairs of the larger elements.

27. The least number of Grove's cells, containing platinum plates

6 inches long and 4 inches wide, with which I have been able to produce a *continuous* sound, has been one; and of Smee's cells three (28), except under certain special conditions (36): the most suitable number of the former is two, and of the latter from five to ten, according to their size; if this amount of power was much exceeded, the action became violent, then quiescent, and the sounds ceased.

28. With a ring of mercury $\frac{1}{9}$th of an inch wide and $1\frac{7}{8}$ inch diameter, the *smallest* number of the Smee's cells (4),—1st, immersed $8\frac{1}{2}$ inches, with which the sounds could be obtained was *two*; the pitch of the sound was rather high and very feeble; it lasted only about three seconds, and was not attended by any visible vibrations: 2nd, immersed 5 inches, was *three*; the tone was high and sound feeble, for a few seconds with visible crispations, and continued after that more feeble and without visible vibrations: and 3rd, immersed $\frac{1}{2}$ an inch, was *four*; the sound was steady, tone moderately high, and crispations $\frac{1}{12}$th of an inch wide. And the *largest* number that could be successfully used,—1st, immersed $8\frac{1}{2}$ inches, was *eight*, and occasionally *nine*; the crispations were then very narrow and the action violent: and 2nd, immersed 5 inches, was *ten*, crispations very narrow and action violent. The largest surface of the *ten* Smee's elements that would produce definite crispations and sounds was about $7\frac{1}{2}$ inches deep by $7\frac{1}{2}$ inches wide; and the smallest surface of *three* elements that would effect it was about 1 or 2 inches deep by $7\frac{1}{2}$ inches wide, and then only faint sounds were produced, lasting about two seconds, and unattended by visible vibrations. It is worthy of remark, that a small number of elements of large surface always produced small crispations and high sounds.

Influence of Quantity of the Current.

29. The width of the vibrations and the pitch of the sound are closely connected with, though not solely dependent upon (40), the quantity of electricity which passes into a given amount of mercury surface in a given time. With a surface of given size, the width of the crispations was invariably decreased, and the pitch of the sound raised, by either increasing the number of the plates (their depth of immersion remaining the same) or their depth of immersion. To ascertain the most suitable quantity of electricity, a voltameter was included in the circuit with the phonetic liquid; the negative ring of

mercury was $2\frac{1}{4}$ inches external diameter, and $\frac{1}{10}$th of an inch wide =about 0·675 square inch of surface; the battery consisted of ten Smee's elements $7\frac{1}{2}$ inches wide (4) immersed $1\frac{1}{2}$ inch in the exciting liquid, and the quantity of hydrogen evolved in the meter whilst the ring produced a moderate and steady sound, was 0·47 cubic inch in three minutes; therefore, with the solution of cyanide of potassium given (3), the quantity of electricity required to give a good sound in a ring containing 1 square inch of mercury surface is equal to that which will evolve about 0·232 cubic inch of hydrogen per minute; this quantity approaches the minimum required for the purpose. In a series of four other experiments with a ring of mercury containing just one square inch of surface, and yielding a steady sound, the following quantities of hydrogen were evolved in 3 minutes:—A 0·7, B 0·69, C 0·71, and D 0·685 cubic inch; that is, A 0·233, B 0·230, C 0·236, and D 0·228 cubic inch per minute: average = 0·2317 cubic inch per minute.

30. In all cases an increase in the quantity of electricity passing (either by increase in the depth of immersion of the battery plates, enlargement of the anode, diminution of the cathode, diminution of conduction-resistance in the circuit, removal of films from the anode by stirring, mixture of strata of exhausted electrolyte by stirring, rise of temperature of the electrolyte, &c.) was attended by an increase in the number and a diminution of the width of the crispations, until at length, by excessive quantity of electricity passing, the movements became violent, and the crispations and sounds suddenly ceased. Too large or too small a quantity of electricity produced undulations (8) without sounds.

31. With a given electric current, a wide strip of mercury gave wide crispations and a base sound, and a narrow strip gave narrow crispations and a high sound, because in the latter case the quantity of electricity passing into a given amount of surface was greater than in the former. Diminution of the conductivity of the electrolyte by dilution with water appeared to decrease more the *loudness* of the sounds than to alter their note, probably by decreasing the specific gravity of the solution, and thus diminishing the *depth* of the crispations.

32. In producing the sounds in a circular pool of mercury by gradually immersing the battery plates, the first effects are furrows

in the surface of the negative mercury parallel to the boundaries of its surface, *i. e. circular* furrows, one within the other; on immersing the plates deeper, transverse furrows occur superimposed upon the others, and thus give rise to the little heaps or elevations (5, 6, 7) of mercury; the sounds do not occur until the *latter* furrows are produced.

Similar effects may be produced by *mechanical* means: if a watch-glass containing a little mercury is affixed by means of sealing-wax to a flat and horizontal strip of window-glass about 8 inches long and 3 inches wide, supported firmly at its ends, and vibration be induced in the mercury by resting the end of a vertical glass rod upon the strip of glass, and drawing wet fingers with pinching pressure down the rod, and the vibration be gradually augmented, a similar series of changes will be produced by the gradual increase of mechanical power to those produced by a gradual increase of electric power.

Influence of the Vibrations upon the Electric Current.

33. It was found, by interposing a galvanometer (with a short and thick wire) in the circuit, that when the vibrations ceased, as they sometimes do very suddenly, the quantity of the electricity passing was instantly diminished, the needles shifting from 22 degrees to 18—an effect, no doubt, of exhausted solution accumulating at the electrodes. To ascertain if the vibrations of the mercury and electrolyte made the electric current *intermittent*, the current from three Smee's elements, $7\frac{1}{2}$ inches wide, immersed $8\frac{1}{2}$ inches in the exciting liquid, was allowed to circulate through the primary wire of a Ruhmkorff's induction-coil (kindly lent to me by Professor G. G. Stokes) (the break-hammer being excluded) and the phonetic liquid, the ends of two fine platinum wires from the secondary terminals being in contact with a drop of solution of iodide of potassium, but no signs of decomposition of the iodide could be detected with the aid of an eye-glass; on applying, however, the secondary terminals to my tongue, rather sharp shocks were experienced, and could be very distinctly felt with the damp fingers of my two hands; also a piece of iron of proper size, and suitably held near or in slight contact with the iron core of the coil, could be distinctly felt to vibrate. If only two of the elements, immersed $8\frac{1}{2}$ inches, were employed, and the mercury electrodes reversed for a short time and then returned to

their former position, and thus a temporary suspension of the vibrations (20) was obtained, no shocks were experienced at the secondary terminals until the vibrations spontaneously recommenced—they were then felt distinctly; nor were any shocks felt whilst a platinum cathode of equal surface (one square inch) to the one of mercury was employed as a substitute for the ring, although an equal quantity of electricity appeared to be circulating; nor were any shocks experienced with two Smee's elements whilst the *pool* of mercury formed the cathode, because no vibrations of the mercury then occurred; but on increasing the number of elements to four (immersed the same depths) vibrations commenced, and induction currents occurred and continued, not only whilst the *phonetic* crispations existed, but also whilst only the circular furrows (32), which emit no perceptible sound, were visible. The absence of induced currents with a battery nearly but not quite strong enough to produce vibrations of the mercury, did not arise from a stoppage of the battery current; for by placing a voltameter in the circuit, conduction was found freely, whilst no movements of the mercury could be seen. The strength of the induced currents increased with the loudness of the sounds. It appears, therefore, that the intermittency was not produced by simple electrolysis of the solution, but by the vibrations, and was not limited to such vibrations as produced audible sounds.

Influence of Coils of Wire in the Circuit.

34. Experiment 1. A coil of copper wire containing 154 feet of size "No. 26" was included in the circuit with eight Smee's elements (4) immersed $8\frac{1}{2}$ inches, and a phonetic vessel: without the coil the vibrations were very fine, and the sound acute and loud; but with the coil they were coarse, and the sound base and feeble. Experiment 2. A coil containing 272 feet of "No. 19" copper wire included in the circuit produced similar effects. And, Experiment 3. A ring of soft iron weighing $27\frac{1}{4}$ pounds, outer diameter 12 inches, inner diameter 8 inches, formed of cylindrical metal 2 inches thick, had wound upon it in the manner of an electro-magnet $152\frac{1}{2}$ feet of four parallel and separately-insulated copper wires, size "No. 17." Six pairs of plates, immersed $8\frac{1}{2}$ inches, were connected in circuit, first with one length, and then with two, three, and four continuous lengths of the wire; every additional length of the wire made the vibrations

wider, the sound baser and more feeble. With three elements only and the total length ($610\frac{1}{2}$ feet) of wire in the circuit, the crispations were coarse and the sounds very feeble; and on adding to the length of the wire by interposing an electro-magnet* containing 390 feet of "No. 12" copper wire, the vibrations and sounds were scarcely perceptible. With twenty Smee's elements, $2\frac{1}{2}$ inches wide and $\frac{1}{4}$ inch deep, crispations $\frac{1}{7}$th of an inch wide occurred, and were unaffected by introducing 240 feet of coiled double copper wire (size "No. 17") into the circuit; whilst with four Smee's, $7\frac{1}{2}$ inches wide and 9 inches deep, the crispations were $\frac{1}{32}$nd of an inch wide, and instantly became $\frac{1}{10}$th of an inch wide on introducing the 240 feet of double wire into the circuit; and with a single Grove's cell, platinum plate 18 inches deep and 12 inches wide, a high and very feeble sound occurred (without visible crispations unless the *pool* formed the cathode); but on interposing the 240 feet of double wire, crispations of moderate width at once occurred in the ring, and evolved a loud sound. Other similar experiments yielded similar results. In all cases the greater the length of the wire (and apparently also the smaller its diameter) up to a certain limit, the louder and more steady were the sounds; and beyond that the feebler were the sounds, until at length both vibrations and sounds entirely ceased.

35. It was repeatedly observed that the smallest number of Smee's elements, immersed $8\frac{1}{2}$ inches, with which the sounds could, under ordinary circumstances, be produced, was three; but on interposing the primary wire of a Ruhmkorff's coil in the circuit, or the wire of the electro-magnet (34*), continuous vibrations and sounds were obtained with only two elements; and with the same battery and a mixed gas voltameter† in the circuit, it required seven to produce sounds without the coil, and five with it.

36. To further examine the influence of coils of wire upon the range of battery power which might be employed, I made a number of experiments, which are not necessary to be described in detail, as they are all of a similar character to those just described (34, 35); the results are as follows:—1st. The introduction of a suitable coil of

* The electro-magnet consisted of a horseshoe 14 inches from poles to bend, formed of a cylindrical bar $1\frac{1}{4}$ inch thick.

† The voltameter contained 33 square inches of acting surface of platinum in each electrode, and was filled with a mixture of 1 measure of pure sulphuric acid and $3\frac{1}{2}$ measures of distilled water.

copper wire (*i. e.* 300 or 400 feet of sizes "Nos. 10 to 14"), containing a massive iron core, enlarged the phonetic range of battery power in each direction; it enabled a less number of elements and a less surface of plate to produce continuous sounds, and also enabled a larger surface of the largest number (10) to be used; in the latter case it probably acted in part by diminishing the quantity of electricity. 2nd. An *extreme* length of thick wire (equal to between 900 and 1000 feet of size "No. 12"), coiled upon massive iron, also enabled a less number of elements to produce a continuous sound, but the vibrations were wider and the sounds much more feeble than with a *medium* length. And, 3rd. A *short* length of thick wire (30 feet of size "No. 10") without an iron core, or a long length of thin wire (154 feet of size "No. 26") without an iron core, did not enable a smaller number of elements to produce continuous sounds. With a small number of large elements, and a coil of thick wire to assist in developing the crispations, it was frequently the case that the sounds did not occur for a short time; and in still feebler cases the mercury required the assistance of mechanical disturbance (22) to enable the crispations and sounds to commence. It appears singular that although a coil of wire must make the current more feeble, it should enable a battery to produce the sounds which was already too feeble to produce them.

37. A massive unexcited thermo-electric battery of thirty pairs of bismuth and antimony, interposed in the circuit with four Smee's elements $7\frac{1}{2}$ inches wide and 9 inches deep, had no perceptible effect upon the vibrations. A fine platinum wire 2 inches long, interposed in a circuit with a battery and phonetic vessel, became red-hot, and caused the vibrations to become wider, and the tone of the sound lower than when the wire was excluded, probably by diminishing the quantity of the current. No new effects were observed on using an intermittent current from the primary wire of a small induction-coil instead of the unaltered current direct from the battery.

Influence of Induction Coils and Iron Cores.

38. A current of electricity from three Smee's elements immersed $8\frac{1}{2}$ inches was passed through the primary wire of a Ruhmkorff's coil (without a break-hammer) and a phonetic vessel; on closing the secondary circuit by a drop of solution of iodide of potassium or by

a wire, the crispations became about one-half their previous width and more steady; the sounds also increased in loudness, and their tone rose considerably. On gradually increasing the number of elements from three to ten, and at the same time gradually diminishing their depth of immersion to $\frac{3}{4}$ths of an inch, these effects of the closed secondary circuit gradually and considerably diminished, and ceased altogether when twenty Smee's elements, $2\frac{1}{2}$ inches wide, and immersed about 1 or 2 inches, were employed. If the current from the ten large elements was passed through the fine or secondary wire of the coil, and a very small phonetic cathode of mercury $\frac{1}{2}$ an inch long and $\frac{1}{10}$th of an inch wide, similar effects were obtained on closing the circuit of the inner coil of large wire.

39. On inserting a cylindrical bundle of iron wires, 6 inches long and $1\frac{3}{4}$ inch diameter, in the axis of the coil of Experiment 2 (34), the coil being in circuit with a phonetic vessel, and three Smee's elements, the crispations became a little wider and the sounds a little more base. Also one length, $152\frac{1}{2}$ feet of "No. 17" copper wire, wound upon the massive soft iron ring of Experiment 3 (34), in circuit with seven elements, immersed $8\frac{1}{2}$ inches, and a phonetic liquid, lowered the tone of the sounds much more than a coil of $137\frac{1}{2}$ feet of "No. 19" copper wire containing no iron core. Much more conspicuous effects of the presence of an iron core were obtained as follows:—The coil of Experiment 2 (34) was connected in circuit with two Smee's elements immersed $8\frac{1}{2}$ inches, and a phonetic ring $1\frac{7}{8}$ inch external diameter and $\frac{1}{9}$th of an inch wide; very narrow crispations occurred, which lasted only about ten seconds; but on inserting the cylindrical bundle of wires, the crispations became double their previous width, and the sounds baser and long continued. And in a second experiment with the coil connected as a double wire 136 feet long, without the core the crispations were continuous and rather narrow, and with the core they were much more coarse and the sounds more feeble.

40. But the most decisive effects, both of core and secondary coil, were obtained with a moveable core $8\frac{1}{2}$ inches long and $2\frac{1}{8}$ inches diameter, containing 6 lbs. of size "No. 32" annealed iron wires; a primary coil consisting of 240 feet of double copper wire, size "No. 17," in circuit with four Smee's elements (4) connected as two pairs, and a secondary coil containing 4243 feet of "No. 26"

copper wire. The presence of an iron core in the coil, therefore, has a similar effect to the coil itself, *i. e.* it makes the vibrations wider and the pitch of the sound lower; and in making the crispations wider, up to about $\frac{1}{10}$th of an inch, the sounds also become louder, and beyond that width the sounds become more feeble; and the presence of a closed secondary coil has the reverse effect, it makes the vibrations narrower, and the pitch of the sound higher. These effects of an iron core diminished on increasing the number of elements to ten, and decreasing their depth to $\frac{3}{4}$ths of an inch, and ceased entirely on employing 20 Smee's elements $2\frac{1}{9}$ inches wide, immersed 1 or 2 inches, the same as with the closed secondary (38).

41. The effects of a closed secondary coil and of an iron core upon the vibrations produced by a current from a battery of low intensity being of opposite kinds, if the two are suitably adjusted, they neutralize each other's effects. The moveable secondary coil containing 4243 feet of "No. 26" copper wire was arranged with the iron core and primary coil of the previous experiment (40), and six of the large Smee's elements (4) connected as three: with the core and without the closed secondary coil, the crispations were very wide; with the closed secondary and without the core, they were very narrow; and with the core and closed secondary acting together, they were of medium width, about the same as if neither the core nor the closed secondary were present: the effects were very conspicuous. These opposite effects of an iron core and closed secondary coil upon the phonetic vibrations are analogous to their opposite effects upon the brightness of the spark at the break-hammer of an induction-coil; the iron core increases, and closing the secondary coil decreases that brightness. A cylindrical core of antimony 9 inches long and $2\frac{1}{4}$ inches diameter, employed instead of the iron core, had no perceptible effect upon the vibrations.

42. A pole of a strong electro-magnet (34) was applied to the end of the iron core of the Ruhmkorff's coil whilst the primary wire (without a break-hammer) was in circuit with a phonetic liquid and three Smee's elements (4) immersed $8\frac{1}{2}$ inches: with the secondary coil closed, and small crispations occurring, magnetization of the electro-magnet slightly raised the tone of the sounds if the magnetic poles in contact were dissimilar, and lowered it if they were similar; and with the secondary circuit open and large crispations occurring, the chief

effect of the magnet was at the moment of its demagnetization; it then affected the tone of the sounds momentarily, and the *position* of the crispations strongly, both when the approximated poles were similar and when they were dissimilar, but in the greatest degree when they were similar.

Influence of Electrolytes in the Circuit.

43. No new effects were observed on interposing in the circuit with a phonetic vessel an electrolytic cell containing mercury electrodes and a solution of iodide of potassium or dilute sulphuric acid, they only appeared to influence the vibrations and sounds by diminishing the quantity of the electric current. Several vessels containing phonetic solutions and annular mercury electrodes were placed in one circuit, and sounds readily produced in all of them at the same time.

Influence of Magnetism.

44. Many experiments were made to ascertain if magnetism affected the vibrations; the phonetic liquid, with cathodes of various sizes, was placed in every imaginable position to the electro-magnet (34) strongly excited, in some cases with the armature of the magnet dipping into the electrolyte, but no effect of the magnetism upon the size or direction of the crispations (or in any other way) was detected.

Communications received since the end of the Session.

I. "On the Synthesis of Tribasic Acids." (Preliminary Notice.) By Maxwell Simpson, M.B., F.R.S. Received July 3, 1862.

I have already shown* that the cyanides of the diatomic radicals (at least those which form glycols) yield bibasic acids when treated with potash, which contain four equivalents of carbon more than the radicals from which they are derived. Analogy would lead us to expect that the cyanides of the triatomic radicals would yield with

* Philosophical Transactions for 1861, p. 61.

the same reagent tribasic acids, containing six equivalents of carbon more than the original radicals. Thus, as we obtain in this way from cyanide of ethylene (to take a particular case) a bibasic acid of the composition $C_8H_6O_8$ (succinic acid), so from the tercyanide of allyle ($C_6H_5Cy_3$), if the analogy holds good, one ought to get a tribasic acid having the composition $C_{12}H_8O_{12}$:—

$$C_4H_4Cy_2+2\left(\left.\begin{matrix}K\\H\end{matrix}\right\}O_2\right)+4\,HO=\left.\begin{matrix}C_8H_4O_4''\\K_2\end{matrix}\right\}O_4+2\,NH_3$$

$$C_6H_5Cy_3+3\left(\left.\begin{matrix}K\\H\end{matrix}\right\}O_2\right)+6\,HO=\left.\begin{matrix}C_{12}H_5O_6'''\\K_3\end{matrix}\right\}O_6+3\,NH_3$$

The following experiments were performed with the view of determining this point.

One equivalent of terbromide of allyle ($C_6H_5Br_3$) and three equivalents of pure cyanide of potassium were made to react upon each other, by exposing them, with the addition of a considerable quantity of alcohol, to the temperature of a water-bath in well-corked soda-water bottles. After 16 hours' heating, I found that almost all the cyanide of potassium had been converted into bromide. I then separated the alcohol, which I assume contained impure tercyanide of allyle in solution, and introduced it together with a quantity of solid potash into a large balloon. To this a reversed Liebig's condenser was attached, and heat applied by means of a water-bath, which occasioned the evolution of a large quantity of ammoniacal gas. As soon as the disengagement of this gas had ceased, the alcohol was distilled off, and the residue cautiously treated with nitric acid in excess. This liberates an organic acid from the potash, and at the same time partially destroys a black tarry matter which is present in large quantity. The whole was evaporated to dryness at a low temperature, and the dry mass treated with alcohol, which dissolves the free organic acid, but not the nitrate of potash which accompanies it. The residue obtained on evaporating the alcohol was then neutralized by ammonia, and precipitated by nitrate of silver. Finally, the acid was separated from the silver-salt by sulphuretted hydrogen, and twice crystallized from water. The crystals gave on analysis numbers agreeing very well with the formula $C_{12}H_8O_{12}$. I obtained 41·24 per cent. carbon and 4·82 hydrogen, instead of 40·91 carbon and 4·54 hydrogen. I have also

analysed the silver-salt of this acid, and obtained results establishing the formula $\left.\begin{matrix} C_{12}H_5 \\ Ag_3 \end{matrix}\right\} O_{12}$.

This acid forms nearly colourless crystals, which are very soluble in water, alcohol, and ether. They have an agreeable acid taste. The free acid gives an abundant precipitate with acetate of lead, soluble in strong acetic acid. The neutralized acid forms with perchloride of iron a reddish-brown precipitate, from which the acid can be partially removed by solution of ammonia. Neither chloride of barium nor chloride of calcium affects the neutralized acid. An abundant precipitate, however, makes its appearance on the addition of alcohol to the mixed solutions. These reactions resemble those of succinic acid. It is, however, readily distinguished from that acid by its behaviour when exposed to heat. Thus it melts when exposed to the temperature of about 158° Cent., and when subjected to a higher temperature suffers decomposition.

In order to satisfy myself that the nitric acid employed in the process I have just described does not play a part in the reaction which generates the new body, as is necessary if the equation given at the commencement of this paper is correct, I have endeavoured to prepare it without the aid of that acid. In this I have succeeded, the body formed being identical in properties and composition with that obtained by the first method.

That this acid is tribasic, we cannot well doubt, if we take into consideration the manner of its formation, and the composition of its silver-salt. Nevertheless I will endeavour, by the examination of several salts, to obtain further evidence on this point.

I regret to say I have not yet succeeded in rendering the tercyanide of allyle sufficiently pure for analysis. On evaporating the alcohol, in the midst of which the terbromide of allyle and cyanide of potassium have reacted, a black mass is obtained exactly like tar in colour and consistence. This I partially purified by solution in ether. On evaporating the ether a brown liquid is left, which is very soluble in water.

This gave, when gently heated with sodium, cyanide of sodium. When treated with potash, it yielded an organic acid and ammoniacal gas. With muriatic acid it gave an organic acid also, no doubt the same, and muriate of ammonia. These three reactions, coupled with

the composition of the acid formed in the second, furnish almost conclusive evidence of the generation of tercyanide of allyle by the action of terbromide of allyle on cyanide of potassium.

The relation that exists between di- and tri-atomic acids is well seen when we formulate these bodies according to the carbonic acid type:—

$$2\,HO, C_4\,H_4'' \left\{ \begin{matrix} C_2\,O_2 \\ C_2\,O_2 \end{matrix} \right\} O_2 \qquad\qquad 3\,HO, C_6\,H_5''' \left\{ \begin{matrix} C_2\,O_2 \\ C_2\,O_2 \\ C_2\,O_2 \end{matrix} \right\} O_3$$

Succinic acid (bibasic). New acid (tribasic).

I propose to continue my researches on the cyanides of the di- and tri-atomic radicals.

II. "Notice of Remarkable Hailstones which fell at Headingley, near Leeds, on the 7th of May, 1862." By Thomas Sutcliffe, Esq., in a letter to Dr. Sharpey, Sec. R.S.

Headingley, July 16th, 1862.

Dear Sir,—Allow me to offer you some memoranda, which I made at Headingley, near Leeds, on the 7th of May last, respecting a hailstorm which visited several parts of England on that day. It appears that it arrived at Newark about 5 o'clock P.M., and was succeeded by a tornado which did much damage; then, pursuing a N.N.Westerly direction it reached Wakefield at 6.41. The hail continued to fall till about 6.58. The afternoon at Headingley had been remarkably hot and close, and the atmosphere densely charged with vapour; at 6.45 the sky had become so overspread with dark clouds that it was impossible to see anything within doors without artificial light. There were several peals of thunder and repeated flashes of rose-coloured lightning. The storm visited the villages on the west of Leeds with especial violence, the hailstones knocking down several people, and breaking nearly all glass exposed to the W.S.W.

The hailstones did not fall in a continuous shower, but in irregular clusters; sometimes a field would be thickly strewed with them, whilst an adjoining one escaped with scarcely any; one part of a greenhouse would be much broken, and the remainder, similarly exposed, escape uninjured. The district over which hail fell was very narrow.

To illustrate the force of the falling stones, I may mention that

circular holes were cut in glass without the sheet being otherwise injured. I have the end of a pendulous branch of beech, 12 inches long and $\frac{3}{8}$ths of an inch in circumference, which was cut from the tree, also several larger branches from apple and lilac trees, which appeared to have been split from the adjoining boughs. Some muslin curtains spread on the grass to dry were torn by the hail with numerous crucial rents.

The hailstones were of different forms and sizes. I sketched about forty varieties; but as many bear a certain resemblance to each other, I select four of them for illustration. These were taken out of deep grass nearly half an hour after they had fallen. Figures 1 to 4 represent them of the size and shape they had when I picked them up. The heaviest I weighed was only 2 ozs., but other persons assert that they weighed some upwards of 5 ozs. each. No. 1 had a creamy white colour, with linear markings from the centre outwards; this variety appeared to constitute the nucleus of most of the larger ones, around which transparent ice had accumulated in rounded continuous masses. From the outside of some of the masses protruded icicles; the remains of two may be seen attached to the side of No. 2. When the stones first fell, some of these icicles were $1\frac{1}{2}$ and 2 inches long, and grotesquely shaped. It has been asserted that all the hailstones had the white nucleus, but this was not the case in our neighbourhood; 35 per cent. of those I gathered were without it, and assumed something of the shape of No. 3, which seemed an aggregate of crystals of clear ice. I found one which was composed of five large masses of ice, quite clear, and in size like five nutmegs. There were some which did not correspond with any of the above descriptions; thus No. 4 has the round white radiated mass on the outside of the clear ice.

I annex meteorological reports for the day of the storm.

No. 1. From the 'Times' of May 8th.

May 7th, 1862, 8 o'clock A.M. Towns selected.

	B.	E.	M.	W.	F.	C.	I.	R.	S.
Aberdeen ...	29·93	50°	48°	N.N.E.	2	8	3 r.	0·15	1
Berwick ...	29·93	55	53	Calm.	0	24 f.	8 r.	0·43	2
Scarborough	29·89	57	55	N.E.	1	4	f.	—	2
Liverpool ...	29 89	55	54	E.	1	9	8 r.	0·43	1
Dover	29·83	62	60	S.E.	1	1	b.	—	1
Portland ...	29·85	55	54	E.S.E.	1	9	8 r.	0·35	2

Fig. 2.

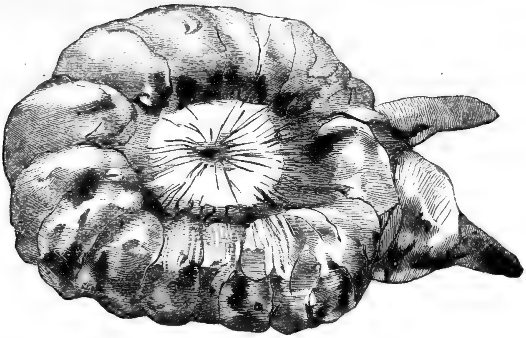


Fig. 3.

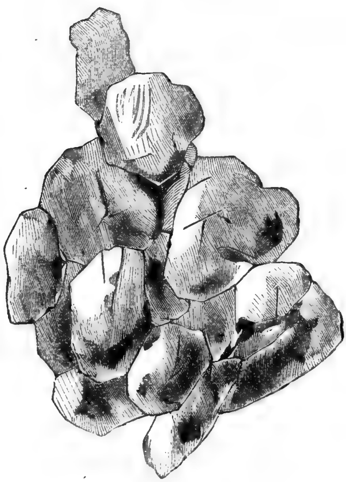


Fig. 1.

Fig. 4.

No. 2. Report kept by Leeds Philosophical Society.
Leeds, May 7, 1862, 5 P.M.

Barometer.	At. Therm.	Dry Bulb.	Wet Bulb.	Wind.	Force.	Cloud.	Shade.		Max. Sun.
							Max.	Min.	
29·380 in.	70°	64°	60°	N.E.	1	10	70	51	100

I am, &c.,
THOMAS SUTCLIFFE.

III. "On the true Theory of Pressure as applied to Elastic Fluids." By R. MOON, M.A., late Fellow of Queen's College, Cambridge. Communicated by Professor SYLVESTER. Received June 26, 1862.

(Abstract.)

It is the author's object—

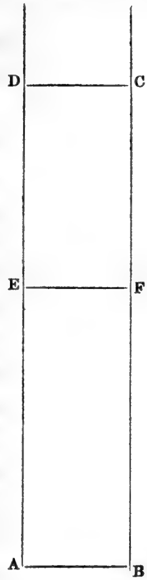
I. To show that, in elastic fluids in motion, or tending to move, it is not generally true, or at least not accurately true, that the pressure depends solely on the density, as is assumed in the ordinary theory of the motion of elastic fluids.

II. To show that, within certain limits and under certain circumstances, pressure may be transmitted instantaneously from one point of an elastic fluid to other points situated at finite distances from the first, before any change has been effected in the density of the intermediate fluid—in a manner analogous to that in which, in the theory of dynamics as applied to rigid bodies, force is assumed to be propagated instantaneously from one point to another.

III. To show that in elastic fluids in motion, or tending to move, the pressure at any point in a given direction will consist of two parts:—one depending solely on the density, which will be equal in all directions; the other depending on the state of motion throughout the fluid generally, and which will vary with the direction in which the pressure is estimated. The former of these two constituents the author proposes to designate the statical pressure; the latter, the instantaneous pressure. The true pressure at any point in a given direction will be found by taking the sum or difference of the statical and instantaneous pressures, according to circumstances.

IV. To indicate the manner in which the instantaneous pressure may be represented mathematically.

V. To show the bearing of the proposed correction on the received theory of sound.



A B C D is a vertical cylinder closed at the base A B, and having an air-tight piston C D capable of moving freely in the upper part of it.

Below the piston the tube is filled with air, which at the time t is wholly free from impressed velocity, but in which the density varies in the following manner: viz., from A B up to an imaginary horizontal plane E F, the density is uniform; while from E F the density gradually increases up to C D, in such a manner that the effective force at every point of the air between E F and C D is exactly the same, and equal to f*. Above the piston a vacuum exists. The piston is supposed to have weight, but, for the sake of simplicity, the air under the piston is supposed to be unaffected by gravity. The weight of the piston is supposed to be such that the effective force on each particle of the piston is the same as that on each particle of the mass of fluid E C, viz. f.

If the pressure exerted by the air which originally occupied the space A F on that which originally occupied the space E C were to continue during the time t_1 the same that it was at the time t, every particle of the former mass of air (which we will designate as the air in A F) would during the time t_1 be under the action of the same effective force f, and would therefore in that time describe the same length of path, viz. $\frac{ft_1^2}{2}$; and on this supposition no change would take place in the density of the air in E C during the time t_1. But, according to the received theory, the pressure of the air in A F on that in E C will continue unchanged until the density of the part of the air in A B which abuts on the common boundary of the two

* This will be the case if $\frac{1}{\rho}\frac{dp}{dx}=f$, or putting $p=a^2\rho$, $a^2 \log_2\rho=fx+c$; where ρ denotes the density at the distance x measured vertically, and c is a const.

masses of fluid has changed. Hence *change in the density of the air in A F must precede change in the density of E C.*

On the other hand, so long as the pressure of the air in E C on the air in A F remains unchanged, the air in A F will remain at rest, and will therefore undergo no change of density. But as, according to the received theory, the pressure of the air in E C on the air in A F depends on the density of the part of the air in E C which abuts on the common boundary of the two masses of air, it follows that *change in the density of the air in E C must precede change in the density of the air in A F.*

But we have before proved the exact contrary, viz. that change in the density of the air in A F must precede change in the density of the air in E C. It is evident therefore that, according to the received theory, no change can, under the circumstances above supposed, take place in the density of either mass of air.

If, however, the density in A F remain unchanged, we have already seen that every particle in E C will in the time t_1 describe a space equal to $\frac{ft_1^2}{2}$; and if the density in E C remain unchanged, we have equally seen that every particle of A F will have remained at rest during t_1; which is a contradiction. It appears therefore that in the case we have been considering the received theory leads us to an absurd result.

It can with still more facility be shown that the received theory leads to an absurd result in the following case.

A B C D is such a tube as before described; but in the present case we shall suppose it filled below the piston with air of uniform density in equilibrium, the pressure of the air being such as to exactly sustain the weight W_1 of the piston. As before, a vacuum is supposed to exist above the piston, and the air is assumed to be unaffected by gravity.

If a second weight W_2 be placed upon the piston, we know that the equilibrium will be destroyed. But if it be true, as the received theory asserts, that the pressure of an elastic fluid depends solely on its density, the pressure of the air on the lower surface of the piston will be exactly the same after W_2 has

been introduced as it was before W_2 was introduced; and, since action and reaction are equal and opposite, whatever be the pressure of the air in the piston, the same will be the pressure of the piston on the air; so that the pressure downwards of the piston on the air beneath will be the same after W_2 was introduced as it was before; and the system therefore will continue in equilibrium after W_2 has been introduced; which is absurd.

By an argument too elaborate to be indicated within the limits of this abstract, the cause of the failure of the existing theory in the instance first above considered is shown; and it is proved that in the second case the effect of the introduction of the weight W_2 is instantaneously to propagate through the air to a definite distance below the piston a finite increase of pressure; such increase of pressure having its maximum immediately underneath the piston, and thence gradually diminishing till, if the tube be long enough, it finally vanishes. The depth to which the instantaneous increase of pressure will extend will be defined by means of two considerations:—1st, that the effective force on every particle of the piston and weight must be exactly the same as that on the air immediately below it; and 2nd, that the aggregate moving force developed in the piston W, the weight W_2, *and the portion of the air in the tube through which the instantaneous pressure extends,* must be equal to the moving force developed by gravity in W_2 when free to move *in vacuo.*

It is also shown that if instead of the weight on the piston being suddenly increased it were to be suddenly diminished, exactly analogous results, *muttatis mutandis,* would occur,—the effect of the sudden removal of part of the weight being instantaneously to *diminish* the pressure to a finite distance below the piston—such diminution having its maximum immediately beneath the piston, and thence gradually diminishing till, at a certain distance below the piston, the whole pressure will be exactly the same as it was before any part of the weight was removed.

If the piston were wholly removed, the pressure of the air originally in contact with it at the instant of removal would be zero.

It is then shown that the addition to or diminution from the weight on the piston in the case last considered will produce no immediate change in the horizontal pressure in the air below the piston.

It is next shown that in cases where there is no impressed velocity,

as in the case first considered in this paper, the instantaneous pressure p_i may be expressed in terms of its partial differential coefficients, and of the density at the point where the pressure is being considered.

It is also shown that, in the general case, where the whole or a portion of the fluid is endued with velocity, the instantaneous pressure may be ascertained by adding to the expression of the last paragraph a term involving the density and the partial differential coefficients of the velocity at the point where the pressure is being considered.

It is finally shown that, in the case of the transmission of a pulse through a cylindrical tube where the motions are small, the equation of motion will be of this form,

$$\frac{d^2y}{dt^2}=\frac{a^2d^2y}{dx^2}-\frac{b^2d^2y}{dxdt};$$

where x denotes the distance from the origin measured parallel to the axis of a given stratum in the state of rest, y the same distance at the time t, and a^2 and b^2 are constants, the value of a^2 being the same as in the ordinary theory.

As this equation leads to the conclusion that there are two velocities, it results that, except perhaps in very rare instances, in which a duplication has been observed in sounds heard at very great distances, the proposed correction of the theory of the motion of elastic fluids will not practically affect the theory of sound.

By the method adopted in the case of elastic fluids, the author conceives himself to have established that, in what are commonly termed inelastic fluids, the pressure during motion will not be equal in all directions.

IV. "On the Nerves of the Liver, Biliary Ducts, and Gall-bladder." By Robert Lee, M.D., F.R.S. Received August 18, 1862.

(Abstract.)

After adverting to the deficiency of existing knowledge respecting the distribution and arrangement of the nerves of the liver, the author states that he has recently made dissections which "prove that all the arteries which ramify throughout the substance of the liver, even the most minute, are accompanied with nerves, on which

there are enlargements resembling ganglia, and that the hepatic ducts and gall-bladder are largely supplied with these gangliform plexuses of nerves, which all arise from the semilunar ganglion and solar plexus."

In a Postscript, received October 4, 1862, the author adds, that from an elaborate dissection which he has made since the date of the paper, "it is demonstrated—

"1. That the nerves of the liver take their origin from ganglia situated around the root of the hepatic artery, which are intimately connected with, or actually form a part of, the semilunar ganglion of the great sympathetic.

"2. That the hepatic nerves, thus originating, proceed to the liver along with the hepatic artery, hepatic veins, the vena portæ, and the hepatic ducts.

"3. That the hepatic nerves, on reaching the liver, send numerous branches to the different lobes, along with the ramifications of the hepatic artery to every part of the organ, and that plexuses of nerves accompany the most minute branches of the arteries.

"4. That the hepatic and cystic ducts are surrounded with plexuses of ganglia and nerves, and that nerves accompany the arteries of the gall-bladder throughout their distribution.

"5. That besides these nerves, accompanying the trunk and branches of the hepatic artery and surrounding the cystic and hepatic ducts, there is a great system of ganglionic nerves distributed to the walls of the vena portæ."

V. "On the Volumes of Pedal Surfaces." By T. A. Hirst, F.R.S. Received 28th August, 1862.

(Abstract.)

Since the term "pedal surface" has but recently been definitively adopted*, it may be well to state that it indicates, simply, the locus of the feet of perpendiculars let fall from a fixed point, the *pedal origin*, upon all the tangent planes of a given surface. It is sometimes convenient, too, to regard the pedal surface as the envelope of a sphere, whose diameter is the radius vector from the pedal origin

* A Treatise on the Analytic Geometry of Three Dimensions. By George Salmon, D.D. 1862.

to any point on the primitive surface. The primitive surface remaining unaltered, the form and magnitude of its pedal vary, of course, with the position of the pedal origin.

In the first part of the memoir, of which the present note is an abstract, the volumes of pedals derived from the same primitive surface, but corresponding to different origins, are investigated, and the general formula found by means of which the volume of any pedal whatever may be calculated when that of any other is known. From this formula are deduced the following new and very general properties of pedal surfaces :—

Whatever may be the nature of the primitive surface, the origins of pedals of the same volume lie on a surface of the third order.

It should be observed that the volume of the pedal is here understood to be that of the conical space swept by the perpendicular, as the tangent plane of the primitive takes all possible positions. In this sense the term volume may clearly be applied to the pedals of *unclosed* surfaces. It is in fact to such surfaces that the above theorem applies; for *when the primitive is a closed surface, but in other respects perfectly arbitrary, the locus of the origins of pedals of constant volume is a quadric, or surface of the second order. The whole series of quadric loci, corresponding to all possible volumes, constitutes a system of similar, similarly placed, and concentric quadrics, the common centre of all being the origin of the pedal of least volume.*

From the three equations which determine the position of the origin of the pedal of least volume, it follows that this origin always coincides with the centre of the primitive, whenever the latter possesses such a point; when, moreover, the primitive, besides being closed, is everywhere convex in curvature, and symmetrical with respect to three rectangular planes, each origin-locus is an ellipsoid whose principal diametral planes coincide with the planes of symmetry.

This is the case with the pedals of the ellipsoid, which, ever since the researches of Fresnel on light, have been regarded with especial interest. Their properties form the subject of the second part of the memoir.

It is shown that the volume of any ellipsoid-pedal, the coordinates of whose origin are given, may be found by simple differentiation of

the expression for the volume of the least or central pedal. Amongst the new properties of such pedals the following may be here cited:—

The volume of the pedal whose origin is at a corner of the rectangular parallelopiped described about the primitive ellipsoid is equal to four times the volume of the central pedal, and to twice the volume of the pedal at any one of the eight points where the ellipsoid is pierced by the diagonals of the parallelopiped.

Again, *the algebraical sum of the volumes of the three ellipsoid-pedals whose origins are at the extremities of any three conjugate diameters of a concentric and co-axal quadric is constant, and equal to three times the volume of the pedal at any one of the eight points where this quadric is pierced by the diagonals of its circumscribed rectangular parallelopiped.*

From this theorem several others are deduced by assuming, for the quadric in question, particular forms. For instance, when it coincides with the primitive surface itself, we learn that *the sum of the volumes of the three ellipsoid-pedals whose origins are at the extremities of any three conjugate diameters of the primitive surface is constant, and equal to six times the volume of the central or least pedal.*

In this theorem is included, of course, the special case where the origins of the three pedals coincide with the vertices of the primitive ellipsoid.

If, for convenience of enunciation, we define the *pedal-altitude* at any point to be the altitude of a parallelopiped whose base is the square on the line joining that point to the centre of the ellipsoid, and whose volume is equal to that of the pedal having the point in question for origin, it is found that *the algebraical sum of the three pedal-altitudes at the extremities of any three orthogonal diameters of a quadric, concentric and co-axal with the primitive ellipsoid, is constant, and equal to three times the pedal-altitude at any one of the eight points on this quadric which are equidistant from its axes.* It follows, consequently, that *this sum is not only invariable for one and the same quadric, but for all concentric and co-axal quadrics which pass through one and the same point equidistant from the principal diametral planes of the primitive ellipsoid.*

In the third part of the memoir, the volume of any pedal of the ellipsoid

$$\frac{x^2}{a_1}+\frac{y^2}{a_2}+\frac{z^2}{a_3}=1$$

is expressed by means of the three first partial differential coefficients of the symmetrical integral

$$V=\int_0^\infty \frac{dv}{\sqrt{(v+a_1)(v+a_2)(v+a_3)}}.$$

If P denote the volume of the pedal whose origin has the coordinates x, y, z, the expression in question is

$$P=-\frac{\pi}{2}\left[a_1 M_1 \frac{dV}{da_1}+a_2 M_2 \frac{dV}{da_2}+a_3 M_3 \frac{dV}{da_3}\right],$$

where

$$3M_1=(a_2+a_3)(3r^2+a)+3(a_2y^2+a_3z^2)+a_2^2+a_3^2,$$
$$3M_2=(a_3+a_1)(3r^2+a)+3(a_3z^2+a_1x^2)+a_3^2+a_1^2,$$
$$3M_3=(a_1+a_2)(3r^2+a)+3(a_1x^2+a_2y^2)+a_1^2+a_2^2;$$

r^2 and a being abbreviations for $x^2+y^2+z^2$ and $a_1+a_2+a_3$, respectively.

The memoir concludes with the expression of the volume P by means of ordinary elliptic functions, and the consideration of the special cases when the primitive is an ellipsoid of rotation. The expression in question may be readily obtained on observing that the integral V is reducible to the form

$$V=2\frac{F(\theta,k)}{\sqrt{a_1-a_3}},$$

where the amplitude θ and modulus k of the elliptic function F of the first kind are determined by the relations

$$\cos^2\theta=\frac{a_3}{a_1},\ k^2=\frac{a_1-a_2}{a_1-a_3}.$$

By the introduction of elliptic functions, however, the great advantages of symmetry are necessarily lost; and in investigating the properties of pedal-volumes, the above symmetrical expressions will in general be preferred. An opportunity thus presents itself, however, of verifying an expression for the volume of the central pedal, the only one hitherto calculated, which was first given in 1844 by Prof. Tortolini in vol. xxxi. of Crelle's Journal.

VI. "On the Causes of various Phenomena of Attraction and Adhesion, as exhibited in Solid Bodies, Films, Vesicles, Liquid Globules, and Blood-Corpuscles." By RICHARD NORRIS, Esq., Birmingham. Communicated by Dr. SHARPEY, Sec. R.S. Received August 28, 1862.

It has long been observed that solid bodies floating on liquids possess the property of modifying the figure of the surface of the liquid; thus pieces of tinfoil or greased bodies depress the liquid around them, while many others by the exercise of an attraction for its particles elevate it, giving rise to small mounds of liquid bounded by concave lines. It has also been observed that likes attract likes and are repelled by unlikes, *i. e.* bodies having like or unlike powers of altering the figure of the surface. These phenomena are generally admitted to depend for their existence on the combined forces concerned in capillary attraction. The following experiments are arranged to show that these effects of attraction are not peculiar to floating bodies or to bodies partially immersed, and that the only requirement is that liquid should be associated with the bodies in which the movement occurs.

Exp. 1. Let two balls of sealing-wax, or other material of greater specific gravity than water, be suspended by hairs in such a manner that they will both be partially immersed in water to an equal extent, the points of suspension being at a little distance apart, and the suspending hairs consequently parallel. When brought within the proper range, they will attract each other in the same manner as the floating bodies. In doing so they necessarily describe a small arc of a circle, of which the suspending hair is the radius, and have therefore not simply moved towards each other in a horizontal line, but have been raised to a higher level.

Exp. 2. Let two small sheets of microscopic glass be applied to each other by their lower edges so as to form an acute angle like the letter V, and let them be supported in this position by pins. On placing a drop of water in the angle, the plates will be drawn together and cohere by their surfaces.

Exp. 3. Suspend moveably, by means of a thread passing over a pulley and a small counterbalancing weight, a horizontal cork disk,

from the under surface of which a drop of water is hanging. On a support beneath, formed by three upright pins, place a small piece of paper or thin glass, on the surface of which there is also a drop of water. On depressing the disk till the two drops of water touch each other, the paper or plate will be instantly drawn up to it; or if the plate at the bottom be heavier than the disk, the latter will be drawn down.

Exp. 4. If a film of wetted collodion be partially stripped from a glass plate, on being loosed it immediately flies back to its original contact. The same effect may be observed if thin paper be wetted and spread on a smooth sheet of glass, or be laid on the surface of water.

Exp. 5. Take two wine-glasses and dip their mouths into a strong solution of albumen; by a little dexterity two delicate convex films will be obtained. On applying the most elevated points of the convexities to each other, the films will be attracted and reduced to plane surfaces strongly adherent to each other. The permanency of the films enables the experiment to be repeated several times in succession.

Exp. 6. When a soap-bubble is allowed to fall on an irregular surface, such as a piece of lint or flannel, it maintains its spherical shape; but if a smooth surface, such as a sheet of glass, be brought into slight contact with it, the wall of the bubble will be immediately attracted and flattened out upon it. In like manner, when two bubbles come into contact by their convex surfaces and cohere, the cohering surfaces become flattened; and bubbles in a group cohere by plane surfaces.

Among other natural bodies, blood-corpuscles present certain peculiarities of arrangement when withdrawn from their proper channels; thus when a minute drop of mammalian blood is placed upon a glass plate, the disks arrange themselves into rouleaux of well-known form. They become attached to each other in this case by their biconcave surfaces. They may also cohere by the edge or circumference, and give rise to a tessellated appearance. This latter arrangement is most easily obtained by placing a minute drop of blood on the under surface of a thin piece of microscopic glass so as to be viewed through it; the blood being in a dependent position. A third mode of union occurs when gum, gelatine, mucilage of linseed, or very thick solution

of starch is added to blood. The corpuscles cohere more closely and tenaciously than in either of the other conditions. They may be said to *blend* with each other, inasmuch as they form homogeneous masses. If the solution of gum or gelatine be added to the blood subsequently to the occurrence of the modes of arrangement previously described, in such a manner as not to disturb them, the already adherent corpuscles will cohere more closely, and the outlines of the corpuscles will be rendered less apparent, till at length homogeneity of appearance results. The same effect takes place if a drop of blood be allowed spontaneously to thicken, but rarely to such an extent as on the addition of gum, gelatine, &c.

This last mode of arrangement includes both the former conditions, inasmuch as the corpuscles cohere firmly not only by their biconcave surfaces but by their edges also.

The first mode, or the formation of rouleaux, may be closely imitated by preparing a number of small disks of cork so poised as to float in the vertical position; however carelessly these disks may be thrown into water, they will be found in a few moments to have arranged themselves into rouleaux after the manner of the blood-disks.

If a collection of blood-corpuscles cohering in the second mode be compared with the manner in which bubbles group themselves, as already described, the similarity will be allowed to be very striking.

From the marked analogy existing in their modes of arrangement, a suspicion naturally arises that the blood-globules are influenced by the same laws as the bubbles and cork-disks. A more critical investigation, however, proves that the phenomena, although allied, possess well-marked distinctions. The capillary action leading to adhesion, as exhibited in the cork-disks and bubbles, is not possible if these bodies are completely submerged in liquid; but experiments carefully performed with the blood-globules demonstrate that both the formation of rouleaux and the peripheral adhesion may take place under circumstances in which it is absolutely certain that each individual corpuscle is completely submerged. Thus if we take two pieces of microscopic glass, and, placing them in contact, press them firmly together and maintain them in opposition by a strong pair of pincers, we shall still have a capillary space between them which will draw in

a thin layer of blood, the corpuscles of which will form themselves into the most perfect and beautiful rouleaux.

The plasticity of the blood-corpuscle is unrivalled by any other physical body. It will assume all sorts of protean shapes under the slightest influences. Elongating to a mere thread, it will pass through a narrow chink; it will wrap itself round an acute projecting angle, or protrude feelers and tails under the influence of currents. In its natural state it possesses sufficient elasticity to resume its original shape on the cessation of modifying influences; but when gum or gelatine has been added, or when the plasma has been permitted to thicken spontaneously, the corpuscle retains any form it may have assumed, till again altered by some fresh influence.

The only artificial body capable of simulating the visible modifications of the blood-corpuscle is an extremely delicate, moderately filled vesicle.

The variations in its behaviour appear to be due to the degree of distention or flaccidity, which are doubtless under the influence of diosmosis.

When the corpuscles are apparently fused together, as after the addition of gum, the mass runs about like a thick liquid. Parts not previously in contact coalesce intimately with each other. Under the influence of currents, these masses stretch at certain points into fine threads, consisting of a single file of corpuscles, each corpuscle being elongated to its utmost, and finally sever at the natural junction of a single corpuscle with its fellow, the two stretched portions receding again into their respective masses.

Familiarity with the various conditions which the blood-corpuscles assume, and the analogous effects which take place in globules of homogeneous liquids, leads irresistibly to the conclusion that, on account of their great plasticity and the extreme tenuity and pliancy of their enclosing membrane, the blood-corpuscles are, under certain circumstances, subject to the law of cohesive attraction, in the same manner as these globules; and that, as with the latter so with the blood-corpuscles, changes in the character of the surrounding liquid determine the facility with which this law may come into operation.

If, as is well known, we place on a non-metallic surface small portions of liquid mercury, they will retain their spheroidal shape; and if any two of them be made to touch, they will be attracted to

each other, and one larger globule will result. If for the mercury we substitute water, using paper or metal as a support, we may get partial spheroids, which, being increased by repeated small additions till they touch each other, immediately coalesce, forming a semi-ovoid mass, instead of a sphere as in the case of the mercury. Mercury containing other metals in solution acts like the water.

Again, if a small quantity of chloroform or bisulphuret of carbon be poured into water, the greater portion will sink to the bottom of the water in globules of various sizes. The portion which floats may also be driven below the surface by striking it from above. These globules, when in contact, act precisely as the mercurial globules, *i. e.* blend with each other. They frequently adhere to the bottom of the vessel; and on an attempt being made to move them, tail-like appendages are produced. Creosote, castor oil, and the ethereal oil of male fern, all give permanent globular masses when forced below the surface of water. By agitation of the water, the latter globules may be elongated into threads from half an inch to an inch in length, and again resume their spheroidal shape. If in the process of elongation the thread be broken through, two spheroids result. If, instead of water, we use in these experiments a solution of soap, whatever form is given by agitation to the masses of oil is retained, and they possess no power whatever to blend with each other—the cohesive power is completely restrained.

If a portion of the same oil be shaken with water, we get a number of minute globules; and by placing them in a cell under the inch power of the microscope, we observe they possess little tendency to coalesce after the type of the chloroform; but if a portion of gum solution be poured into the cell, the process of incorporation commences immediately, and proceeds with rapidity. When these globules are formed in thick syrup, they exhibit a very great tendency to combine; but if to the syrup a little thick gum be added, this action is wholly prevented; neither do they adhere to each other when in contact.

If we mix three parts of a solution of 5 grains of gelatine in 1 drachm of water with 1 part oil of male fern, forcibly shaking them together in a test-tube, and draw a little of the mixture between two glasses in contact, we obtain numbers of globules about the size of the blood-corpuscles, and many much smaller. These globules

will be seen to possess the power of adhering to each other in groups and rows; and on a closer examination the cohering parts will be observed to have undergone a mutual flattening, just as in the case of the corpuscles and bubbles before mentioned.

We find then that the blood-corpuscles, while beneath the surface of the serum, adhere to each other, sometimes by their biconcave surfaces, so as to form rouleaux, sometimes by their peripheries, and sometimes in both ways simultaneously. They adhere also to foreign substances with which they come in contact, and on which they rest; and then currents in the liquor sanguinis give rise to tail-like processes. An adherent mass of corpuscles is capable of being elongated, and frequently gives way in the centre, when the two parts recede into their respective masses.

These effects occur contemporaneously with changes in the liquor sanguinis. In the normal state of this liquid, the corpuscles have no tendency to cohere; but the slightest modification of it, even while within the vessels, confers cohesive power on the white corpuscle; and the further alteration which occurs in blood taken from the body disposes the red disks to arrange themselves in rouleaux. When the liquor sanguinis is further altered by the addition of colloid substances, or allowed to modify itself spontaneously, the corpuscles become less elastic, and evince a great tendency to float with their surface upwards, and hence to cohere by their edges.

The attraction being proportionate to the amount of surface in contact, when the disks are free to move in the vertical position, and the tendency to cohesion is but moderate, they arrange themselves by their plane surfaces as the cork-disks do, not because the edges of the disks have no attraction for each other, but the planes offering a larger surface of attraction, this position is not so easily disturbed by currents in the serum. That this is the fact may be learned from the circumstance that when the plane surface of a cork- or blood-disk comes in contact with the side of a rouleau, it becomes as firmly fixed as if applied to the plane of its fellow. Every specimen of blood offers numerous instances of this kind.

These then being some of the peculiarities of blood-corpuscles, we learn, on the other hand, that globules of homogeneous liquids attract and become incorporated with each other when submerged in other liquids, being, like the blood-corpuscles, influenced in this

respect by variations in the surrounding medium. They may also, by disturbances of this medium, become elongated; and if the elongated mass severs, each portion falls back into itself and becomes an independent globule. They also become adherent to the solids which they touch, and exhibit tail-like processes. It will be universally admitted that these latter phenomena depend on cohesive attraction. Compared with the blood-corpuscles, these bodies are rigid and unyielding; how much more readily may we therefore ascribe the like effects observed in the former to the same cause.

It has been urged that nothing approaching to the character of an attractive influence has ever been observed with the blood-disks; but it must be remembered that the attraction of cohesion could not be indicated by motion between corpuscles at perceptible distances, but could only take place when the particles of the bodies were so closely applied to each other as to be within the radius of the sphere of molecular influence; and it is only under certain special circumstances that such an attraction could be even inferentially visible.

I have repeatedly observed such an attraction exercised among corpuscles under the only circumstance in which the observation is possible. After the addition of gum or gelatine to the blood, and the cessation of the consequent disturbance, there will still be many individual corpuscles and little circular masses floating sluggishly and unattached in the serum. After a time some of these will come in contact with each other at one point in their circumference; and if the disturbance in the liquor sanguinis is very slight, they will cohere at this point, and then will be seen to become gradually applied to each other for half their circumference. This is an action which can readily be understood as the successive operation of molecular attraction on the particles of the corpuscles immediately contiguous to those in absolute contact, but can receive no explanation on the hypothesis of adhesiveness. The best mode of observing this important phenomenon is to draw the mixture of gum and blood between two glasses in contact.

VII. "On Stasis of the Blood, and Exudation." By Richard Norris, Esq. Communicated by Dr. Sharpey, Sec. R.S. Received August 28, 1862.

Confusion has been introduced into the question of stasis, as related to inflammation, by neglecting to discriminate between the various forms of stasis, of which there are four.

1. If the frog's web be exposed to certain irritants (*e.g.* chloroform), the arteries are so constricted that the heart-force is temporarily shut off from the capillaries, which become packed by the reflux of blood from the veins. This form of stasis is dissipated immediately on the cessation of the arterial constriction. The blueness of the extremities consequent upon exposure to cold is probably dependent on the same mechanism.

2. The second form of stasis depends upon such enfeeblement of the heart's force as interferes with the due propulsion of blood into the extreme vessels. It also disappears upon the re-establishment of a sufficient propulsive power.

3. The third form is that described by H. Weber as follows:—"If a limb [of a frog] be strangulated, there arises in its web within four to eight hours, without any irritation being applied, a stasis which is identical with inflammatory stasis, except that after sixty hours' duration it will be dissipated as soon as the circulation is set free." The removal of this stasis by the re-establishment of the circulation distinguishes it from inflammatory stasis, and shows its relation to the forms already described.

4. The fourth form of stasis is producible (artificially) by the application of irritants, and has for its specific characteristics—

α. It is readily induced when the heart-force is unimpaired and the blood-channels are free.

β. It requires hours or even days for its dissipation, or it may even be irresolvable.

γ. It presents under the microscope a homogeneous appearance, as if the vessels had been injected with a tinted size or gelatine. The outlines of the corpuscles are undistinguishable. I call this "homogeneous" or "inflammatory" stasis.

It is unnecessary to offer additional evidence upon the first

characteristic, which is an accepted dogma. The following experiment refers to the mode of resolution.

Fig. 1.

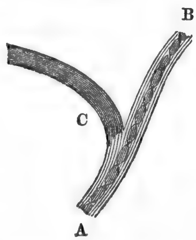
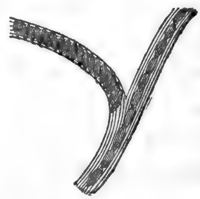


Fig. 2.



March 24th, 1861.—On examining the web of a frog which had been inflamed with tincture of iodine the day previously, I watched a vessel in which the homogeneous stasis existed, and observed the stasis to resolve in a peculiar manner. A normal current, such as is usually seen in capillaries, was circulating in the direction from A to B (fig. 1,) and impinging on the contents of the obstructed vessel C. The stagnation in the vessel C appeared to thaw as it were. The corpuscles were not pushed onwards in the mass, but seemed to take on the appearance of the impinging current, and the parts so reduced from the homogeneous to the heterogeneous condition did not appear to contain any great excess of corpuscles. This action soon extended through the whole length of the vessel, and immediately this was consummated a perfect current set in as in fig. 2. At the extreme edge of another division of this web I noticed two stagnated loops as in fig. 3. The circulation around them was in full activity. These I watched for more than an hour, and observed them to become gradually much lighter in colour, passing from a deep red to a pale orange. The point of junction C retained its depth of tint much the longest. All this time the contents of the vessels maintained perfectly their homogeneous character. At length all at once the outlines of the corpuscles became visible as in fig. 4, and the circulation was re-established. In this case, as in the previous one, there was no pushing on before of a plug of adherent corpuscles, but a gradual permeation of the liquor

Fig. 3.

Fig. 4.

Fig. 5.

sanguinis from the points A and B (fig. 3), with subsequent alteration of both the colour and disposition of the corpuscles. Fig. 5 also shows the progressive resolution of homogeneous stasis in a capillary vessel.

November 14th, 1861.—I took a vigorous frog, and having observed that the circulation was healthy, placed a ligature around a limb. On again observing I found the circulation arrested, but the corpuscles were very distinctly seen with $\frac{1}{4}$-inch power, and floated about with facility when the web was touched. I now applied to the web a small drop of chloroform, and, on again looking, found many of the capillaries had assumed the homogeneous condition. These observations strongly impressed me with the view that homogeneous stasis depends upon a new condition of the corpuscles, and not upon their crowding. The cause of homogeneous stasis appears to be the withdrawal of fluid from the capillaries, leading to a local modification of the liquor sanguinis, and consequently to the cohesion of the corpuscles with each other and the walls of the vessels, in obedience to the principles explained in my paper "On the Causes of various Phenomena of Attraction and Adhesion, as exhibited in Solid Bodies, Films, Vesicles, Liquid Globules, and Blood-corpuscles," in the present Number of the 'Proceedings.'

It is true that homogeneous stasis may occur in vessels previously packed with corpuscles, and we then find that the homogeneously solidified portion is of a much redder colour than when it occurs in a part previously free; in both cases irritation is essential to its production.

Again, corpuscular packing may arise as a secondary effect in consequence of the obstruction of the current by homogeneous stasis, the heart-force being unimpaired.

If the plug be pressed out of the cut extremity of a vessel obstructed by homogeneous stasis, we find the corpuscles adherent to each other; some have burst; we find also free nuclei.

In homogeneous stasis a certain quantity of colouring matter escapes from the corpuscles, and probably contributes to the appearance of homogeneity.

Stasis generally occurs first at the venous radicles, because here the *vis à tergo* is weaker.

Having attributed inflammatory stasis to a modification of the

liquor sanguinis, it is incumbent on me to show how and why this modification occurs.

Whilst it must be admitted that the cellular elements of the tissues have the power of imbibing and utilizing fluid plasma when it is brought into immediate apposition to them, there is not convincing evidence that these elements exercise any positive educing force upon this fluid while it remains within the vessels. Still less is there evidence that, of two cells situated the one nearer to, the other more remote from the vessel, the latter has any attractive power superior to that of the former, which, to secure its nutrition on the theory of positive attraction, it must have, for the plasma attracted by the nearer cell would remain in the possession of that cell till removed from it by a superior force.

The law of diosmosis suffices to explain the supply of fluid plasma to the cellular elements without recurrence to the hypothesis of a positive attractive force resident in the cells themselves. It is impossible to doubt that such structures as capillaries are diosmotic.

The more braced the condition of the minute vessels the less diosmosis, and *vice versâ*. It is not during contraction of the minute vessels produced by irritants that stasis occurs, but during the relaxation consequent on such contraction,—a relaxation which must be attributed to exhaustion of their irritability by the stimulus applied. This relaxation permits the diosmotic escape of fluid from the vessels, causing an inspissation of the plasma within them, and consequent adhesion of the corpuscles constituting inflammatory stasis. This escape of fluid may be termed primary exudation.

The muscular paralysis in question is not necessarily connected with neural paralysis, since it is producible in parts which, though abounding in contractile elements, are without nervous tissue, as, for example, in the umbilical cord. In fact the more completely the nervous influence is removed and destroyed the more sensitive does the muscular tissue become to irritants.

Neural paralysis does undoubtedly play a part in inflammation. Whilst the nerve-influence is exercised over a part, it affords a protective influence which renders the contractile elements less sensitive to local irritants, and consequently less prone to that absolute muscular paralysis which precedes primary exudation. But the neural paralysis does not necessarily involve absolute muscular paralysis,

although it facilitates its production. In corroboration of these views I may refer to the well-known results following the section of various nerves, *e. g.* the pneumogastric and the fifth.

This muscular paralysis is probably producible directly by muscle-sedatives, as it is indirectly by muscle-irritants. Since diminished nerve-force produces hyperæmia, and since diminished nerve-force furnishes the conditions under which homogeneous, *i. e.* inflammatory, stasis is most prone to occur, we see why hyperæmia and inflammation are so frequently conjoined. The experiments of Claude Bernard on the sympathetic, while showing the connexion between neural paralysis and hyperæmia, also indicate that neither neural paralysis nor hyperæmia are convertible terms with inflammation.

A distinction is drawn between that diosmotic exudation which leads to homogeneous stasis, and that subsequent copious transudation of fluid which fills up the interstices of tissues or leaks into cavities.

If in a frog's web homogeneous stasis has occurred in the venous radicles so as to completely prevent the passage of the blood into the veins, the current in the capillaries and supplying arteries might naturally be expected to be brought to a stand, as it certainly would be if the walls of the capillaries removed from the immediate seat of the obstruction were impervious; but so far from this being the case, the blood brought to the part is seen to pass on in a perfectly regular manner without the slightest rebound. This absence of rebound is an evidence that the liquor sanguinis is passing through the vascular parietes at the same rate it is being propelled into the obstructed vessels.

It is not till the capillaries become packed with corpuscles and the circulation is confined to the arterial trunk that any rebound after the ventricular contraction becomes apparent. This rebound is the cause of throbbing in inflamed parts.

The views here briefly given seem to me to form a consistent theory, supported by experiment as far as the subject admits of experiment, in accordance with the phenomena of inflammation as observed in the warm-blooded animals.

VIII. "Additional Observations on the Proximate Principles of the Lichens." By JOHN STENHOUSE, LL.D., F.R.S. Received October 3, 1862.

The Lichens on which I have recently been experimenting are two in number, namely, the South American variety of *Roccella tinctoria*, which is imported in considerable quantities from the neighbourhood of Lima and Valparaiso, and is known in commerce as "Lima weed;" and the *Roccella tinctoria* var. *fuciformis*, the same which I had formerly designated *Roccella Montagnei*; it is the "Angola weed" of commerce.

Soon after the publication in 1848 of my first paper on this subject, Dr. Schunck* threw out the hypothesis that the various compounds produced by boiling lecanoric, erythric, alpha- and beta-orsellic acids with alcohol were all one and the same ether—the pseudo-erythrin of Heeren. No further light was thrown upon this obscure subject till the publication of Hesse's able paper in the March Number of Liebig's 'Annalen' for 1861. The Archil-lichen which Professor Hesse investigated was that from Angola. He extracted its colour-yielding principle—to which, from its feeble acid properties, he restores the name of erythrin originally given it by Heeren—by treating it with milk of lime, and precipitating either with carbonic or hydrochloric acid. On drying and boiling the erythrin with strong alcohol, he produced the ether which he terms orsellinic ether, the composition and properties of which he found to correspond precisely with those which Schunck and I had previously ascertained. Hesse's formula for this ether is

$$C_{16}\dagger H_7 (C_4 H_5) O_8.$$

By treating it with chlorine and bromine, he succeeded in replacing two equivalents of hydrogen by these elements, producing what he termed the bichloro- and bibromo-orsellinic ethers. I have repeated Hesse's experiments, so far as the preparation of the bibrominated ether is concerned, and find, as will be seen by the subjoined analyses, that his statements are perfectly correct.

I. 0·7960 grm. substance, dried *in vacuo* over sulphuric acid, gave in the usual manner 0·8390 grm. Ag Br.

* Philosophical Magazine, October 1848. † C=6 &c. *et seq.*

II. 0·6735 grm. gave 0·8400 grm. CO_2 and 0·1740 grm. Aq.

The following is a comparison of the theoretical and experimental percentages :—

Theory.			Experiment. I.	Experiment. II.
C_{20}	=120	33·89	——	34·00
H_{10}	= 10	2·83	——	2·87
Br_2	=160	45·19	44·79	——
O_8	= 64	18·09	——	——
	354	100·00		

I likewise prepared a quantity of the corresponding methylic ether, by boiling erythrin in wood-spirit. When an alcoholic solution of this ether was treated with excess of bromine, the bibrominated orsellinate of methyl was produced. It crystallized in colourless flat needles, and furnished on analysis the following results :—

Dried *in vacuo* over sulphuric acid,

I. 0·3135 grm. substance gave 0·3463 Ag Br.

II. 0·3555 grm. gave 0·4155 CO_2 and 0·0850 Aq.

These results correspond with the formula

$$C_{16}\, H_5\, Br_2\, (C_2\, H_3)\, O_8,$$

as is manifest from the following comparison :—

Theory.			Experiment. I.	Experiment. II.
C_{18}	=108	31·77	——	31·87
H_8	= 8	2·35	——	2·65
Br_2	=160	47·06	47·00	——
O_8	= 64	18·82	——	——
	340	100·00		

I likewise prepared some alpha-orsellic acid from the South American variety of the *Roccella tinctoria*. On treating the ethyl-compound obtained from this acid with bromine, a body was formed which in properties and composition was exactly the same with the bibrominated ethyl-compound obtained from erythrin by Hesse, as may be seen by the following analytical details :—

I. 1·1890 grm. substance gave 1·2658 grm. Ag Br.

II. 1·0415 grm. gave 1·2970 grm. CO_2 and 0·2860 grm. Aq. Or, expressed in percentages,

	Found. I.	Found. II.	Theory.
C ..	——	33·96	33·89
H ..	——	3·05	2·83
Br..	45·30	——	45·19

The circumstance that the ethers, whether obtained from erythrin or from alpha-orsellic acid, have not only the same properties and percentage composition, but likewise the same atomic weights, affords a very strong confirmation of the correctness of Dr. Schunck's hypothesis.

Orsellinate of Amyl.

On boiling dried erythrin with amylic alcohol for several hours, and removing the excess of alcohol by distillation, the residue yielded on standing a crystalline mass, which, however, I was unable to free entirely from resinous matter. Consequently it could not be analysed. It formed platy crystals of considerable lustre. There is every reason to believe, therefore, that it was the amyl-compound in question. The attempt to form a bromine-derivative, which it was expected might be more easily purified, did not furnish satisfactory results, though such a body was apparently produced.

Formation of Erythroglucine.

I have already shown * that when the ethyl-compound of erythrin, then known as erythric ether, is boiled with potash or baryta, alcoholic vapours are given off, while orcin and erythroglucine remain in the solution. I have recently found that though alpha-orsellic acid, as I had already established, when boiled with alkalies yields orcin but no erythroglucine whatever, yet when the ethyl-compound obtained from it is boiled with lime or any of the alkalies for five or six hours, erythroglucine as well as orcin is produced. In order to verify this very unexpected result, the erythroglucine, which was obtained with all its characteristic properties, was subjected to analysis.

Dried at 100° C,

0·5315 grm. substance gave 0·7670 grm. CO_2 and 0·4185 grm. Aq.

* Philosophical Transactions, 1848, p. 74.

The following is a comparison of theory and experiment:—

Erythro-glucine.	Theory.		Experiment.
$C_8 =$	48	39·35	39·35
$H_{10} =$	10	8·19	8·74
$O_8 =$	64	52·46	——
	122	100·00	

As therefore the ether produced from two different sources, namely, from erythrin and alpha-orsellic acid, when boiled with alkalies yields erythroglucine, I think there is every reason to expect that the ethers derived from lecanoric and other lichen acids, when similarly treated, will likewise yield orcin and erythroglucine, thus affording still further confirmation of the correctness of Dr. Schunck's hypothesis. It now naturally occurred to me to treat the methylic ether obtained from alpha-orsellic acid with lime, in order either to procure a homologue of erythroglucine, or to attain results which might throw some light on the constitution of erythroglucine itself. The result of this experiment was somewhat unexpected; for though several trials were made on a considerable scale, and the boiling with lime was continued, as before, till the ether was entirely decomposed, much orcin but no erythroglucine was produced. The same negative results were obtained when the methylic ether prepared from erythrin was subjected to a similar treatment. It appears therefore that ethyl is necessary to the formation of erythroglucine from alpha-orsellic acid, and that it cannot be replaced by methyl.

From the very great analogy which erythroglucine bears to ordinary mannite (being, in fact, the mannite of the lichen series), I was induced to submit it to the action of hydriodic acid, in precisely the same way adopted by Wanklyn and Erlenmeyer with mannite. A considerable quantity of erythroglucine was introduced into a retort and distilled with a great excess of strong hydriodic acid, while a current of carbonic acid gas traversed the hot liquid. A dark-coloured oil of an ethereal odour, and heavier than water, mingled with abundance of free iodine, passed over into the receiver, while a large quantity of a black humus-like body remained in the retort. The latter substance was evidently an organic compound containing much iodine; but as it was insoluble in all the usual solvents, such as

water, alcohol, and ether, it was impossible to purify it. The oil was agitated with metallic mercury, to remove free iodine, then washed with water, dried over chloride of calcium, and redistilled. The quantity obtained in this manner was extremely small. The specimen examined was almost colourless, and boiled at 90° C. The residue in the retort contained another oil, boiling, with rapid decomposition, at a much higher temperature. Analysis furnished the following results :—

0·3615 grm. substance gave 0·3630 CO_2 and 0·1870 Aq, corresponding to 27·39 Carbon and 5·75 Hydrogen per cent.

These numbers approximate to those which iodide of propyl or iodide of butyl should furnish; but owing to the very small quantity of substance at my disposal, I was unable to subject this curious oil to a thorough examination.

Tribrom-beta-orcin.

I have ascertained that when bromine is added to an aqueous solution of beta-orcin, a brominated derivative is formed, which crystallizes in needles. It is but slightly soluble in water, very soluble in alcohol and ether. It is perfectly similar in its properties to tribromorcin, derived from ordinary orcin, and most probably resembles it in constitution. Unfortunately the small amount of beta-orcin in my possession prevented me obtaining the tribrom-beta-orcin in sufficient abundance to submit it to analysis.

In the present state of our knowledge, the following equations will serve to throw light upon the constitution of a few of the more important lichen-derived compounds :—

(1) When alcohol is boiled with erythrin, the following reaction takes place :—

$$\underset{\text{Erythrin.}}{C_{56}H_{30}O_{28}} + 2C_4H_6O_2 = \underset{\text{Picroerythrin.}}{C_{24}H_{16}O_{14}} + \underset{\text{Orsellinic ether.}}{2C_{20}H_{12}O_8} + 2HO$$

(2) When erythrin is boiled with alkalies, the following transformations ensue :—

$$(a)\ \underset{\text{Erythrin.}}{C_{56}H_{30}O_{28}} + 2HO = \underset{\text{Picroerythrin.}}{C_{24}H_{16}O_{14}} + \underset{\text{Orsellinic acid.}}{2C_{16}H_8O_8}$$

The picroerythrin is then transformed into erythroglucine, orcin, and carbonic acid, and the orsellinic acid into carbonic acid and orcin :—

$$\left.\begin{array}{l}(\beta)\ \underset{\text{Picroerythrin.}}{C_{24}H_{16}O_{14}}+2HO=\underset{\text{Erythroglucine.}}{C_8H_{10}O_8}+\underset{\text{Orcin.}}{C_{14}H_8O_4}+2CO_2 \\ \qquad 2\underset{\text{Orsellinic acid.}}{C_{16}H_8O_8}=2\underset{\text{Orcin.}}{C_{14}H_8O_4}+4CO_2.\end{array}\right\}$$

These equations have been established either by myself or other observers.

IX. "On the Theory of Parallels." By Lieut.-General T. Perronet Thompson, F.R.S. Received August 4, 1862.

More than thirty published efforts, from Ptolemy downwards, attest the satisfaction with which the Theory of Parallels would be seen established without merging the difficulty into an axiom.

As many of these are certainly not elementary, it amounts to an admission that any knowledge on the subject would be acceptable, even though it left the necessity of beginning from the axiom with freshmen.

Believing that the generation of the straight line with the impossibility of two enclosing a space, and of the plane with the straight line joining any two points lying wholly in the surface, may be rigidly demonstrated from the property of the sphere, which Plato calls its "perfection," or the faculty of turning about its centre without change of place,—I am induced to submit whether some light may not be offered by the following :—

Place two equal circles in the same plane, and let a straight line rest upon them (spheres and a superincumbent plane might be taken, but it is conceived the other is easier). The centres remaining at rest, let the circles dilate as by inflation, preserving always the equality of the diameters to one another.

It would appear to be deducible from the Platonic property, that the motion of any point in the circumference during the inflation must be perpendicular to the circumference, and consequently at any instant to the straight line which touches the circumference. Also the touching point in that straight line is at any instant impelled in a direction perpendicular to the circumference and to the touching line; out of which it seems impossible that the points of contact in the circumference and in the touching line should ever separate; for that would imply a motion other than perpendicular in one or both.

If this was supposed allowed, it would follow from making the

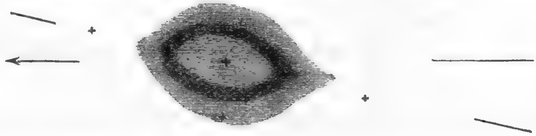
circles grow less till they vanish, that the distance of the points of contact in the incumbent line was always equal to the distance of the centres. We should therefore be presented with rectilinear quadrilateral plane figures, of which, from the equality of opposite sides, and the angles at the two extremities being right angles, all the angles must be right angles. From which it would be an easy step to the proof that the angles of every rectilinear triangle are together equal to two right angles; and so the Theory of Parallels be entered by another door, and the twelfth axiom be a deduction instead of a groundwork.

It would be interesting if the Theory of Parallels should be proved traceable to Plato's property of the sphere.

X. "Letter to Professor STOKES, Sec. R.S., containing Observations made at Malta on a Planetary Nebula." By WILLIAM LASSELL, Esq., F.R.S.

Malta, 26th Sept. 1862.

MY DEAR SIR,—In directing my large equatoreal upon the well-known planetary nebula situated in Æ 20^h 56^m N.P.D. $101° 56'$ (1862), it has revealed so marvellous a conformation of this object that I cannot forbear to send you a drawing of it, with some description of its appearance. With comparatively low powers, *e. g.* 231



and 285, it appears at first sight as a vividly light-blue elliptic nebula, with a slight prolongation of the nebula, or a very faint star, at or near the ends of the transverse axis. In this aspect the nebula resembles in form the planet Saturn when the ring is seen nearly edgewise. Attentively viewing it with higher powers, magnifying respectively 760, 1060, and 1480 times, and under the most favourable circumstances which have presented themselves, I have discovered within the nebula a brilliant elliptic ring, extremely well defined, and apparently having no connexion with the surrounding nebula, which indeed has the appearance of a gaseous or gauze-like envelope, scarcely interfering with the sharpness of the ring, and only diminishing somewhat its brightness. This nebulous envelope extends a little further from

the ends of the conjugate than from the ends of the transverse axis; indeed, it is but very faintly prolonged, and only just traceable towards the preceding and following stars. There is a star near its border, northwards, in the projection of the conjugate axis.

The breadth or thickness of the ring is, unlike that of Saturn, nearly uniform, or equal in every part, so that its form most probably is either really elliptic, and seen by us in a line nearly perpendicular to its plane; or, if really circular, and seen foreshortened, a section throughout any part of it, limited by the internal and external diameters, must be a circle. In other words, it will be like a circular cylinder bent round. It could scarcely fail to bring to my mind the annular nebula in Lyra, especially as there is a conspicuous central star (proportionally, however, much brighter than that which is in the centre of that nebula), and yet the resemblance is only rudely in form, for this ring is much more symmetrical and more sharply defined, suggesting the idea of a solid galaxy of brilliant stars.

The ring is not perfectly uniform in brightness, the south-preceding part being slightly the most vivid. The transverse axis is inclined to the parallel of declination about 13°. A series of micrometrical measures of the length and breadth of the ellipse gives a mean of $26''{\cdot}2$ for the transverse, and $16''{\cdot}6$ for the conjugate axis.

The accompanying drawing has not been at all corrected by these measures, but is the result of several sketches made during different observations, and is a faithful transcript of the appearance of the nebula to my eye, when most favourably seen.

The object is, as may be supposed, one of extreme difficulty, requiring in the highest degree the combination of light and definition in the telescope, and a favourable state of atmosphere,—which will further appear when I state that it was not until I was favoured with an unusually fine night, and had applied a power of 1480, that the whole of the details were brought out.

I confess I have been greatly impressed by the revelation of this most wonderful object, situated on what perhaps we may consider as the very confines of the accessible or recognizable part of the universe, affording ground for the inference that more gorgeous systems exist beyond our view than any we have become acquainted with.

I am, &c., WILLIAM LASSELL.

November 20, 1862.

Major-General SABINE, President, in the Chair.

In accordance with the Statutes, notice of the ensuing Anniversary Meeting for the election of Council and Officers was given from the Chair.

Mr. Gassiot, Dr. Hooker, Prof. Clerk Maxwell, Prof. Sylvester, and the Rev. R. Willis, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

Captain Charles Frederick Alexander Shadwell, R.N., and Mr. Balfour Stewart were admitted into the Society.

Pursuant to notice given at the last Meeting, The Right Honourable James, Earl of Caithness, was proposed for election and immediate ballot; and the ballot having been taken, His Lordship was declared duly elected.

The following communications were read:—

I. "On the Synthesis of Tribasic Acids." By MAXWELL SIMPSON, M.B., F.R.S. (See p. 236.)

II. "Notice of Remarkable Hailstones which fell at Headingley on the 7th of May, 1862." By THOMAS SUTCLIFFE, Esq. (See p. 239.)

III. "On the true Theory of Pressure as applied to Elastic Fluids." By R. MOON, M.A. (See p. 242.)

IV. "On the Nerves of the Liver, Biliary Ducts, and Gall-bladder." By ROBERT LEE, M.D., F.R.S. (See p. 246.)

V. "On the Volumes of Pedal Surfaces." By T. A. HIRST, F.R.S. (See p. 247.)

VI. "On the Causes of various Phenomena of Attraction and Adhesion, as exhibited in Solid Bodies, Films, Vesicles, Liquid Globules, and Blood-Corpuscles." By RICHARD NORRIS, Esq. (See p. 251.)

VII. "On Stasis of the Blood, and Exudation." By RICHARD NORRIS, Esq. (See p. 258.)

VIII. "On the Theory of Parallels." By Lieut.-General T. PERRONET THOMPSON, F.R.S. (See p. 268.)

IX. "On the Fossil Remains of a long-tailed Bird (*Archeopteryx macrurus*, Ow.) from the Lithographic Slate of Solenhofen." By Prof. RICHARD OWEN, F.R.S. Received November 6, 1862.

(Abstract.)

The author details the circumstances connected with the discovery of the fossil remains, with the impressions of feathers, in the Lithographic slates of Solenhofen, of the Oxfordian or Corallian stage of the Oolitic period, and of the acquisition for the British Museum of the specimen which forms the subject of his paper.

The exposed parts of the skeleton are,—the lower portion of the furculum; part of the left os innominatum; nineteen caudal vertebræ in a consecutive series; several ribs, or portions of ribs; the two scapulæ, humeri, and antibrachial bones; parts of the carpus and metacarpus, with two unguiculate phalanges, probably belonging to the right wing; both femora and tibiæ, and the bones of the right foot. Impressions of the quill-feathers radiating fan-wise from each carpus, and diverging in pairs from each side of the long and slender tail. The above parts indicate the size of the winged and feathered creature to have been about that of a rook. The several bones, with their impressions and those of the feathers, are described, and the bones are compared with their homologues in different Birds and in Pterodactyles. Whence it appears that, with the exception of the caudal region of the vertebral column, and apparently of a bi-unguiculate manus, with less confluent condition of the metacarpus, the preserved parts of the skeleton of the feathered animal accord with the ornithic modifications of the vertebrate skeleton. The main departure therefrom is in a part of that skeleton most subject to variety. Twenty caudal vertebræ extend from the sacrum in a consecutive and naturally articulated series, resembling in structure and proportions those of a squirrel. The tail-feathers are in pairs corresponding in number with the vertebræ, diverging therefrom at an angle of 45° backward, becoming more acute near the end, and the last pair extending nearly

parallel with and $3\frac{1}{2}$ inches beyond the last caudal vertebra. This feathered tail is 11 inches long and $3\frac{1}{2}$ inches broad, with an obtusely rounded end. This novel and unexpected character of the tail is owing to the constancy with which all known existing and tertiary birds have presented the short bony tail with the terminal modification in most of them of the ploughshare bone.

Professor Owen next gives the results of investigations into the osteogeny of embryo-birds, showing the number of vertebræ corresponding to the anterior caudals in *Archeopteryx* which coalesce with the pelvis in the course of growth, and the degree to which the posterior caudals retain a resemblance to those of *Archeopteryx* in the Birds with rudimental wings. From eighteen to twenty caudal vertebræ may be counted in the young Ostrich. In *Archeopteryx* the embryonal separation persists with such continued growth of the individual caudal vertebræ as is commonly seen in long-tailed Vertebrates, whether Reptilian or Mammalian. The author remarks that the modification and specialization of the terminal bones of the spinal column in modern birds is closely analogous to that which converts the long, slender, many-jointed tail of the modern embryo-fish into that short and deep symmetrical shape, with coalescence of terminal vertebræ into a compressed lamelliform bone, like the 'os en charrue' of birds, to which the term 'homocercal' applies; such extreme development and transformation usually passing through the heterocercal stage, at which, in palæozoic and many mesozoic fishes, it was arrested. Thus he discerns in the main differential character of the mesozoic bird a retention of structure which is embryonal and transitory in the modern representatives of the class, and consequently a closer adhesion to the general vertebrate type.

The least equivocal parts of the present fossil declare it to be a Bird, with rare peculiarities indicative of a distinct order in that class. Although the head is absent, the author predicts, by the law of correlation, a beak-shaped mouth for the preening of the plumage; and he also infers a broad and keeled sternum in correlation with the remains of feathered organs of flight.

The paper is accompanied by drawings of the fossil and its parts, and of homologous parts in Birds and Pterodactyles. The author assigns to the fossil animal the name of *Archeopteryx macrurus*.

November 27, 1862.

Major-General SABINE, President, in the Chair.

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Council and Officers recommended for election was read as follows:—

President.—Major-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L.

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—Benjamin Guy Babington, M.D.; George Bowdler Buckton, Esq.; William Benjamin Carpenter, M.D.; Warren De la Rue, Esq., Ph.D.; Sir Philip de M. Grey Egerton, Bart.; Captain Douglas Galton, R.E.; Robert Godwin-Austen, Esq.; Joseph Henry Green, D.C.L.; Joseph Dalton Hooker, M.D.; John Lubbock, Esq.; Professor James Clerk Maxwell, M.A.; Professor Richard Owen, D.C.L., LL.D.; Professor Henry J. Stephen Smith, M.A.; Professor James Joseph Sylvester, M.A.; Professor Charles Wheatstone, D.C.L.; Rev. Robert Willis, M.A.

Mr. George Bentham and Captain Alexander Ross Clarke, R.E., were admitted into the Society.

The following communications were read:—

I. "Dynamical Problems regarding Elastic Spheroidal Shells and Spheroids of Incompressible Liquid." By Professor WILLIAM THOMSON, F.R.S. Received August 22, 1862.

(Abstract.)

In this paper the deformation of a homogeneous elastic solid bounded by two surfaces which are concentric spherical surfaces when the solid is free from strain, is investigated under each of the two following conditions:—

I. The displacement of every point of each surface is given.

II. The force per unit of area is given in magnitude and direction at each point of each surface.

The formulæ are applied to determine the deformation produced in the earth by the tide-generating forces of the moon and sun, on certain definite hypotheses as to the rigidity of the earth. Thus the theoretical results used in a previous communication by the same author, "On the Rigidity of the Earth" (Proceedings of the Royal Society, May 15, 1862), are proved.

II. "On the Exact Form and Motion of Waves at and near the Surface of Deep Water." By Professor W. J. Macquorn Rankine, C.E., F.R.S. &c. Received September 27, 1862.

(Abstract.)

The investigations of the Astronomer Royal and of other mathematicians on the question of straight-crested parallel waves in a liquid, are based on the supposition that the displacements of the particles are small compared with the length of a wave. Hence it has been very generally inferred that the results of those investigations are approximate only, when applied to waves in which the displacements, as compared with the length of a wave, are considerable.

In the present paper, the author proves that one of those results, viz., that in very deep water the particles move with a uniform velocity in vertical circles whose radii diminish in geometrical progression with increased depth, and consequently that surfaces of equal pressure, including the upper surface, are trochoidal,—is exact for all displacements, how great soever.

The trochoidal form of waves was first explicitly described by Mr. Scott Russell; but no demonstration of its exactly fulfilling the cinematical and dynamical conditions of the question has yet been published.

In 'A Manual of Applied Mechanics' (first published in 1858), the author stated that the theory of rolling waves might be deduced from that of the positions assumed by the surface of a mass of water revolving in a vertical plane about a horizontal axis; but as the theory of such waves was foreign to the subject of the book, he deferred until now the publication of the investigation on which that statement was founded.

Having communicated some of the leading principles of that investigation to Mr. William Froude in April 1862, the author was informed by that gentleman that he had arrived independently at similar results by a similar process, although he had not published them.

The following is a summary of the leading results demonstrated in the paper.

Proposition I.—In a mass of gravitating liquid whose particles revolve uniformly in vertical circles, a wavy surface of trochoidal profile fulfils the conditions of uniformity of pressure; such trochoidal profile being generated by rolling, on the under side of a horizontal straight line, a circle whose radius is equal to the height of a conical pendulum that revolves in the same period with the particles of liquid.

Proposition II.—Let another surface of uniform pressure be conceived to exist indefinitely near to the first surface; then, if the first surface is a surface of continuity (that is, a surface always traversing identical particles), so also is the second surface. (Those surfaces contain between them a continuous layer of liquid.)

Corollary.—The surfaces of uniform pressure are identical with surfaces of continuity throughout the whole mass of liquid.

Proposition III.—The profile of the lower surface of the layer referred to in Proposition II., is a trochoid generated by a rolling circle of the same radius with that which generates the upper surface; and the tracing-arm of the second trochoid is shorter than that of the first trochoid by a quantity bearing the same proportion to the depth of the centre of the second rolling circle below the centre of the first rolling circle, which the tracing-arm of the first rolling circle bears to the radius of that circle.

Corollaries.—The profiles of the surfaces of uniform pressure and of continuity form an indefinite series of trochoids, described by equal rolling circles, rolling with equal speed below an indefinite series of horizontal straight lines.

The tracing-arms of those circles (each of which is the radius of the circular orbits of the particles contained in the trochoidal surface which it traces) diminish in geometrical progression with a uniform increase of the vertical depth at which the centre of the rolling circle is situated.

The preceding propositions agree with the existing theory, except that they are more comprehensive, being applicable to large as well as small displacements. The following proposition is entirely new.

Proposition IV.—The centres of the orbits of the particles in a given surface of equal pressure stand at a higher level than the same particles do when the liquid is still, by a height which is a third proportional to the diameter of the rolling circle and the length of the tracing-arm, or radius of the orbits of the particles, and which is equal to the height due to the velocity of revolution of the particles.

Corollaries.—The mechanical energy of a wave is half actual and half potential; half being due to motion, and half to elevation. The crests of the waves rise higher above the level of still water than their hollows fall below it; and the difference between the elevation of the crests and the depression of the hollows is double of the quantity mentioned in Proposition IV.

The hydrostatic pressure at each individual particle during the wave-motion is the same as if the liquid were still.

Friction between a Wave and a Wave-shaped Solid.

In an Appendix is given the investigation of the problem, to find approximately the amount of the pressure required to overcome the friction between a trochoidal wave-surface and a wave-shaped solid in contact with it. The application of the result of this investigation to the resistance of ships was explained in a paper read to the British Association in 1861, and published in various Engineering Journals in October of that year. The following is the most useful of the formulæ arrived at. Let w be the heaviness of the liquid; f, the coefficient of friction; g, gravity; v, the velocity of advance of the solid; L, its length, being that of a wave; z, the breadth of the surface of contact of the solid and liquid; β, the greatest angle of obliquity of that surface to the direction of advance; P, the force required to overcome the friction; then

$$P=\frac{fwv^2}{2g}\cdot Lz(1+4\sin^2\beta+\sin^4\beta).$$

In ordinary cases the value of f for water sliding over painted iron is about ·0036. The quantity $Lz(1+4\sin^2\beta+\sin^4\beta)$ is what has been called the "augmented surface." In practice, $\sin^4\beta$ may in general be neglected on account of its smallness.

III. "On the Tides of the Arctic Seas.—Part II. The Semidiurnal Tides of Port Leopold, North Somerset." By the Rev. SAMUEL HAUGHTON, M.A., F.R.S., Fellow of Trinity College, Dublin. Received October 8, 1862.

(Abstract.)

The first part of the author's researches on the Tides of the Arctic Seas was forwarded to the Royal Society in November 1861, and contained the discussion of the Diurnal Tides of Port Leopold. In the present communication the Semidiurnal Tides of the same port are discussed, and the following results obtained. The eccentricity of the moon's orbit is calculated from the parallactic inequality, and found to be 0·5303.

The solitidal interval is 56^m.

The lunitidal interval $4^h\ 54^m$.

The ratio of the solar to the lunar coefficient is found to be 0·3956.

The mass of the moon $\frac{1}{71\cdot 11}$th.

And the depth of the Atlantic is calculated from received tidal theories. The most probable results are found to be,—

From semidiurnal tidal intervals . . 3·529 miles.

From diurnal coefficients . . . . 3·690 ,,

There are other values of the depth of the sea, much greater than these, which follow from other considerations of the tidal theory; and the author is unable to explain why theory should give results so different. The preceding, however, he believes to be most in accordance with facts.

IV. "On the Action of Chloride of Iodine on Iodide of Ethylene and Propylene Gas."—Second Notice. By MAXWELL SIMPSON, M.B., F.R.S. Received October 23, 1862.

In my last communication to the Society* I announced that a body having the composition expressed by the formula $C_4 H_4 I Cl$ was formed when iodide of ethylene was subjected to the action of chloride of iodine. I have since ascertained that the same body may be obtained by the direct action of ethylene gas on the latter reagent. By this process it can be prepared in large quantity with great facility.

* Proceedings, vol. xi. p. 590.

It is only necessary to pass the gas into a watery solution of the chloride of iodine, wash the reddish oil which collects at the bottom of the solution with dilute potash, and distil. The portion which passes over at about 145° Cent. is pure chloriodide of ethylene.

The specific gravity of the chloriodide at zero is 2·151. Heated with an alcoholic solution of potash, it suffers decomposition, iodide of potassium being formed, and a gas given off which burns with a green flame. This is no doubt chloride of aldehydene ($C_4 H_3 Cl$). This reaction goes far to prove that the true constitution of this body is represented by the formula $C_4 H_3 Cl, HI$, and not by the formula $C_4 H_3 I, H Cl$, proposed in my former paper.

Propylene gas derived from glycerine also yields an oil when passed into a solution of chloride of iodine, as I have already stated. In order to purify this, I found it necessary to distil it *in vacuo*, rejecting what came over at the beginning and towards the end of the process. The numbers I obtained on analysing this body prove its composition to be $C_6 H_6 ICl$.

Chloriodide of propylene, as I may call this compound, is when freshly prepared a colourless oil, having an ethereal odour and a sweet taste. Its specific gravity at zero is 1·932. When an effort is made to distil it under atmospheric pressure, it suffers decomposition, hydriodic acid being evolved in large quantity. Mixed with an alcoholic solution of potash and distilled, it yields iodide of potassium and an oily liquid (contained in the distillate and separable from it by water) which is very volatile and burns with a green flame. This is doubtless chloride of allyle ($C_6 H_5 Cl$).

The oil formed by the action of chloride of iodine on propylene gas obtained from amylic alcohol, I have not been able to obtain in a fit state for analysis.

The application of the foregoing process to other hydrocarbons would no doubt place in our hands many similar compounds.

V. "On certain Developable Surfaces." By A. Cayley, Esq. Received October 25, 1862. Read November 27, 1862.

(Abstract.)

If $U=0$ be the equation of a developable surface, or say a developable, then the hessian HU vanishes, not identically, but only by virtue of the equation $U=0$ of the surface; that is, HU contains U as a

factor, or we may write HU=U . PU. The function PU, which for the developable replaces, as it were, the hessian HU, is termed the prohessian; and since, if r be the order of U, the order of HU is $4r-8$, we have $3r-8$ for the order of the prohessian. If $r=4$, the order of the prohessian is also 4; and in fact, as is known, the prohessian is in this case =U. The prohessian is considered, but not in much detail, in Dr. Salmon's 'Geometry of Three Dimensions' (1862), pp. 338 and 426; the theorem given in the latter place is almost all that is known on the subject. I call to mind that the tangent plane along a generating line of the developable meets the developable in this line taken two times, and in a curve of the order $r-2$; the line touches the curve at the point of contact, or say the ineunt, on the edge of regression, and besides meets it in $r-4$ points. The ineunt, taken three times, and the $r-4$ points form a linear system of the order $r-1$, and the hessian of this system (considered as a curve of one dimension, or a binary quantic) is a linear system of $2r-6$ points; viz. it is composed of the ineunt taken four times, and of $2r-10$ other points. This being so, the theorem is, that the generating line meets the prohessian in the ineunt taken six times, in the $r-4$ points, and in the $2r-10$ points

$$(6+r-4+2r-10=3r-8);$$

it is assumed that $r=5$ at least.

The developables which first present themselves are those which are the envelopes of a plane

$$(a, b, \ldots \between t, 1)^n=0,$$

where t is an arbitrary parameter, and the coefficients $(a, b, \ldots)$ are linear functions of the coordinates; the equation of the developable is

$$\text{disct } (a, b \,.\,. \between t, 1)^n=0,$$

the discriminant being taken in regard to the parameter t. Such developable is in general of the order $2n-2$; but if the second coefficient b is $=0$, or, more generally, if it is a mere numerical multiple of a, then a will divide out from the equation, and we have a developable of the order $2n-3$; the like property, of course, exists in regard to the last but one, and the last, of the coefficients of the function. We thus obtain developables of the orders 4, 5, and 6 sufficiently simple to allow of the actual calculation of their prohessians. And the chief object of the present memoir is to exhibit these prohessians; but the memoir contains some other researches in relation to the developables in question.

December 1, 1862.

ANNIVERSARY MEETING.

Major-General SABINE, President, in the Chair.

The President announced that Sir Philip de Malpas Grey Egerton, Bart., had been re-elected by the Council a Trustee of the Soane Museum.

Professor Sylvester, on the part of the Auditors of the Treasurer's Accounts, reported that the total receipts during the past year, including a balance of £684 17*s.* carried from the preceding year, amounted to £4408 0*s.* 7*d.*; and that the total expenditure in the same period amounted to £4172 13*s.* 3*d.*, leaving a balance at the Bank of £618 17*s.*, and in the hands of the Treasurer of £16 10*s.* 4*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

His Royal Highness The Prince Consort.

Peter Barlow, Esq.
William Borrer, Esq.
John, Marquis of Breadalbane.
Sir Benjamin Collins Brodie, Bart.
James Burnes, Esq.
The Archbishop of Canterbury.
Sir John Croft, Bart.
George Granville Francis, Earl of Ellesmere.
James Ormiston M^cWilliam, M.D.
Joseph William Moss, Esq.
Rear-Admiral Sir James Clark Ross.
Edward Stanley, Esq.
The Ven. Archdeacon Thorp.
Rev. Mark Aloysius Tierney.
James Walker, Esq.

On the Foreign List.

Jean Baptiste Biot.
Francesco Carlini.

Defaulters.

John Auldjo, Esq.
Capt. L. L. B. Ibbetson.

Fellows elected since the last Anniversary.

On the Home List.

George Bentham, Esq.
Henry William Bristow, Esq.
The Right Hon. James, Earl of Caithness.
Alexander Ross Clarke, Captain R.E.
John W. Dawson, Esq., M.A.
Frederick J. Owen Evans, Esq., R.N.
John Braxton Hicks, M.D.
The Very Rev. W. Farquhar Hook, D.D.
George Rolleston, M.D.
Charles William Siemens, Esq.
Maxwell Simpson, Esq.
Balfour Stewart, Esq.
Thomas Pridgin Teale, Esq.
Sir James Emerson Tennent, LL.D.
Isaac Todhunter, Esq., M.A.
C. Greville Williams, Esq.

On the Foreign List.

César Mansuete Despretz.
Franz Ernst Neumann.
Ernst Heinrich Weber.

The PRESIDENT then addressed the Society as follows :—

GENTLEMEN,

IN addressing you for the first time from this Chair as the President whom you have honoured by your choice, my first duty must be the mournful one of expressing our deep regret at the loss we have sustained of the eminent person who preceded me. To the highest professional distinction and the scientific attainments appertaining thereto, and to the indispensable moral qualities of truth, justice, and candour, Sir Benjamin Brodie united other qualifications also highly befitting the office in which you placed him :—remarkable firmness and decision, accompanied by kindliness and urbanity, and an unusually extensive acquaintance with the sentiments and opinions of different classes of society, particularly of those who cultivate Literature and the Arts. The failure of his health and of his sight deprived us latterly of that habitual attendance at the Meetings of the Society, which he regarded as a duty and valued as a privilege. Whilst we mourn the loss which we have sustained by his decease, we have the gratification of knowing that he deemed the Presidency of the Royal Society the crowning distinction of his most honourable life.

In adverting in the briefest terms which it is possible for me to employ to the choice which you have been pleased to make of a successor to this most eminent man, I can only say that you have honoured me far beyond any pretensions which I could have ventured to claim for myself, and that it will be my earnest endeavour to justify your choice by emulating the example of Sir Benjamin Brodie in attention to the business and devotion to the interests of the Society.

Since the last Anniversary our country and our Queen have had to mourn the loss of the illustrious Prince, the active and enlightened friend and promoter of measures for advancing either the welfare and comfort of the great body of our people, or the interests of Science, Literature, and the Arts. In this great general loss the cultivators of Science have their own particular share. Qualified in an eminent degree to estimate the importance of Science as one of the elements of the strength and prosperity of a nation, his sound and extensive knowledge enabled him to appreciate the value of researches in the various branches of natural knowledge, and to give them on all suitable occasions the advantage of his countenance and support.

The Anniversary Meeting may be considered a fitting opportunity for apprising the Fellows generally of subjects out of the ordinary routine which may have occupied the attention of the Council in the past year. Without too much trespass on your time, I may permit myself to notice very briefly one or two such subjects, in the interest of which I may hope the Society will warmly participate.

The first of these relates to a desire which has manifested itself in the Colony of Victoria to possess a telescope of much greater optical power than any previously used in the Southern Hemisphere, to be employed chiefly in observations of the southern nebulæ. A convenient and suitable site has recently been appropriated for an observatory at Melbourne; and a grant of £4500 has been voted by the Colonial Legislature for the completion of the requisite buildings. In July of the present year the Board of Visitors of the Melbourne Observatory presented the following address to Sir Henry Barkly, K.C.B., Governor of Victoria:—

"The attention of the Board having been drawn to the following circumstances:—

"I. That, as long since as 1849, the facts brought to light by

Lord Rosse's Telescope were judged by the Royal Society of London and the British Association for the Advancement of Science to be so important as to justify them in making an urgent appeal to the British Government for the erection, at some suitable place in south latitude, of a telescope for the examination of the multiple stars and the nebulæ of the Southern Hemisphere, having greater optical power than that used by Sir John Herschel at the Cape of Good Hope; which appeal there is little doubt would have been successful but for the Russian war and the consequent expenditure;

"II. That, since that time, Lord Rosse reports that he has discovered systematic changes in some of the most important northern nebulæ;

"III. That the interest and scientific importance of the solution of the problem of their physical structure, as well as the probability of its accomplishment, are thus greatly increased;

"IV. That some of the most important nebulæ, and those presenting the greatest variety of physical features in close proximity, can be observed only in places having a considerable southern latitude;

"V. That the geographical position and clear atmosphere of Melbourne render it peculiarly suitable for this work, and that the arrangements already made for the establishment of an Astronomical Observatory on a permanent footing offer great facilities for carrying it on;

"VI. That, independently of the especial object to which such telescope would be applied, an Astronomical Observatory cannot be considered complete without an equatorially-mounted telescope of large optical powers:

"It was Resolved,—

"1st. That, in the opinion of the Board, the establishment of such a telescope in Melbourne would materially promote the advancement of science.

"2nd. That, before applying to the Colonial Government for any pecuniary grant in aid of this object, His Excellency the Governor be requested to obtain, through the Secretary of State for the Colonies, an expression of opinion from scientific men in England as to the importance of the results to be expected from it; the most suitable construction of telescope for the purpose, both as to the optical part

and the mounting; its probable cost, and the time requisite for its completion."

In October the President and Council received from the Duke of Newcastle, Her Majesty's principal Secretary of State for the Colonies, a communication in which their attention is requested to the copy of a despatch from Sir Henry Barkly, enclosing the address of the Board of Visitors which has just been read, and soliciting the cooperation of the Royal Society by a report on the several points on which an opinion is desired. The Duke further expressed his confidence in the readiness of the Royal Society to do whatever may be in their power for the encouragement of science in the Colony of Victoria.

No time was lost in bringing this communication before the Council, who requested the President and Officers to prepare the draft of a reply, to be approved at a subsequent meeting.

Upon the general question, viz. the importance of the results to be obtained by such a telescope at Melbourne in latitude 38° S., and on the honour which the establishment and maintenance of the proposed observatory would reflect on the Colony of Victoria, the reply might well have been immediate; but there are other points on which the opinion of the Royal Society is requested, viz. "the most suitable construction of telescope for the purpose, both as to the optical part and the mounting, its probable cost, and the time requisite for its completion," for replies to which more time and consultation are requisite. It happens fortunately that all these points were well discussed in the correspondence which passed between the members of the Committee of the Royal Society appointed in 1852 to consider the steps most desirable to be taken towards the establishment of a telescope of very great optical power for the observation of Nebulæ in the southern hemisphere. This correspondence was printed, and some copies of it yet remain; but since that epoch, a great step in advance has been made, by the construction by Mr. Lassell, at his own expense, of a 4-feet reflector, exceeding in dimension the telescope contemplated in 1852, by its conveyance from Liverpool to Malta, and by its employment for a twelvemonth past at Malta under the personal superintendence of Mr. Lassell himself. It is possible that the opinions previously formed may be in some respects modified by the additional experience which has thus been acquired; the President and Officers have therefore

placed themselves in communication with Mr. Lassell, and with other gentlemen who took the most active part in the previous correspondence, with a view of conveying in their reply the best information which it is in their power to procure; and they hope that they may be able to submit the draft of the reply to the new Council at its first meeting.

I cannot close this brief notice without congratulating the Society on the prospect thus opened of accomplishing an object of such manifest importance as to have induced the Royal Society and the British Association to solicit jointly the aid of Her Majesty's Government in effecting it;—and however great their disappointment may have been at the refusal which they received on that occasion, they will, if the present hopes are realized, have no reason to regret that it has been left to the Colony of Victoria to carry into execution an undertaking which may well be expected to hold a high place in the annals of science in all future time; and thus to set a noble example to the other Colonies of the British Crown. With such extensive dominion, embracing almost every variety of natural circumstance, it is only by the active cooperation of her colonies, according to the varying measure of their ability, that our country can hope to fulfil her scientific responsibilities.

Another subject on which information may be interesting to the Fellows, is that of the progress and approaching completion of the Manuscript Catalogue of the Titles of Scientific Memoirs contained in the Scientific Periodicals in all languages from the commencement of the present century to the year 1860. The formation of this Catalogue was commenced under the sanction of the Council in 1858, and has proceeded uninterruptedly to the present time. The titles are written in quadruplicate, and are designed to form—1st, a Serial Index; 2nd, an Index arranged alphabetically according to Authors' names; and 3rd, a Classified Index, classified according to subjects in all branches of science; the 4th set of titles remaining available for purposes not yet decided on. The first of these, *i. e.* the Serial Index, already forms sixty-two MS. volumes, and includes the most important series of Transactions and Journals; the titles in each series being arranged in chronological order. These volumes have been placed in the Library, where they are now available for reference; and fresh volumes

are added from time to time as they are completed. The preparation of the second, or Alphabetical Index, is considerably advanced. For the third, or Classified Index, the greater part of the titles are prepared, but the arrangement of the classification has not yet been finally decided upon.

The number of Titles at present entered in the Catalogue amounts to about 150,000; and there yet remain to be added from our own library about 10,000 more; besides such as may be added from other sources. As was anticipated, numerous deficiencies in this department of our Library were discovered in the course of the work. The Library Committee has taken measures to have these made good. In some instances imperfect series have been completed, and exchanges for new series effected, on the application of the Foreign Secretary to the several Societies by whom they are published; considerable purchases have also been made; and by all these means the Library has been largely increased in a department of essential importance to the interests of all the sciences. The expense incurred in preparing the Catalogue thus far has amounted to £980. When the Manuscripts of the three Indexes, Serial, Alphabetical, and Classified, are completed, it is proposed to place them in the Library for the use of the Fellows generally. In the course of the present Session it will be proper to consider the question of extending the sphere of their utility more widely by printing. It is scarcely possible to estimate too highly the advantage of rendering such indexes easily and generally accessible to persons engaged in scientific pursuits.

I will now proceed with your permission to the duty especially appropriate to the day, that of announcing the decision of the Council in awarding four Medals in the present year, and of stating the grounds on which those awards have been made.

The Council have awarded the Copley Medal to Thomas Graham, Esq., Master of the Mint, F.R.S., for three Memoirs on the Diffusion of Liquids, published in the 'Philosophical Transactions' for 1850 and 1851; for a Memoir on Osmotic Force in the 'Philosophical Transactions' for 1854; and particularly for a paper on Liquid Diffusion applied to Analysis, including a distinction of Compounds into Colloids and Crystalloids, published in the 'Philosophical Transactions' for 1861.

It is well known that when a bladder nearly full of alcohol is immersed in a vessel of water, the water will pass through the bladder and become mixed with the alcohol, though little or none of the alcohol will pass out to become mixed with the water. This result constitutes a particular case of the phenomena described by Dutrochet under the terms Endosmosis and Exosmosis. Ever since the attention of men of science was called to these phenomena by the French philosopher, they have attracted a large share of attention; and many chemists and physiologists have at different times endeavoured to define the laws that regulate these actions, which were regarded by many as proceeding from a force that had not previously been recognized.

It was not, however, till Mr. Graham adopted the simple expedient of placing the two liquids under experiment in contact with each other without the intervention of any septum, that a distinct idea of the steps of the operation was obtained. Mr. Graham proved that these movements were due partly to the action of the liquids on each other, and partly to their action on the septum. In three papers published in the 'Philosophical Transactions' for 1850 and 1851, he traced the laws of "Liquid Diffusion," and was thus enabled to show that the process in liquids is regulated by principles closely analogous to those which in earlier researches he had demonstrated to prevail in the diffusion of gases.

In these experiments a number of small jars of about 4 oz. capacity were prepared with necks ground to a uniform aperture of $1\frac{1}{4}$ inch in diameter. Into these jars the trial solutions were poured: each jar was then closed by a glass plate, and placed in a cylindrical vessel containing about 20 oz. of distilled water; the mouth being submerged at least one inch below the level of the water. The glass plate was then cautiously removed. Each solution having been thus treated, the jars were left for several days undisturbed at a steady temperature. After a sufficient length of time the mouth of each jar was again closed with a plate of glass, and the jars were withdrawn. The water contained in the outer vessel was then in each case evaporated, and the salt that had passed into it determined by weight. Saline substances were thus found to be divisible into certain groups of bodies of equal diffusibility; the rates of diffusion of the different groups being connected by simple numerical relations.

Amongst other important points determined, it was found that if two substances which do not combine chemically and which possess different rates of diffusibility, be mixed, and be then placed in a diffusion cell, they may be partially separated by the process of diffusion; and in some cases even chemical decomposition may be effected by this means. Thus if ordinary potash alum be subjected to diffusion, the sulphate of potash will diffuse out rapidly, and leave the sulphate of alumina in large proportion.

In 1854 Mr. Graham communicated to the Royal Society a paper on Osmotic Force, in which he examined the influence of a septum upon the process of diffusion; investigating particularly the class of phenomena specially studied by Dutrochet. By a simple modification of the apparatus employed, he was enabled to make his experiments quantitative, and thus to give them numerical precision. He found that the nature of the solution and its chemical operation on the material of the septum were very important. When animal membranes were employed, dilute alkaline solutions absorbed water rapidly, while weak acid solutions gave out water instead of absorbing it; neutral salts having little effect in promoting these osmotic movements.

But these investigations, important as they are, constitute but the introduction to the inquiry published in 1861, upon the application of Liquid Diffusion to Analysis. In this remarkable memoir Mr. Graham has shown that chemical compounds generally may be subdivided into two great classes, which are characterized by their relation to the process of liquid diffusion: one of these classes he terms *Crystalloids*, and the other *Colloids* as being typified by animal gelatine. The crystalloids form a solution generally free from viscosity, and always sapid. They are especially endowed with a tendency to diffusion through a membranous septum. The colloids, on the contrary, such as gum, starch, dextrine, caramel, tannin, gelatine, and albumen, are characterized by a remarkable sluggishness, and indisposition to diffusion or crystallization. When pure, they are nearly tasteless. Colloid bodies do not necessarily belong to the organic kingdom, though they are frequently met with among its constituents; and owing to their tendency to undergo slow but perpetual molecular change, together with their peculiar relations to water, they seem to be especially suited to form the plastic materials re-

quired for building up the tissues of the living organism. Although the two classes are widely separated in their properties, a complete parallelism appears to hold between them. Their existence in nature seems to call for a corresponding division of Chemistry into a Crystalloid and a Colloid department. In many cases the same body may be obtained either in the crystalloid or in the colloid modification. The chemistry of the two classes is distinct, and the reactions of the same body are different according as it is in the crystalloid or the colloid form. Chemists have been enabled to apprehend properly a number of anomalous facts and discrepancies in the reactions of various substances which were supposed to be identical until Mr. Graham had called attention to this important molecular difference in their structure. He has shown that the crystalloid is the static, as the colloid is the dynamic condition of a body: the usual tendency of the colloid is a gradual approach towards the crystalloid form.

The method by which Mr. Graham has obtained these important results is characterized by that simplicity which so eminently distinguishes the mode in which he has proceeded during the whole of this lengthened and important inquiry. The memoir contains a description of the process, which he names *Dialysis*, for separating a crystalloid from a colloid; and it is scarcely necessary to insist upon the practical importance which this method possesses. In the examination of organic mixtures for poisons which, like the vegetable bases, are crystallizable, it will afford most valuable aid, as it separates the poison without adding anything except a little pure water. Many organic colloids, such as gum, albumen, or caramel, may by its means be readily freed from saline impurities, which can scarcely be removed by any other known means. Its application to the recondite processes of secretion, and to many of the chemical changes taking place in the living organism, need not be insisted on. The door thus opened to further inquiry will no doubt be eagerly entered by the physiological chemist, who can hardly fail of obtaining new insight into the obscure but deeply important operations of the nutrition, reparation, and removal of tissue.

Mr. Graham,

In receiving this Medal, the highest honour which it is in the power of the Royal Society to award, you will accept it as a testi-

mony of the very high value which the Society attaches to your researches. It is our earnest hope that your health will be such as to permit you, in addition to your important public duties, to continue your most valuable scientific labours.

The Council have awarded the Rumford Medal to Professor Kirchhoff, of Heidelberg, for his researches on the Fixed Lines of the Solar Spectrum, and on the Inversion of the Bright Lines in the Spectra of Artificial Light.

The existence of definite rays in the light of flames coloured by various salts has long been known, and attracted the attention of Sir John Herschel as long ago as 1822; and in papers published a few years later, Mr. Fox Talbot called attention to the value and delicacy of this character in qualitative chemical analysis; showing, for example, how the red produced by lithia, and that produced by strontia, might thus be instantly distinguished by the difference in the system of bright lines seen in their respective spectra. He remarked at the same time what an exceedingly minute quantity of a metallic substance could thus be detected in a flame. But chemists generally were not aware of the precious means of qualitative analysis thus lying at their command; and, in fact, the elaboration of this mode of qualitative chemical analysis required a combination of chemical and physical observations for which the same individual was seldom properly qualified and equipped. It was necessary, on the one hand, to prepare a variety of substances in the highest state of chemical purity, and, on the other, to take a number of careful angular measures of the positions of bright lines in the spectra of various flames.

This labour has now been in great measure accomplished by the joint exertions of Professors Kirchhoff and Bunsen, to whom indeed is due the merit of having made this mode of chemical research available to the scientific world, and of having caused spectroscopes to be now in the hands of chemists generally, by whom they are employed with the greatest advantage in the qualitative examination of inorganic substances. Already the method has led to the discovery of three new elements, Cæsium and Rubidium in the hands of Professor Bunsen, Thallium in those of our own countryman, Mr. Crookes.

In the course of these researches Professor Kirchhoff made the remarkable discovery that flames which of themselves copiously emit rays of definite refrangibility, and consequently exhibit bright lines in their spectra, act at the same time as absorbing media of such a character as to stop rays of those precise degrees of refrangibility, when light containing rays of all kinds is transmitted through them. Accordingly when a bright source of light, which of itself gives a continuous spectrum, is viewed through such a flame, and the mixed light, consisting partly of the light emitted by the flame, and partly of the light transmitted through it, is analysed by a prism, instead of a bright line on a dark ground, there is seen a dark line on a bright ground, occupying exactly the same place. In order, however, that this inversion should be observed, it was found to be essential that the temperature of the bright source should exceed that of the flame; otherwise, in comparing the illumination of the place of the line in the spectrum with that of its neighbourhood, the loss by absorption would not overbalance the gain by emission.

These results Professor Kirchhoff showed to be a necessary consequence of Prevost's theory of exchanges, taken in an extended sense; so that, from the appearance of a bright line in the spectrum of an incandescent vapour (for it is only in the state of vapour that incandescent matter emits rays of definite refrangibility), it might be inferred that the chemical substance in the vapour to which the bright line was known to be due, would also in the state of vapour, at the same temperature, act as an absorbing medium capable of exhibiting a corresponding dark line in the spectrum of light transmitted through it,—an effect which the vapour would still continue to exhibit at a lower temperature, unless the mode of absorption were changed by so reducing the temperature.

This doctrine finds a striking application in the explanation which it affords of the existence of dark lines in the solar spectrum. The exact coincidence of the double line D of Fraunhofer with a corresponding bright line seen in many flames was pointed out by Fraunhofer himself; and a similar coincidence was observed by Sir David Brewster between a system of bright lines shown by deflagrating nitre, and a corresponding group of dark lines in the solar spectrum. The theory of Professor Kirchhoff leads us to expect such coincidences beforehand, and from the presence or absence in the solar

spectrum of dark lines answering to the bright lines observed in a flame, and referable to a known element, to infer with a high degree of probability the presence or absence, in a state of vapour, of that element in the atmosphere of the sun. Thus the presence of the elements sodium, potassium, iron, and some others in the sun's atmosphere has been rendered in the highest degree probable. And the same reasoning that applies to the sun applies also to the fixed stars, which are known to exhibit in their spectra fixed lines of their own, while many of their lines are identical with those of the sun; so that the enormous distance of these bodies does not prevent us from drawing some conclusions as to their chemical constitution.

It is not, however, to be expected that *all* the fixed lines of the solar spectrum should thus be traced home to particular elements. It is well known that when the sun is near the horizon dark lines make their appearance in the spectrum which are not seen when the sun is high, and which plainly owe their origin to absorption by the earth's atmosphere; though what the particular compounds are which absorb light in this manner is still unknown. It is very possible that gases, having a similar property of absorbing definite rays, may exist in the outer portions of the solar atmosphere, which, at the very high temperature necessary for incandescence, would either be decomposed, or would have their mode of absorption so changed that there would no longer be a perfect correspondence between the bright lines which might be exhibited by the light emitted at a high temperature, and the dark lines produced by absorption at a low temperature.

In mentioning these striking results due to Professor Kirchhoff, it seems right to refer to some earlier researches bearing closely on the subject. In 1848 M. Foucault made the remarkable discovery that the voltaic arc was at the same time a source of light, giving out the double bright line D, and a medium capable of absorbing light of that precise refrangibility; but he did not extend the result to ordinary flames, nor connect the absorption and emission by the theory of exchanges; and indeed his observation, which was only published in '*l'Institut*,' seems to have attracted little attention, and was not known to Professor Kirchhoff when he discovered the general inversion of bright lines. In two remarkable papers published in the

'Transactions of the Royal Society of Edinburgh,' Mr. Balfour Stewart has made the requisite extension of Prevost's theory in the case of radiant heat, deducing important consequences which he has verified by experiment. The transition was natural from radiant heat to light, and was made by Mr. Stewart himself. The paper containing this extension of the theory, and the experiments by which he had verified it, was published in the 'Proceedings of the Royal Society' for March 15, 1860, and accordingly only a very short time after Professor Kirchhoff announced to the Berlin Academy his discovery of inversion. Mr. Stewart, however, did not occupy himself with the emission of rays of definite refrangibility, his experiments having been chiefly made on heated coloured glasses.

The great number of the dark lines in the solar spectrum renders necessary a very exact determination of the places of dark and bright lines, lest an accidental juxtaposition should be mistaken for coincidence. To prosecute his researches with success, Professor Kirchhoff found it necessary to prepare a map of the solar spectrum combining largeness of scale with exactness to an extent not to be found in any published map; and to this laborious undertaking he vigorously applied himself. The result may be judged of by the admirable map of a portion of the spectrum published in the 'Transactions of the Berlin Academy;' but, unfortunately, the very zeal with which Professor Kirchhoff prosecuted his observations threw an obstacle in the way of his progress. The constant strain brought on a weakness of the eyes, which obliged him to suspend his observations for a time. It is understood, however, that the work is progressing, as other observers are engaged in taking the observations which he himself could not prudently continue.

Professor Miller,

In delivering to your care as our Foreign Secretary this Medal, to be transmitted to Professor Kirchhoff, we will ask you to accompany this testimony of our high esteem by the expression of our sympathy in the cause which has temporarily interrupted the excessive devotion with which he has pursued his valuable researches, and of our hope that he will not permit himself to be tempted personally to resume his observations until his eyes shall have perfectly recovered.

The Council have awarded a Royal Medal to the Rev. Dr. Thomas Romney Robinson, F.R.S., of Armagh, for the Armagh Catalogue of 5345 Stars, deduced from observations made at the Armagh Observatory, from the year 1828 up to 1854; for his papers on the Construction of Astronomical Instruments, in the 'Memoirs of the Astronomical Society;' and his paper on Electro-magnets, in the 'Transactions of the Royal Irish Academy.'

In various papers published in the 'Transactions of the Royal Irish Academy,' and in the 'Memoirs of the Royal Astronomical Society,' Dr. Robinson long since showed that he had profoundly studied the use as well as the mechanical construction of astronomical instruments, the various errors to which they are liable, and the best methods of discovering and eliminating them; and he proved himself to be fertile in ingenious suggestions for the improvement of instruments.

It is not, however, necessary to dwell further on these papers, as the Medal is more especially awarded for the Armagh Catalogue of the places of 5345 Stars, deduced from observations made at the Armagh Observatory between 1828 and 1854. This work has recently been published on the recommendation of the Royal Society by the aid of the annual Government Grant. Most of the stars have been observed five times, both in Right Ascension and North Polar Distance, with the Transit Instrument and Mural Circle respectively. No one who has not gone through some work of the same kind can imagine the vast amount of labour which the reduction of such a mass of observations requires. The individual results from each observation are given, so that we can form an accurate idea of the precision finally attained. Dr. Robinson has spared no pains to study the errors of all kinds of the individual instruments employed, either to correct them or to make the due allowance for them. Thus he examines the divisions of the mural circle by help of twelve microscopes, though only four are used in taking the actual observations. The effect of the variation of atmospheric density on the rate of the transit clock is compensated by a suitably adjusted barometer forming part of the pendulum. The true cylindricity of the pivots, both of the transit and the mural circle, is ensured by the method, introduced by him, of using a diamond point in the turning.

The materials which have been accumulated so laboriously and so

skilfully in this catalogue are of the highest value. The stars observed are almost entirely those observed by Bradley, and the comparison of the observed places gives a great addition to our knowledge of the proper motions of the fixed stars, and thus forms an enlarged basis for our deductions respecting the proper motion of our system in space.

Dr. Robinson gives a separate catalogue of the proper motions which he considers to be established by the comparison above mentioned.

The duties of the observatory and the preparation of the above-mentioned catalogue have not prevented Dr. Robinson from devoting a large amount of attention to physical research; as will be seen from his papers on the Lifting Power of an Electro-magnet, published in the 'Transactions of the Royal Irish Academy,' which contain the results of careful quantitative experiments extending, as far as other avocations permitted, over a period of about ten years; those on the curious subject of the Stratification of the Electric Discharge, published in the 'Proceedings' of the same Academy; and more recently an elaborate paper "On Spectra of Electric Light, as modified by the Nature of the Electrodes and the Media of Discharge," presented to the Royal Society on the 19th of last June, and since ordered to be printed in the 'Philosophical Transactions.' This last paper contains the results of careful measures of the angular position of the bright lines observed in no less than 173 spectra. The results would seem to indicate that in this branch of physics, which has recently excited a great deal of attention, overhasty generalizations have in some cases been made; but however that may be, the data collected in this paper can hardly fail to be of high importance in the event of any theory being propounded to account for the positions and variations of brightness of the lines.

Professor Stokes,

In presenting you with this Medal to be transmitted to Dr. Robinson, I may express the regret of the Royal Society that he should have been prevented by indisposition from attending personally to receive our congratulations on the accomplishment of the great work on which he has expended so vast an amount of labour; and our assurance of the warm welcome with which the Society will

greet his future contributions, whether on astronomical or physical subjects.

The Council have awarded one of the Royal Medals to Dr. Alexander William Williamson, Professor of Chemistry and of Practical Chemistry in University College, London, for his researches on the Compound Ethers, and his subsequent communications in Organic Chemistry.

One of the most important discussions entered upon by chemists in the last few years has been the true molecular arrangement of ordinary vinic alcohol and ether. The interest shown in the subject, both at home and abroad, may be ascribed to the opinion generally prevailing amongst chemists, that upon the correct appreciation of the phenomena involved in the process of etherification must depend the successful unlocking of many secrets connected with organic chemistry.

Gay-Lussac and the French school regarded ether as olefiant gas combined with one molecule of water, and alcohol as olefiant gas combined with two molecules of water. Berzelius thought ether and alcohol were oxides of different hydrocarbons; and, seeing the difficulty of explaining the process of etherification by the then current hypothesis, supposed that a peculiar force was exerted, to which was given the name of Catalysis. Lastly, the German school, with Liebig at their head, regarded ether as the oxide of the unisolated radical ethyl, and alcohol as its hydrated oxide.

Dr. Williamson here took up the question, and showed that when sulphuric acid and alcohol are brought together, a change of place is effected between the ethyl of the sulphovinic acid produced and an atom of hydrogen of the alcohol, and consequently that the ether which results from the action is compounded of a double molecule of ethyl and a single molecule of oxygen.

These views were put beyond the range of mere hypothesis by a happily conceived series of experiments, which resulted in the discovery of the important substances known as the "compound" or "mixed ethers." Dr. Williamson showed that these bodies are formed upon the type of water, the two hydrogen atoms of which are capable of replacement by different hydrocarbon radicals.

These considerations were embodied in two papers which have had

a marked influence upon the development of organic chemistry; an early fruit of which was the discovery by Gerhardt of the organic anhydrous acids, which may be regarded as the application of Dr. Williamson's processes to negative instead of positive radicals.

A large field of inquiry having been thus opened, a suggestive paper on the Constitution of Salts and Acids speedily followed, in which Dr. Williamson showed that acetic acid is formed by the substitution of one atom of oxygen for two atoms of hydrogen in alcohol. Acetic acid, thus regarded, encloses the new radical othyl or acetyl.

The question of types and the doctrine of substitution are further elaborated in six memoirs read before the Royal Society in 1854.

In these researches are exemplified the rarely associated powers of discovering new facts, and of evolving from them truths of a higher order. They also show that the scientific importance of substances must not be estimated by the bare knowledge gained from their ultimate analysis, but that valuable generalizations may follow from the establishment of correct ideas of their molecular grouping; generalizations, indeed, which effect a new insight into nature, and to a great extent anticipate a rich field of subsequent research.

PROFESSOR WILLIAMSON,

In presenting you with this Medal, testifying the value which the Royal Society attaches to the researches on which you have been engaged, I would venture to express on their behalf the hope that, in the many years which we trust are yet before you, science may continue to profit, and may profit largely, by your exertions in researches for which you have shown yourself to be so highly qualified.

On the motion of Sir R. Murchison, seconded by Mr. Tite, it was resolved:—

"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The Statutes relating to the election of Council and Officers having been read, and Dr. Anderson and Major-General Sir Andrew Scott Waugh having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected.

The following Gentlemen were declared to be duly elected as Council and Officers for the ensuing year:—

President.—Major-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.—{ William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L.

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—Benjamin Guy Babington, M.D.; George Bowdler Buckton, Esq.; William Benjamin Carpenter, M.D.; Warren De La Rue, Esq., Ph.D.; Sir Philip de M. Grey Egerton, Bart.; Captain Douglas Galton, R.E.; Robert Godwin-Austen, Esq.; Joseph Henry Green, D.C.L.; Joseph Dalton Hooker, M.D.; John Lubbock, Esq.; Prof. James Clerk Maxwell, M.A.; Prof. Richard Owen, D.C.L., LL.D.; Prof. Henry J. Stephen Smith, M.A.; Prof. James Joseph Sylvester, M.A.; Prof. Charles Wheatstone, D.C.L.; Rev. Robert Willis, M.A.

The thanks of the Society were voted to the Scrutators. The Society then adjourned.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s.* annually.	Paying £4 annually.	Total.
December 1, 1861..	6	48	327	5	275	661
Since compounded..	......	......	+4	......	−4	......
Since elected	......	+3	+3	......	+12	+18
Since readmitted ..	......	......	......	......	+1	+1
Since deceased	−1	−2	−7	−1	−7	−18
Defaulters	......	......	......	......	−2	......
December 1, 1862..	5	49	327	4	275	660

Receipts and Payments of the Royal Society between November 30, 1861, and December 1, 1862.

	£	s.	d.
Balance at Bank, and on hand	684	17	0
Annual Subscriptions and Compositions	1624	0	0
Rents	239	18	2
Dividends on Stock	950	16	5
Ditto, Ditto, Trust Funds	280	2	10
Ditto, Ditto, Stevenson Bequest	459	7	9
Sale of Transactions, Proceedings, &c.	389	4	4
Chemical Society, Proceedings, 1860–61	50	0	0
Tea Expenses and Gas, repaid	66	8	1
The Earl of Rosse: part Cost of Engravings	50	0	0
Parcel Charges recovered	11	10	0
Miscellaneous	1	16	0
	£4808	0	7

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £117 6*s.* 4*d.* per annum.

Estate at Acton, Middlesex (34 A. 3 R. 11 P.), £110 0*s.* 0*d.* per annum.

Fee farm rent in Sussex, £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per Cent. Annuities.

£27,077 9*s.* 0*d.* Consolidated Bank Annuities.

£513 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock.

Scientific Relief Fund.

Investments up to July 2nd, 1861, New 3 per Cent. Annuities ....£4934 15*s.* 0*d.*

Subscriptions 44 5 1

£4979 0 1

	£	s.	d.
Salaries, Wages, and Pension	985	12	0
The Scientific Catalogue	212	2	5
Books for the Library and Binding	335	9	6
Printing Transactions and Proceedings, Paper, Binding, Engraving, and Lithography	1251	9	6
Purchase of £600 Three per Cent. Consols	552	15	0
Bookcases, Carpet, and Upholstery Work	102	18	9
Heating Apparatus in Store Rooms	57	7	0
Fireproof Iron Safe	40	7	8
Coal and Lighting	103	3	11
Tea Expenses	56	7	3
Painting, Cleaning, and Miscellaneous House Expenses	55	7	10
Cleaning Frames and Revarnishing Pictures	48	5	0
Fire Insurance	42	1	6
Lecture Expenses	11	0	6
Law Expenses	26	13	10
Shipping Expenses	14	10	9
Stationery	14	2	0
Taxes	11	5	0
Postage, Parcels, and Petty Charges	35	18	10
Subscription, Mablethorpe Schools	2	2	0
Donation Fund	165	0	0
Wintringham Fund	34	11	6
Copley Medal Fund	4	9	5
Mr. De La Rue, Bakerian Lecture	3	17	0
Prof. Kölliker, Croonian Lecture	2	17	6
Rev. Dr. Stebbing, Fairchild Lecture	2	17	8
Balance at Bank	618	17	0
,, Catalogue Account	14	14	6
,, Petty Cash ,,	1	15	9
	£4808	0	7

W^{M} ALLEN MILLER,

Treasurer.

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending December 1, 1862.

				£	s.	d.
Annual Subscriptions				1138	0	0
Admission Fees				150	0	0
Compositions				336	0	0
Rents				239	18	2
Dividends on Stock (exclusive of Trust Funds)				950	16	5
" on Stevenson Bequest				459	7	9
Sale of Transactions, Proceedings, &c.				389	4	4
Chemical Society, for Proceedings, 1860–61				50	0	0
Geological Society, Tea Expenses and Gas	£28	9	1			
Linnean Society, ditto	12	16	2			
Chemical Society, ditto	12	16	2	66	8	1
Geographical Society, Expenses for Gas	8	4	8			
Social Science Association, and St. George's Rifles, ditto	4	2	0			
The Earl of Rosse, part Cost of Engravings				50	0	0
Parcel Charges recovered				11	10	0
Miscellaneous				1	16	0
				3843	0	9
Received too late to be included in the above.						
Chemical Society, for Proceedings, 1861–62				50	0	0
Income available for the Year ending Nov. 30, 1862				3893	0	9
Expenditure in the Year ending Nov. 30, 1862				3406	5	3
Excess of Income over Expenditure in the Year ending Nov. 30, 1862				£486	15	6

				£	s.	d.
Salaries, Wages, and Pension				985	12	0
The Scientific Catalogue				212	2	5
Books for the Library	£222	5	6	335	9	6
Binding ditto	113	4	0			
Printing Trans., Parts II. & III. 1861	315	2	3			
Ditto Proceedings, Nos. 47–51	130	15	7			
Ditto Miscellaneous	98	7	1	1251	9	6
Paper for Transactions and Proceedings	163	4	9			
Binding and Stitching ditto	69	17	0			
Engraving and Lithography	474	2	10			
Bookcases, Carpet, and Upholstery Work				102	18	9
Heating Apparatus in Store Rooms				57	7	0
Fireproof Iron Safe				40	7	8
Coal and Lighting				103	3	11
Tea Expenses				56	7	3
Painting, Cleaning, and Miscellaneous House Expenses				55	7	10
Cleaning Frames and Revarnishing Pictures				48	5	0
Fire Insurance				42	1	6
Lecture Expenses				11	0	6
Law Expenses				26	13	10
Shipping Expenses				14	10	9
Stationery				14	2	0
Taxes				11	5	0
Postage, Parcels, and Petty Charges				35	18	10
Subscription, Mablethorpe Schools				2	2	0
				£3406	5	3

W^M ALLEN MILLER,

Treasurer.

December 11, 1862.

Major-General SABINE, President, in the Chair.

The President announced that he had appointed the following Members of the Council to be Vice-Presidents :—

The Treasurer.
Dr. Carpenter.
Sir Philip de M. Grey Egerton, Bart.
Professor Wheatstone.
The Rev. Professor Willis.

Dr. Rolleston and Mr. Henry Bristow were admitted into the Society.

The following communications were read :—

I. "Observations on several Mineral Substances, including their Analysis, &c." By Dr. T. L. Phipson, F.C.S. Communicated by Dr. Stenhouse. Received November 3, 1862.

(Abstract.)

In this paper the author gives an account of a series of mineral substances more or less recently discovered, and several of which have been lately imported into England to be utilized in various arts and manufactures. Fourteen of these substances have been submitted to careful analysis by the author, and their chemical composition and properties determined. The minerals described include—

1. Sombrerite, a phosphate of alumina and lime from the Antilles. 2. A fossil phosphate from the Isle of Wight, a most perfect description of wood fossilized by apatite and fluorine. 3. A phosphate from the West Indies, derived from the decomposition of guano. 4. Bicarbonate of ammonia from the Chinca Isles, off the coast of Peru, for specimens of which the author is indebted to Capt. Marcus Lowther, R.N., and which appears never to have been completely analysed before. 5. Tinkalzite, a hydrous borate of lime and soda from Peru, already utilized in the arts as a substitute for borax. 6. Stibiconise, from Borneo, a hydrated antimonious acid, which

has been lately imported into England in notable quantities. 7. A modern limestone rock, forming on the coast of Flanders. 8. The Limon de la Hesboye, a loam which covers a large portion of Belgium and part of France, and which is extremely remarkable for its fertility: although it contains upwards of 90 per cent. of sand, its analysis shows that it possesses all the chemical ingredients necessary to form a fertile soil. 9. Vitriolite, or natural sulphate of iron, recently discovered in Turkey, and which has been analysed by M. Pisani and the author; it is remarkable from the fact that part of the iron is replaced by copper, without changing the crystalline form or the percentage of water. 10. An oolitic hematite from Namur (Belgium), remarkable for its peculiar structure, which may have been caused by the incrustation of insects' eggs (*Notonecta*), as we see is the case with the oolitic limestone of the Mexican lakes. 11. The argentiferous quartz or gossan of Cornwall, in which the author finds that the silver is contained as Fahlerz (grey copper): when the grey copper is freely disseminated through the rock, the percentage of silver (metallic) averages about 0·2 per cent.; but in the ordinary yellow and brown gossan, where the grey copper is not visible, the silver averages about 0·04 per cent., or $14\frac{1}{3}$ oz. to the ton. 12. The iserine sands of Australia and Bourbon Isle. 13. A bituminous conglomerate from Australia, remarkable as containing nearly 40 per cent. of petroleum and bitumen, with carbonate of lime, sand, and mica, &c.; it exudes from a tertiary limestone on the river Murray. 14. The arseniferous sulphur of Naples, which, according to the author's analysis, contains, besides 11·162 per cent. of arsenic, about 0·264 of selenium, which can be easily extracted from it in a pure state.

Of each of these substances the author gives in the present paper a detailed description and a complete analysis, believing that such researches are not devoid of utility. As most of the substances alluded to are applicable in some way or other to the wants of man, the author is continuing these investigations as opportunity offers, by submitting to careful analysis the different new or little-known minerals which happen to come under his notice.

II. "On the Strains in the Interior of Beams." By George Biddell Airy, F.R.S., Astronomer Royal. Received November 6, 1862.

(Abstract.)

The author states that he had long desired to possess a theory which should enable him to compute numerically the strains on every point in the interior of a beam or girder, but that no memoirs or treatises had given him the least assistance. He had therefore constructed a theory, which solves completely the problems for which he wanted it, and which appears to admit of application at least to all ordinary cases.

The theory contemplates forces acting in one plane. A beam, therefore, is considered as a lamina in a vertical plane,—the same considerations applying to every vertical lamina of which a beam may be conceived to be composed.

The author remarks that it is unnecessary to recognize every possible strain in a beam. Metallic masses are usually in a state of strain from circumstances occurring in their formation; but such strains are not the subject of the present investigation, which is intended to ascertain only those strains which are created by the weight of the beam and its loads. The algebraical interpretation of this remark is, that it is not necessary to retain general solutions of the equations which will result from the investigation, but only such solutions as will satisfy the equations.

After defining the unit of force as the weight of a square unit of the lamina, and the measure of compression-thrust or extension-pull as the length of the ribbon of lamina whose breadth is the length of the line which is subject to the transverse action of the compression or tension, and whose weight is equal to that compression or tension, the author considers the effect of tension, &c., estimated in a direction inclined to the real direction of the tension, and shows that it is proportional to the square of the cosine of inclination. He then considers the effect of compounding any number of strains of compression or tension which may act simultaneously on the same part of a lamina, and shows that their compound effect may, in every case, be replaced by the compound effect of two forces at right angles to each other, the two forces being both compressions, or

both tensions, or one compression and one tension. Succeeding investigations are therefore limited to two such forces.

Proceeding then to the general theory of beams, it is remarked that if a curve be imagined, dividing a beam into any two parts, the further part of the beam (as estimated from the origin of coordinates) may be considered to be sustained by the forces which act in various directions across that curve, taken in combination with the weight of the further part of the beam, the load upon that part, the reaction of supports, &c. Expressing the forces in conformity with the principles already explained, the three equations of equilibrium are formed, in which are involved several integrals depending on the form of the curve and on the forces. As the same equations must apply to any adjacent curve, the author remarks that this is a proper case for application of the Calculus of Variations; and on making that application, a remarkable relation is found to exist among the three functions depending on the forces acting at one point, from which it is immediately inferred that their algebraical expressions are the partial differential coefficients (of the second order) of a single function of the coordinates of the point of action. On substituting the partial differential coefficients, the integrations can be immediately performed; and the three equations assume a form of great simplicity, from which the sign of integration has entirely disappeared.

A form is then assumed for the principal function, with indeterminate coefficients, and it is shown that some of the constants may be eliminated by means of the three equations. But in the actual applications it is necessary to determine some remaining constants by considerations peculiar to each case. Now there is one modification of the strains whose value can be ascertained by ordinary mechanics, namely, the horizontal part of compressive force in the part of the beam above the neutral line, and the horizontal part of tension force in the part of the beam below the neutral line. (These words apply to a beam supported at both ends; in the case of a beam projecting from a wall, the words "compression" and "tension" must be reversed.) By determining the corresponding expression on the theory of this memoir, and comparing the two, the remaining constants and the form of the function are completely determined. From its partial differential coefficients are found the three functions depending on the forces acting at any one point (as already men-

tioned), and from these three functions are found the magnitudes of the two principal forces of compression or tension, and the angle which one of them makes with y, in a form admitting of numerical calculation.

The author then applies the theory to six cases, namely, (1) a beam projecting from a wall; (2) a beam supported at its ends; (3) a beam supported at its ends and carrying a load on its center; (4) a beam supported at its ends and carrying an excentric load; (5) a beam strained at both ends by the connexion of other beams, in the manner of the tubes of the Britannia Bridge; (6) a beam strained at one end only. Cases (3) and (4) require the use of discontinuous functions. Tables are given, exhibiting the numerical magnitudes of the two principal forces and the angle made by one of them with y, for 121 points in case (1), and for 231 points in each of the other cases. By means of these numbers, diagrams are formed, exhibiting in each case the directions of the lines of compression-thrust and tension-pull in every part of the beam.

III. "Photochemical Researches.—Part V. On the Measurement of the Chemical Action of Direct and Diffuse Sunlight." By R. W. Bunsen, For. M.R.S., and H. E. Roscoe, B.A. Received November 11, 1862.

(Abstract.)

In one of the four communications which the authors have already had the honour of presenting to the Royal Society on the subject of the measurement of the chemical action of light, the attempt was made to determine experimentally the laws regulating the distribution of the chemical action of the sunlight and diffuse daylight on the surface of the earth when the sky is perfectly unclouded and the atmosphere clear. The methods of measurement there employed do not, unfortunately, apply to the much more usually occurring case of cloudy skies and hazy atmosphere. The aim of the present communication is to describe an entirely different mode of measuring the chemical action effected at any point on the earth's surface by the total sunlight and diffuse daylight, under the most widely varying conditions of situation, climate, and state of the atmosphere.

In spite of the various futile attempts which have been made to register and measure the chemical action of light by means of photographic tints, it still appeared possible in this way to attain the desired end. No instruments founded on such a mode of measurement can yield reliable results unless we know the conditions under which photographic surfaces of a constant degree of sensitiveness can be obtained, and unless the relations be determined which exist between the degree of tint produced, and the time and intensity of the light acting to effect such a tint.

The first point which the authors examine, is whether the photographic tints produced vary in shade in the direct ratio of the intensities of the acting light. Several experiments proved that no direct ratio between the degree of blackening and the intensities of the light exists. Hence it is necessary to relinquish the idea of employing any mode of measurement founded on the comparison of photographic tints of different shades. The next point examined is whether equal shades of blackness always correspond to equal products of the intensities of the acting light into the times of insolation. For the purpose of testing the truth of this proposition, an instrument is employed by which photographic sensitized paper can be exposed for times which can be exactly measured to within small fractions of a second. This instrument consists essentially of a pendulum vibrating about $\frac{3}{4}$ seconds, by whose oscillation a sheet of darkened mica is withdrawn from, and brought back over, a horizontal strip of paper prepared with chloride of silver, and fixed in a constant position relative to the pendulum and sheet of mica. The time during which each point in the length of the strip is exposed is different, and the time of insolation for each point can be calculated when the length and position of the strip, and the duration and amplitude of the pendulum's vibration are given. A Table exhibits for each millimetre in length of the strip, as measured by a scale attached, the time of exposure in seconds which the corresponding point of the strip undergoes in one vibration of the pendulum. These numbers require to be multiplied by n if the paper has been insolated for n vibrations.

The paper insolated whilst the pendulum is oscillating, exhibits throughout its length a regularly diminishing shade from dark to white; and the time of insolation of any point is found by reference

to the Table. If we wish to determine which of these shades corresponds to another tint produced by a separate insolation, we cannot make the comparison by daylight or ordinary lamp-light, as these lights produce considerable changes of tint in the sensitive paper. The two shades may, however, be perfectly and safely compared by the light of a bright soda-flame; this light possesses the great advantage of being chemically inactive, and likewise of rendering imperceptible those slight differences of colour which make the comparison of two shades by the ordinary light so difficult.

In order to compare any other photographic tint with the point of equal shade on a strip, the latter, together with its millimetre scale, is attached to a board, in a darkened room. The board slides in a groove, so that it can be moved horizontally; and in front of the paper strip a small block holds in a fixed position a small piece of the tinted paper which it is desired to compare. On throwing the light of a bright soda-flame upon both surfaces it is easy, by moving the board from side to side, to find the exact point at which the shade of the strip is identical to that of the other tinted paper. It is then only necessary to consult the Table in order to find the time in seconds during which the paper must have been exposed in order that it should attain the tint in question. A series of lights of known intensities was obtained, by allowing the sun to shine through holes of known size. The images thus formed fell on to a piece of prepared paper; and the tints produced were compared with a strip darkened in the pendulum-apparatus, and thus the time of exposure necessary to effect the shade determined. Experiments made with intensities varying from 1 to 50, show that within these limits equal shades of blackness correspond to equal products of the intensities of the acting light into the times of exposure; so that the light 1 acting for the time 50, produced the same degree of blackening as the light 50 acting for the time 1.

A method for measuring the chemical action of light by simple observations is then founded upon this proposition. Thus, if we assume as the unit of photochemical action that intensity of light which produces in the unit of time a given degree of shade, we have only to determine, on a strip of paper tinted in the pendulum-apparatus, the point where the shade of the strip coincides with the given tint; the reciprocals of the times which correspond to these

points of equal shade give the intensities of the light expressed in terms of the above unit.

This method of measurement is available only—

1. If the phenomena of photochemical induction do not interfere with the blackening of the paper.

2. If a photographic surface of a constant degree of sensitiveness can be prepared.

3. If an unchangeable tint can be obtained which can be exactly compared with the photographic paper.

The result of a series of experiments made by varying the number of the vibrations and calculating the intensity from each observation, showed that photochemical induction does not exert any prejudicial effect upon the measurements.

The question into which the authors enter at greatest length as being the most important for determining the exactitude of the measurements, relates to the mode of preparing a standard paper possessing a constant degree of sensitiveness. The relative degree of sensitiveness is determined by exposing the papers to one and the same light for the same length of time, and then comparing their tints with the shades of a strip prepared in the pendulum-apparatus, fixed in a solution of hyposulphite of soda, and furnished with an arbitrary scale. The influence of the strength of the nitrate-of-silver solution upon the sensitiveness is first examined; a series of experiments shows that with the same homogeneously salted paper, the sensitiveness of the film does not alter when the strength of the silver solution varies from 8 to 10 or 12 parts of nitrate of silver to 100 of water. Further examination showed that the time during which the paper lies upon the surface of the silver bath may vary from 15 seconds to 8 minutes, without any difference in the sensitiveness of the paper being noticed; and no difference is found by the employment of silver solutions which had been long in use and those freshly prepared. The papers thus silvered may be preserved for from 12 to 15 hours in the dark without undergoing any change in their sensitiveness.

If the paper be allowed to float on the surface of the solution of chloride of sodium as on that of the silver bath, the sheet after silvering exhibits, on drying, a very unequal degree of sensitiveness in its various parts. If, on the contrary, the sheet be well soaked in the

salt-bath no such irregularity appears, and the sheet is of an equal degree of sensitiveness throughout its whole surface. This fact is determined by several extended series of experiments. The effect of change of concentration of the salt-bath upon the sensitiveness of the film is very great; and, as far as the observations extend, no limit exists beyond which an increase or a diminution of the percentage of salt in solution ceases to affect the sensitiveness of the film. Hence, in order to obtain constant results it is necessary to employ a solution of chloride of sodium of constant strength. By using solutions of the same strength, papers of a constant degree of sensitiveness are obtained.

The influence of the thickness of the paper employed is next examined. Experiment shows that differences in the thickness of white paper, such as is usually employed for photographic purposes, is without influence upon the sensitiveness of the film of chloride of silver.

The changes in atmospheric temperature, from 3° C. to 50° C., and in atmospheric moisture are likewise found not to influence the sensitiveness of the prepared paper.

From the experimental results detailed in the communication, it appears that by adhering to a certain mode of preparation, a standard paper can be obtained, which at all times possesses a degree of sensitiveness sufficiently constant for the purposes of exact measurement. In the following extract from a larger Table, the readings are given which were made from papers prepared in three different salt solutions of the strengths mentioned, and silvered in a solution containing 12 of nitrate of silver to 100 of water. Equality in the numbers in each of the columns III. and IV. denotes equality in the readings and in the tint, and therefore equality in the sensitiveness of the prepared surfaces. Three sheets of paper were dipped into each solution. These numbers likewise show the great degree of accuracy with which tints can thus be compared.

I. Paper.	II. Na Cl to 100 parts of water.	III. Intensity No. 1.	IV. Intensity No. 2.
Upper part of sheet No. 2 ...	3·026	87·0	75·4
Middle part of sheet No. 3 ...	2·950	86·3	74·4
Middle part of sheet No. 2 ...	3·028	86·0	74·9
Lower part of sheet No. 2 ...	3·000	85·9	74·4

The next subject considered is the preparation of an unvarying tint which can be easily obtained and used as the standard of comparison. This is effected by grinding together 1000 parts of pure oxide of zinc with 1 part of pure lamp-black. A series of experiments showed that a colour can thus be prepared which possesses a constant and unalterable shade; and this can be used as a measure of the standard tint.

Having proved that a standard photographic paper of constant sensitiveness, and a standard tint of unvarying shade can be prepared, it is only necessary to apply the proposition that equal products of the intensities of the light into the times of insolation effect equal shades of blackness, in order to found a method of comparative measurement of the chemical action of the total daylight. As the *unit* of measurement, the authors propose to adopt that intensity of the light which in one second produces the standard tint of blackness upon the standard paper.

When the standard paper is insolated in the pendulum-apparatus, a strip is obtained which is tinted with every gradation of shade from dark to white. If the point on this strip is determined which coincides in shade with a paper covered with the standard tint, we have only to look into the Table to obtain the time of insolation (t), in seconds, which is necessary to produce the shade corresponding to the reading on the millimetre scale. If this time of insolation were found to be one second, the intensity of the light then acting would be $I=1$; for any other time the intensity of the chemical rays would be $\frac{1}{t}$.

As an example of such measurement, the authors append three series of observations, giving the total amount of chemically active rays falling on a horizontal surface at Manchester in summer and winter, made at intervals of 10 minutes throughout three separate days. These observations are likewise graphically represented as curves, which show maxima and minima exactly corresponding to the appearance and disappearance of the sun; and from them some idea may be formed of the vast differences which occur in the intensity of the chemical rays falling on the earth's surface during the longest and the shortest days.

In conclusion the authors state that it is possible, by using the

pendulum-apparatus, to construct a portable instrument by means of which a large number of observations can be made upon a few square inches of paper. They reserve the description of their instrument for a future occasion.

IV. "Notes of Researches on the Poly-Ammonias.—No. XXI. On Paraniline." By A. W. HOFMANN, LL.D., F.R.S. Received December 2, 1862.

In a short paper submitted to the Royal Society about a year ago *, I called attention to some of the by-products which are obtained in the manufacture of aniline upon a large scale, and more especially to toluylene-diamine, the primary diamine of the toluyl-series.

MM. Collin and Coblenz, aniline manufacturers at Labriche, near St. Denis, Paris, to whose kindness I had been indebted for the material used in these researches, immediately after their publication transmitted to me with the utmost liberality a large quantity of basic oils boiling at temperatures higher than the boiling-point of aniline, which are separated from the pure aniline by rectification, and are known in the language of the laboratory as *queues d'aniline*. The investigation of this complex mixture has been interrupted by numerous engagements arising from the International Exhibition; and it was only within the last two months that I was enabled to resume the inquiry.

This inquiry is far from being finished; but some of the results already obtained are sufficiently definite for publication.

Submitted to distillation, the *queues d'aniline* begin to boil at about 182°, considerable quantities of pure aniline passing over; the temperature gradually rises without any indication of a fixed boiling-point, until it becomes necessary to remove the thermometer; in fact the last bases are volatilized only at temperatures not very far short of a red heat. By collecting separately what distils between 200° and 220°, and again what comes over between 270° and 290°, basic oils are obtained from which, by appropriate treatment, very considerable quantities, respectively, of toluylamine (toluidine) and toluylene-diamine may be separated. The former of these bases more

* Proceedings, vol. xi. p. 518.

especially is obtained in so large a quantity from this source, that M. Eugen Sell, a young chemist working in my laboratory, was enabled to engage in a more minute investigation of this substance.

The bases which accompany the monamine and diamine of the toluyl-series being liquids, their separation is by no means easily accomplished. Theory suggests that this mixture consists chiefly of the higher homologues of the toluyl-bases. These substances being far more easily prepared from their pure hydrocarbons *, I have for the present refrained from entering very minutely into the examination of these oils. The following remarks are therefore exclusively devoted to the fraction of the bases which boils at the highest temperature.

On collecting separately what comes over above 330°, a brown, viscid, scarcely mobile liquid is obtained, which at the first glance appears to present scarcely sufficient interest for a more minute examination. This liquid proved to be a mixture of several compounds. Treated with dilute sulphuric acid it solidified into a semisolid crystalline mass, which by filtration separated into a crystalline sulphate almost insoluble in water, and a sulphate easily soluble, the base of which forms the subject of this communication.

Decomposed by means of caustic soda, this sulphate yielded a viscid basic oil, which after some days solidified into a semisolid crystalline mass. This was purified from adhering oil by pressure between folds of bibulous paper, and crystallized first from water, and subsequently once or twice from boiling alcohol. Long white silky needles were thus obtained easily soluble in alcohol and ether, difficultly soluble in water, fusible at 192°, and boiling beyond the range of the mercurial thermometer, but distilling without decomposition.

When submitted to combustion, this substance was found to contain

$$C_6 H_7 N,$$

and thus to have exactly the same composition as aniline, from which

* I may mention in passing, that dinitrocumol, $C_9 H_{10} (NO_2)_2$ obtained by submitting cumol to the action of a mixture of nitric and sulphuric acids, when distilled with iron and acetic acid, yields cumylene-diamine, a beautiful crystalline base,

$$C_9 H_{14} N_2 = \left.\begin{matrix} (C_9 H_{10})'' \\ H_2 \\ H_2 \end{matrix}\right\} N_2,$$

fusible at 47°, the composition of which was determined by the analysis of the base itself and of the platinum-salt.

it differs in all its properties. I propose for this new compound the name of *paraniline*.

Paraniline forms a series of splendidly crystallized salts, the study of which proved that the above expression must be doubled, and that the true molecular value of this compound is represented by the formula

$$C_{12} H_{14} N_2.$$

The molecule of paraniline is capable of fixing either one or two equivalents of acid. The salts with one equivalent of acid are most readily obtained; they are of a light yellow colour, and their solution exhibits in an unusual degree the phenomenon of green fluorescence.

From a solution in concentrated hydrochloric acid a beautiful hydrochlorate crystallizes in transparent yellow six-sided plates, containing at 100°

$$C_{12} H_{14} N_2, 2HCl + H_2 O,$$

which on treatment with water are immediately converted into yellow needles sparingly soluble in water, more soluble in alcohol, insoluble in ether, containing

$$C_{12} H_{14} N_2, HCl + H_2 O$$

at 100°, and

$$C_{12} H_{14} N_2, HCl$$

at 115°.

The *platinum-salt* crystallizes in yellow difficultly soluble needles,

$$C_{12} H_{14} N_2, HCl, Pt Cl_2 \text{ (at 110°)}.$$

I have analysed only one *nitrate*, which crystallizes in short, yellowish, starlike-grouped needles, containing

$$C_{12} H_{14} N_2, HNO_3,$$

but two *sulphates*. One of them is readily obtained by dissolving paraniline in dilute sulphuric acid, when spherical aggregates of small needles are separated, easily soluble in water, less soluble in alcohol, which have the composition

$$C_{12} H_{14} N_2, H_2 SO_4.$$

Digested in aqueous solution for some time with an excess of paraniline, the sulphate just described assimilates a second equivalent of the base, a salt being formed very similar to the previous one, but containing after crystallization from alcohol

$$2(C_{12} H_{14} N_2), H_2 SO_4.$$

What is the constitution of this diamine? For the decision of this question I must wait till MM. Collin and Coblenz have kindly supplied me with a fresh quantity of their *queues d'aniline*. As yet I have only ascertained that iodide of ethyl gives rise to the formation of two ethylated bases, viz.,

$$C_{14}\,H_{18}\,N_2 = C_{12}\,H_{13}\,(C_2\,H_5)N_2,$$

the composition of which I have fixed by the analysis of the chloride, iodide, and platinum-salt, and

$$C_{16}\,H_{22}\,N_2 = C_{12}\,H_{12}\,(C_2\,H_5)_2\,N_2,$$

of which I have only examined the platinum-salt. The saline solutions of the ethylated bases are likewise remarkable for their fluorescent properties.

Chloride of benzoyl furnishes with paraniline small needles, insoluble in water but soluble in alcohol, of the formula

$$C_{19}\,H_{18}\,N_2\,O = C_{12}\,H_{13}(C_7\,H_5\,O)N_2.$$

Paraniline, it cannot be doubted, is the product of the action of heat upon aniline; and its formation suggests the existence of a series of similar bodies, similarly related to the other ammonias, which the progress of science cannot fail to reveal.

Experiments made in search of these bodies have hitherto been rewarded only by partial success. Nevertheless I have already succeeded in converting several ammonias into higher bases, and I intend to pursue the direction of research which is indicated by these results.

V. "Additional Observations on the Proximate Principles of the Lichens." By John Stenhouse, LL.D., F.R.S. (See p. 263.)

VI. "Letter to Professor Stokes, Sec.R.S., containing Observations made at Malta on a Planetary Nebula." By William Lassell, F.R.S. (See p. 269.)

December 18, 1862.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "Description of a new Specimen of *Glyptodon*, recently acquired by the Royal College of Surgeons of England." By Thomas Henry Huxley, F.R.S., Hunterian Professor of Comparative Anatomy at the College. Received November 14, 1862.

In the present brief preliminary notice I propose to give an account of the more remarkable features of the skeleton of a specimen of the extinct genus *Glyptodon*, recently added to the Museum of the Royal College of Surgeons.

The specimen was obtained in 1860, by Signor Maximo Terrero, on the banks of the River Salado, and was presented to the College by that gentleman, through the instrumentality of the late President of the College, J. F. South, Esq.

It arrived in England in an extremely broken and mutilated condition; but, by the exercise of great care and patience, Mr. Waterhouse Hawkins, to whom the President and Council of the Royal College of Surgeons entrusted the task of adjusting the scattered fragments, has succeeded in restoring to their natural condition the greater part of the vertebral column, the limbs, and much of the head. In the execution of this laborious undertaking Mr. Hawkins has had, from time to time, all the anatomical aid that Mr. Flower, the Conservator of the College Museum, and I could afford him; and the authorities of the College have finally entrusted me, as one of the Professors of the College, with the duty of describing the specimen.

This duty I propose to discharge by preparing a full description of the skeleton in a memoir to be presented (accompanied by a draught of the requisite illustrations) to the Royal Society. But as the preparation of such a memoir will require some time, I wish, at present, to lay before the Royal Society a preliminary account of those particulars in the structure of this animal which must interest anatomists in general as much as the special student of the fossil Edentata, in the hope that the notice may appear in the 'Proceedings' of the Society.

The mass of bony fragments which arrived from South America has afforded material for the reconstruction of the carapace, and of the following parts of the skeleton:—the anterior moiety of the skull with the entire palate; the mandible; some of the cervical, and the greater part of the dorsal, lumbar, sacral and coccygeal vertebræ, with vertebral and sternal ribs; the pelvis and the hind limbs; part of the scapula, and an entire fore limb. And there can be no doubt that all these remains belong to one and the same animal, as no duplicate bones have been discovered, nor any which there is the least reason to believe belong to a different individual. This circumstance gives a particular value to the present specimen, apart from the fact that, notwithstanding the researches of Professor Owen, of D'Alton, of Lund, and of Nodot, our knowledge of the structure of the anterior part of the skull, of the vertebral column and pelvis, and of the fore limb of *Glyptodon* and its immediate allies, is either nil or extremely imperfect. I now proceed to note the more important and the novel anatomical peculiarities which it reveals.

Of the *skull* the new specimen exhibits the anterior moiety, from the anterior boundary of the cranial cavity to the anterior end of the nasal bones, together with the almost entire bones of the face and the lower jaw; it thus furnishes a nearly complete supplement to the fragmentary cranium, consisting of the brain-case and the nasal bones, with the zygomatic processes, formerly described by Professor Owen as a part of *Glyptodon clavipes*, and now set up in the College Museum, together with a carapace, a tail, and a hind foot, as the typical example of that species*. In the form of the frontal bone, of the orbits, of the nasal bones, and of the zygomatic process, the skull of the new specimen agrees very closely with that of *Glyptodon clavipes*. From the slighter rugosity of the supraorbital region, the less development of the temporal ridges, and the fact that the nasal suture persists in the new specimen, I conceive it to have been a younger animal.

The anterior nasal aperture is trapezoidal, and narrower below

* The parts thus combined together were not found so associated, and the question may arise whether the skull, hind foot, and tail are really parts of the animal to which the carapace (on whose characters the species is founded) belonged. Provisionally I assume that they are. But so many difficulties are involved in the precise determination of the species of these extinct Armadillo-like Edentata, that for the present I leave the question open.

than above. The vomer is very thick and strong, and the turbinal bones are well developed. The premaxillæ, though small slender bones, enter largely into the lateral boundary of the nasal aperture. Inferiorly they are separated in the middle line by a narrow fissure, which runs back into the crescentic anterior palatine foramen.

The maxillary bones are extremely elongated; while the palatine bones are small in proportion to them, and, like the premaxillæ, are separated by a very narrow median fissure. The extreme length of the roof of the palate, formed by these three pair of bones, is 10 inches; while its width (between the inner edges of the teeth), though rather greater in front than behind, nowhere exceeds $1\frac{3}{4}$ inch. From before backwards the palate has a double curvature, being concave downwards from the anterior end of the premaxilla to the level of the third tooth, and convex thence to the end of the palate-bones; so that the posterior part of the palate has a very marked inclination upwards and backwards.

There were eight teeth in each maxilla, all trilobed, the longitudinal grooves separating the lobes being less marked in the anterior teeth.

The mandible is represented by the two horizontal rami, with the symphysis, the greater part of the right coronoid process, and the entire right condyle, together with many of the sixteen teeth. It very closely resembles the mandibles of *Schistopleuron gemmatum*, described by Nodot, but is wholly unlike the restored jaw of *Glyptodon clavipes* given (on the authority of a drawing) by Professor Owen*.

The articular surface is situated almost wholly upon the anterior surface of the condyle of the mandible, looking but very slightly upwards; it is transversely elongated, slightly concave from side to side, and convex from above downwards. In all these respects it furnishes a counterpart to the glenoid articular surface of the temporal bone of *Glyptodon clavipes*, already described by Professor Owen.

The length of the head of the present specimen, when entire, was probably not less than 13 inches. The greatest depth of the cranium, from the centre of the frontal bone to the middle of the

* The mandible of the Turin *Glyptodon*, mentioned at the end of this paper, is quite similar to that of the new specimen, and to that of M. Nodot's *Schistopleuron*.

palate is about 6 inches; the length of the mandible can hardly have been less than 12 inches.

Of the vertebral column, the greater part of the sacral and dorsal region, and some fragments of the cervical region, are preserved. The latter show that the atlas was distinct, but that the axis was anchylosed with one or two succeeding vertebræ, as in the Armadillos. The fifth and sixth cervical vertebræ were probably free, but no traces of them have been found. The anterior part of what remains of the rest of the vertebral column consists of a very broad flat bone, composed of three vertebræ firmly anchylosed together, and having their spinous processes represented by a short but very stout osseous knob, which projects upwards and backwards. Anteriorly, these anchylosed vertebræ exhibit on each side of the neural canal an articular facet with a convex surface, resembling a segment of a horizontal cylinder; posteriorly, articular surfaces of a similar character, but concave, are situated in corresponding positions.

Each side of this 'trivertebral bone' presents two large and deep articular cavities for the heads of ribs, fragments of which are still preserved. The anterior rib, remarkable for its stout and massive proportions, was undoubtedly the first; and this circumstance I believe gives a clue to the precise character of the vertebræ which are anchylosed together to form the trivertebral bone; for in the Armadillos the head of the first rib is fitted into a deep fossa, formed partly by the last cervical, and partly by the first dorsal vertebra. Furthermore, the body and transverse processes of the last cervical vertebra in the Armadillos present articular facets of an essentially similar character to those observable on the anterior face of the bone under description*; and, finally, the last cervical vertebra is practically immoveable upon the first dorsal in many Armadillos, while the two vertebræ are completely anchylosed together in the priodont Armadillo. I conceive, then, that this remarkable bone of the *Glyptodon* is formed by the anchylosis of the last cervical and first and second dorsal vertebræ.

Of the remainder of the spinal column thirteen consecutive vertebræ are preserved; and all of these were immoveably united into

* I may remark in passing, that all the cervical vertebræ of the Armadillos, from the third backwards, are articulated together by joints similar in principle of construction to those which connected together the trivertebral bone of *Glyptodon* with the vertebræ in front of and behind it.

one long continuous tunnel or arched tubular bridge of bone, a structure which is without a parallel among the Mammalian Vertebrata. Of these thirteen vertebræ, the four anterior are so completely anchylosed together, that the original lines of demarcation between them are hardly discernible. Persistent sutures separate the fourth from the fifth, and the latter from the sixth; but all trace of the primitive distinction of the sixth and seventh is lost. The other vertebræ are separated by sutures which become coarser and less close posteriorly. In all but the first, second, third, eleventh, and thirteenth vertebræ, the parts representing the vertebral centra are broken away; but where they persist, they are so similar that they were doubtless of similar form throughout. Each centrum is, in fact, a comparatively thin bony plate, so curved as to form a segment of a hollow cylinder of much larger diameter in the front than in the hinder vertebræ, the sides of which pass superiorly into the arches of the vertebræ.

The foremost vertebra of the thirteen is as broad as the posterior part of the 'trivertebral bone,' and presents a couple of convex articular facets which articulate with the lateral articular concavities described above in that bone. The vertebræ rapidly narrow, however, until the fourth is not more than three-fifths as wide as the first, while it is proportionately deeper; and this increase of depth relatively to width goes on until in the thirteenth vertebra the spinal canal is deeper than it is wide.

The spinous processes of these vertebræ are all broken short off; but sufficient remains of their bases to make the following points clear.

The spinous process of the first is almost obsolete, being a mere ridge sloping back towards the second, with which it is continuous. This appears to have been necessary to afford the requisite play for the knob of the trivertebral bone in its movements of flexion and extension on the rest of the spinal column.

The spinous process of the second vertebra was long and thick, and probably somewhat high. It appears to have been completely distinct from the third, which was thinner, and was anchylosed with its successors (as far as that of the twelfth vertebra inclusive) into a long continuous crest. The apices of the spinous processes may, however, have been distinct. So much as is left of the base of this

crest, shows that it was thickest at the sixth and seventh vertebræ (of the thirteen), and that it became thinner both anteriorly and posteriorly.

The spinous process of the twelfth vertebra, forming the termination of the crest, appears to have ended in a free, thin, but rounded edge. What remains of the spinous process of the thirteenth vertebra, on the other hand, thins off anteriorly to a natural edge, which is inclined upwards and backwards. Posteriorly the spinous process becomes very thick and stout, and appears to have had a considerable height. It ends in a fractured hinder margin.

The broad wing-like plates which represent the coalesced transverse processes of the first, second, and third vertebræ of the thirteen, exhibit distinct articular surfaces for the capitula and tubercula of ribs. Further back, the natural edges of the apophysial ridges are broken away, up to the eighth vertebra. Here they are entire on the left side and broken on the right; but, curiously enough, the broken processes are higher than the entire ones, so that the transverse processes in this region of the body must have been asymmetrically developed. The thirteenth vertebra presents peculiarities which could only be made intelligible by a lengthened description, and by figures. The contours of the articular processes become first distinctly traceable at the posterior part of the eleventh vertebra. They are better marked at the posterior part of the twelfth, and at the anterior part of the thirteenth vertebra.

The nervous foramina are not intervertebral, but pierce the arches of the vertebræ throughout the series. In the thirteenth the outlet of the foramen is separated, by a longitudinal bar of bone, into an upper and a lower division.

The posterior part of the thirteenth vertebra is much injured, and does not adjust itself naturally to the anterior end of that part of the lumbar region of the vertebral column (consisting of two vertebræ) which remains continuously anchylosed with the sacrum. One or two vertebræ may possibly be wanting, or even three; but I conceive the last to be the extreme limit of the deficiency*.

The great Priodont Armadillo has twenty dorso-lumbar vertebræ. If the *Glyptodon* had the same number, there would be three missing;

* Unless I greatly err in my interpretation of the photographs, these three missing vertebræ are preserved in the Turin *Glyptodon*.

for there are two dorsal vertebræ in the trivertebral plate, thirteen follow it, and two lumbar are anchylosed with the sacral, making altogether seventeen.

The 'sacrum,' composed of anchylosed lumbar, proper sacral, and coccygeal vertebræ, contains at fewest twelve, and perhaps thirteen vertebræ. The centra of the two lumbar vertebræ and of the two proper sacral vertebræ which follow them are preserved. They are thin and broad plates, flat above and slightly concave below, exhibiting a most marked contrast with the half-cylinder of the hindermost of the thirteen dorsal vertebræ above described. It would seem to require the interposition of at least two, if not three, vertebræ to effect the transition of the one form of centrum into the other.

The last coccygeal is the only vertebra among all those preserved the centrum of which exhibits characters at all like those of an ordinary mammal, its terminal face being a very broad oval, slightly concave, disk. The centrum of the penultimate coccygeal is much flatter and narrower; and this flattening and narrowing predominates still more in the antepenultimate and that vertebra which lies before it, or the fourth from the end. From this point to the two anterior sacrals the floor of the vertebral canal is completely broken away, but there can be no doubt that the centra were represented by a thin bony plate.

The line of the centra of the coccygeal vertebræ forms a very marked arch behind the two sacral vertebræ, whose centra form a nearly horizontal floor; while the dorso-lumbar vertebræ (including the trivertebral bone) form a second arch, flatter than the first.

The spinous processes of all these lumbo-sacro-coccygeal vertebræ, up to the fourth from the end inclusively, are anchylosed together in a long and strong osseous crest, broad and extremely rugose above, eight inches high in front, but slowly diminishing as it follows the curve of the centra posteriorly to five inches.

The spinous process of the penultimate coccygeal vertebra is very thick, but is broken short off. It was probably not less than 4 inches high, and afforded a middle point of support for the carapace between the ischial protuberances. The sides of the median crest, and of the two vertebræ which appear to constitute the true sacrum, are anchylosed firmly with nearly the whole of the inner edge of the vast ilium. Behind these the vertebræ seem to have been devoid of

transverse processes, as far as the fourth from the end. But the antepenultimate had a long and slender transverse process on each side; the penultimate has an equally long but much stouter process, while the last coccygeal vertebra has transverse processes of no less length, and extremely stout.

The expanded distal ends of these processes unite with one another, and with the inner surfaces of the greatly expanded ischia.

The ilia are immense quadrate bones, slightly concave anteriorly and posteriorly, with their planes so directed as to form rather less than a right angle forwards with the vertebral column. The crest of each iliac bone is thick, expanded, and rugose, and so arched as evidently to have afforded attachment and support to the carapace; which therefore rested directly, partly on the three transversely disposed pillars afforded by the coccygeal vertebræ and the two ischia, partly on the longitudinally arched crests of the sacrum and of the thirteen dorsal or dorso-lumbar vertebræ, and partly on the second great transverse support yielded by the arched crests of the ilia. Apart from their anchylosis, the whole of the parts named must have been practically fixtures in consequence of this arrangement of the carapace; and the only moveable parts of the vertebral column must have been the tail (of which unfortunately no portion has been found in the present specimen), posteriorly moveable on the last coccygeal vertebra,—the trivertebral bone with its two pair of ribs, capable of an up-and-down motion on the foremost of the thirteen vertebræ,—and then the cervicals, more or less moveable upon the anterior part of the trivertebral bone and upon one another.

I am not aware of the existence of any mammal in which the vertebral column presents characters of a similar singularity.

The mobility of the rib-bearing trivertebral bone, by a hinge-joint upon the rest of the vertebral column, is peculiarly anomalous. However, if, as appears to have been the case, the heads of the ribs attached to this bone were incapable of movement, and the first rib was furthermore directly anchylosed with the sternum, respiration must have been carried on entirely by the diaphragm, if the anterior dorsal vertebræ had been immoveable on the posterior ones. The hinge-like movement of the trivertebral bone, on the other hand, by permitting the ribs and sternum to describe a longitudinal arc alternately downwards and forwards, and upwards and backwards, would

allow of a most efficient bellows-action of the thorax, similar in principle to that effected by the ordinary movements of the ribs.

The trivertebral bone is about	6	inches long.
The thirteen vertebræ along their convexity ..	$29\frac{1}{2}$	,,
The sacrum	$35\frac{1}{2}$	,,
If three lumbar vertebræ are wanting allow ..	9	,,
	80	

Judging by the analogy of the Armadillos with which the *Glyptodon* presents such close resemblance, and from the shortness of such cervical vertebræ of *Glyptodon* as can be reconstructed, the neck did not exceed in length $\frac{1}{10}$th of the length of the vertebral column from the first dorsal to the last coccygeal. That would give 8 inches for the neck, and would give a grand total for the spinal column, exclusive of the tail, of 88 inches, or 7 feet 4 inches. The length of the carapace of *Glyptodon clavipes* in the Museum of the Royal College of Surgeons is 5 feet 7 inches.

The carpus of *Glyptodon* is in some respects very like that of *Dasypus sexcinctus,* but it consists of eight bones instead of seven, the trapezium and trapezoid being perfectly distinct, instead of forming a single bone, as in *Dasypus.* The scaphoid articulates with the os magnum, and the cuneiform with a metacarpal, as in *Dasypus.* But it is not a little remarkable that, whereas in *Dasypus* it is the fifth metacarpal whose proximal end partially articulates with the cuneiform, in *Glyptodon* the corresponding bone articulated wholly with the cuneiform, and not with any of the distal row of carpal bones. The metacarpal articular end of that bone is, in fact, divided into two facets—an inner, larger, which articulates with part of the proximal end of the fourth metacarpal, and an outer, smaller, which is appropriated by the proximal end of the fifth metacarpal.

That the cuneiform should articulate with two metacarpal bones, and that the unciform should not articulate with the fifth metacarpal at all, are very remarkable peculiarities of the wrist of *Glyptodon.*

The pisiform is a large curved bone, the proximal end of which articulates by a large facet with the ulna, and by a small one with a facet on the palmar aspect of the cuneiform. It closely resembles the same bone in Armadillos.

The trapezium and trapezoid, taken together, have a form closely

resembling that of the single trapezio-trapezoid of *Dasypus*. The trapezium possesses only a very small double articular facet on its palmar face. If this gives support to a metacarpal, it must have been very small; and as at present neither it nor any of the hallucal phalanges have been discovered, it is possible the pollex may have been altogether rudimentary. In any case the pollex must have been so much smaller and more slender in proportion than that of *Dasypus,* that the animal must have had a practically tetradactyle fore foot.

The second metacarpal is the longest of all which have been discovered, but is not quite so thick as the third. Its proximal end articulates with the trapezium, trapezoid, and magnum.

The third metacarpal, an almost cuboidal bone, but broader than long, articulates with the magnum, the cuneiform, and the adjacent metacarpals.

The fourth metacarpal, still shorter and broader in proportion, articulates with the unciform and cuneiform, and with the adjacent metacarpals.

The fifth metacarpal has not been found. The two proximal or first and second phalanges are very short, broad, discoidal bones in the second and in the third digits; and the second, which alone exists in the fourth digit, has the same character. The proximal phalanges of the fifth digit have not been found.

The distal or third phalanx is a broad bone, squarely truncated at the extremity, and longer than the rest of the digit, in the second, third, and fourth, and presumably in the fifth digit. Each of these phalanges is thicker on one side than on the other, so that the upper surface, which is convex from side to side, and also from before backwards, slopes from the thick towards the thin edge.

The distal phalanx of the second digit has its thick edge on its ulnar side, but all the others have their thick edges radial. The distal phalanx of the fifth digit is more pointed, smaller, and thicker in proportion than the others.

The hind foot is quite normal in structure, possessing five toes and the regular number and disposition of tarsal, metatarsal, and phalangeal bones. The third or middle digit is the longest, and its distal phalanx is the longest of all. It is nearly square, and its outer and inner edges are almost equally thick. The distal phalanges of

the other toes are all thicker on the side turned towards the middle toe. That of the second toe is almost as square as that of the third; but the distal angles of that of the third and fourth are bevelled off on the fibular side, while the terminal phalanx of the hallux is similarly bevelled off upon the tibial side. The metatarsal bones have the same thick prismatic form, and the proximal phalanges the same discoidal character as in the fore foot.

The calcaneal process is directed outwards at an angle of 45° from the axis of the foot, and must have been much raised in the natural position.

While the work of restoration, whose results have just been briefly detailed, was going on, we learned from Dr. Falconer that a nearly entire specimen of a *Glyptodon* was exhibited in the Museum at Turin. An application was at once made to the authorities of the Museum for information, and, if possible, for photographs of this skeleton, and was responded to with the most obliging readiness.

These photographs of a skeleton in some respects more, in others less perfect than that of the College, have confirmed the conclusions already arrived at in the most satisfactory manner; and I trust before long to be in possession of descriptive details of parts of this specimen which are wanting in our own, and which will enable me to complete the anatomy of the skeleton of the gigantic extinct Armadillo.

II. "On the Relation of Aqueous Vapour to Radiant Heat." By John Tyndall, F.R.S. &c. Received November 20, 1862.

(Abstract.)

The object of this paper is to prove to meteorologists that they may apply, without misgiving, the results which the author has already announced, regarding the relation of aqueous vapour to radiant heat. The author describes new experiments made with dry and humid air, first, with an experimental tube stopped by plates of rock-salt; secondly, with an open experimental tube; and thirdly, with an arrangement in which both the plates and the tube were abandoned,

dry air being caused to displace moist, and moist air dry, in the open atmosphere. He considers and removes objections, and points out the bearing of his experiments on various questions in meteorology. The formation of cumuli and the cause of the tropical rains are considered; the effect which the absence of aqueous vapour must have upon climate is pointed out; and the *à priori* conclusions to be drawn from the experiments are shown to agree with observation. Reference is made to anomalies of observation which have been hitherto unexplained, but which admit of easy solution by reference to the radiant and absorbent power of aqueous vapour. The author endeavours to supplement the views hitherto entertained regarding the action of mountain masses as condensers of the atmospheric moisture. He accounts for the enormous radiation observed at great elevations, and concludes by showing the possible bearing of his results on the theory of "*Serene*" and of hail.

III. "Distribution of the Surface of the Third Order into Species, in reference to the absence or presence of Singular Points, and the reality of its Lines." By Dr. Schläffle, Professor of Mathematics in the University of Berne. Communicated by Arthur Cayley, Esq. Received December 18, 1862.

(Abstract.)

The theory of the 27 lines on a surface of the third order is due to Mr. Cayley and Dr. Salmon; and the effect as regards the 27 lines of a singular point or points on the surface, was first considered by Dr. Salmon in the paper "On the triple tangent planes of a surface of the third order," Camb. and Dub. Math. Journ. t. iv. pp. 252–260 (1849). The theory as regards the reality or non-reality of the lines on a general surface of the third order, is discussed in Dr. Schläffle's paper, "An attempt to determine the 27 lines, &c.," Quart. Math. Journ. t. ii. pp. 56–65, and 110–120. This theory is reproduced and developed in the present memoir under the heading, I. General cubic surface of the third order and twelfth class; but the larger part of the memoir relates to the singular forms which are here first completely enunciated, and are considered under the headings II.,

III. &c. to XXII., viz. II. Cubic surface with a proper node, and therefore of the tenth class, &c., down to XXII. Ruled surfaces of the third order. Each of these families is discussed generally (that is, without regard to reality or non-reality), by means of a properly selected canonical form of equation; and for the most part, or in many instances, the reciprocal equation (or equation of the surface in plane-coordinates) is given, as also the equation of the Hessian surface and those of the spinode curve; and it is further discussed and divided into species according to the reality or non-reality of its lines and planes. The following synopsis may be convenient:—

I. General cubic surface, or surface of the third order and twelfth class. Species I. 1, 2, 3, 4, 5.

II. Cubic surface with a proper node and therefore of the tenth class. Species II. 1, 2, 3, 4, 5.

III. Cubic surface of the ninth class with a biplanar node. Species III. 1, 2, 3, 4.

IV. Cubic surface of the eighth class with two proper nodes. Species IV. 1, 2, 3, 4, 5, 6.

V. Cubic surface of the eighth class with a biplanar node. Species V. 1, 2, 3, 4.

VI. Cubic surface of the seventh class with a biplanar and a proper node. Species VI. 1, 2.

VII. Cubic surface of the seventh class with a biplanar node. Species VII. 1, 2.

VIII. Cubic surface of the sixth class with three proper nodes. Species VIII. 1, 2, 3, 4.

IX. Cubic surface of the sixth class with two biplanar nodes. Species IX. 1, 2, 3, 4.

X. Cubic surface of the sixth class with a biplanar and a proper node. Species X. 1, 2.

XI. Cubic surface of the sixth class with a biplanar node. Species XI. 1, 2.

XII. Cubic surface of the sixth class with a uniplanar node. Species XII. 1, 2.

XIII. Cubic surface of the fifth class with a biplanar and two proper nodes. Species XIII. 1, 2.

XIV. Cubic surface of the fifth class with a biplanar node and a proper node. Species XIV. 1.

XV. Cubic surface of the fifth class with a uniplanar node. Species XV. 1.

XVI. Cubic surface of the fourth class with four proper nodes. Species XVI. 1, 2, 3.

XVII. Cubic surface of the fourth class with two biplanar and one proper node. Species XVII. 1, 2, 3.

XVIII. Cubic surface of the fourth class with one biplanar and two proper nodes. Species XVIII.

XIX. Cubic surface of the fourth class with a biplanar and a proper node. Species XIX. 1.

XX. Cubic surface of the fourth class with a uniplanar node. Species XX. 1.

XXI. Cubic surface of the third class with three biplanar nodes. Species XXI. 1, 2.

XXII. Ruled surface of the third order and the third class. Species XXII. 1, 2, 3.

IV. "Experimental Investigations on the Stratified Appearance in Electrical Discharges."—"Effect obtained by varying the Resistance." By John P. Gassiot, F.R.S. Received December 11, 1862.

1. In the 'Proceedings of the Royal Society,' May 26, 1859, I have stated that, "on attaching the terminals of my water-battery (Phil. Trans. 1844, p. 39) to the wires of a carbonic acid vacuum-tube, inserted about 2 inches apart, I obtained a stratified discharge similar to that of an induction coil."

2. The battery remained as it was originally constructed, consisting of 3520 pairs of copper and zinc cylinders inserted in glass cells. As the rain-water with which each cell had been from time to time filled evaporated, they were again charged: this process of evaporation and recharging continuing for several years, during this lengthened period the battery was three or four times cleaned by dusting and wiping the cells, boards, and slips of glass on which the cells rested; but the constant deposition of dust and moisture had so far reduced the static effects of the battery, that this year it would scarcely elicit a spark of about $\frac{1}{5000}$th of an inch in air between the plates of my

micrometer electrometer (Phil. Trans. 1839), and I had therefore determined on taking the entire battery asunder, as well as in order to attach fresh zincs to the copper cylinders.

3. Previous to undertaking this somewhat tedious and troublesome process, it occurred to me to try what effect would be obtained from the discharge of the battery in carbonic acid vacua, by merely recharging it with brine in lieu of rain-water: the result of this experiment was such as to induce me for the present to forego my intention of having new zincs, particularly as a very large number of them were found to be much less oxidized than I had expected, and rather to turn my attention to some improved mode of insulation.

4. To accomplish this, the zincs were cleaned, and the old pieces of string with which they were kept from metallic contact with the copper cylinders were removed and replaced with new. The wood trays, on which the battery is placed, were cleaned, and carefully covered with a thick coating of shell-lac varnish, as were also the glass cells, the latter having been first heated in a sandbath in order to withdraw all trace of moisture before the varnish was applied.

5. On each tray slips of window-glass, also coated with shell-lac, were fixed *edge*ways, forming a kind of rail on which the cells were placed: the glass vessels being conical at the base, the strips of narrow glass presented little more than four points of shell-lac on which each cell rested. Those zincs which were found to be much oxidized were placed aside; and ultimately *three batteries* were completed, each of 1120 pairs of plates, forming, when connected with each other, *one* battery of 3360 cells: each cell was carefully charged with a saturated solution of common salt and water, and the trays, when placed on the racks, were separately insulated by resting on pieces of ebonite.

6. It will be observed that I have reduced my battery from 3520 to 3360 pairs of elements; the tension, as shown by the spark discharge, was nearly the same as when it was originally charged; but I have observed, after the battery has rested for a short time, the first discharge between the wire terminals in air appears to be more dense, presenting the same appearance as the discharge of a weakly-charged Leyden jar. The purport of this communication is not, however, to describe the general effects obtained by the

battery, many of which require more time for verification than I at present have at my disposal, but to lay before the Royal Society certain novel results which I have obtained, and which I hope may tend to elucidate, and possibly assist in explaining, the phenomena of what is termed the stratified electrical discharge.

7. I continue the practice I originally adopted of numbering my vacuum-tubes *: during the progress of my experiments, I found that the discharge of the battery was much more sensible to the slightest variation of the state of tension in each of these tubes, than that of the induction coil; the sudden disruption in the discharge of the latter presents greater obstacles to the more careful study of the phenomena than is offered by the direct discharge of the battery.

8. I soon ascertained that in some of my vacuum-tubes I was enabled to study the action of the discharges under peculiarly favourable conditions. I anticipate that these conditions may be still further improved; but, from the results I have already obtained, I venture the opinion that it may be doubtful whether the true theory of electricity as developed by the voltaic battery will be correctly explained, unless by carefully experimenting with batteries satisfactorily insulated. The battery I have described, with the improved method of obtaining vacua, first suggested to me by Dr. Frankland, offered me the opportunity of examining the discharge under conditions heretofore unknown.

9. The vacuum-tubes in which the experiments were made, and which relate to the subject of this communication, were Nos. 70, 248, 315, 319, 320, 324. These I shall refer to in the order of the experiments.

No. 248 (fig. 2 in woodcut) is about $2\frac{1}{2}$ inches long, 1 inch diameter: to the platinum wires small balls of coke are attached, about $1\frac{1}{2}$ inch apart, the wires being protected inside the vacuum, as far as the balls, by glass tubing.

No. 70 (described Phil. Trans. 1859, p. 151) is 14 inches long, with platinum wires 12 inches apart: a small glass bulb containing crystals of iodine was placed in this tube before it was charged with carbonic acid; and after the vacuum had been obtained,

* Phil. Trans. 1859, p. 137: the tubes were marked with consecutive numbers, a note being taken of each as it was finally sealed.

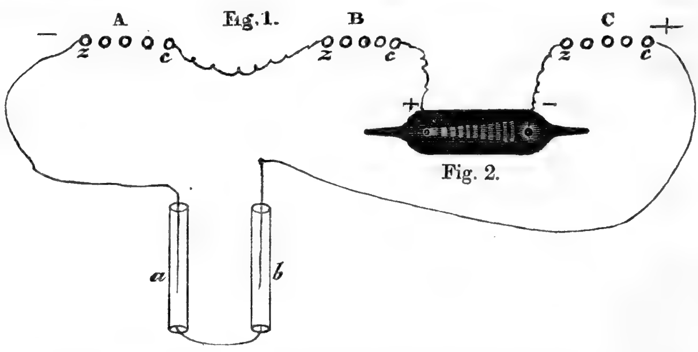
the glass bulb was broken. This tube consequently contains vapour of iodine.

No. 319 (Plate I.) is 20 inches long, 4 inches diameter: one terminal consists of aluminium, cup-shaped, about 3 inches diameter; the other a wire of the same metal. There is about $14\frac{1}{2}$ inches between the cup and the wire.

No. 320 is 3 inches long, 1 inch diameter: very thin platinum wires, $\frac{5}{8}$ths of an inch in length from the part protected by glass tube, are placed $1\frac{1}{4}$ inch apart from point to point in the tube.

No. 315 and No. 324 (Plates I. and II.) are about 5 inches long, with two balls of aluminium $\frac{3}{8}$ths of an inch diameter, and 3 inches apart: the balls are attached to platinum wires, these wires being also, as in No. 248, hermetically sealed and protected with glass tubing.

The vacuum in each of the tubes was obtained by the carbonic acid process.



10. Fig. 1 represents the general arrangement of the apparatus: A, B, C, the three batteries (§ 5); *z* and *c* the zinc and copper terminals of each battery; the discharge of 1120 pairs, of 2240, or of 3360 can be thus separately examined. My general practice is to place the experimental tube between either one or the other of the batteries (fig. 2), the negative or zinc terminal of C being attached to one wire, and the copper or positive of B to the other. A and B are then connected, and the circuit is completed either by a resistance arrangement being attached to the zinc terminal of A and the copper terminal of C, or the circuit is closed by a wire. It was with the view of being enabled to vary the resistance at pleasure, that I

introduced the two tubes *a* and *b*, containing the distilled water, in the circuit. I ascertained that, by varying the depth to which the wires attached to the terminals of the battery are plunged in one or both of the tubes containing the water, the resistance could be regulated with great precision, and that it was immaterial in what part of the circuit the vacuum-tube or the resistance was introduced, provided the circuit is completed.

11. In proceeding to describe the experiments, I may premise that, in using the terms *intermittent* and *continuous* as applied to the discharge of the battery, I desire only to denote that when the discharge is examined by a vibrating or revolving mirror, in the *former* the appearance of a series of distinct discharges is plainly perceptible; in the *latter* this separation is not seen, but the discharge appears as a continuous light.

12. No. 248. In this tube, with 2240 series, luminous glows are observed on both balls, that on the negative being larger and more brilliant; in the dark discharge between the balls no trace of striæ can be seen; but at intervals a flash discharge takes place. The luminous glow on each ball appears as a continuous discharge; both glows at times flitter, attaching themselves sometimes on one, and then on the opposite sides of the balls; but even then they are not resolvable by the mirror.

13. When a resistance of about 3 inches in length, of distilled water placed in the glass tubes *a* or *b* (§ 10) (fig. 1), is introduced in the circuit, the discharge assumes the narrow stratified appearance which I described in a similar vacuum-tube*, fig. 2: the discharge is now intermittent, being separated by the revolving mirror: as the wire is depressed and the resistance thus reduced, the discharges when examined by the mirror are found to be quicker in succession, being less and less separated until we arrive at a point at which the discharge suddenly changes its character, appearing now as a continuous light. Gradually raising the wire, and thus increasing the resistance, the discharge becomes again stratified and intermittent, more or less as the resistance is increased or reduced.

14. No. 70. This tube contains vapour of iodine (§ 9): with the induction coil a luminous discharge is obtained, which exhibits very narrow striæ; with the battery of 3360 cells striæ are not observable,

* Proc. Roy. Soc. June 1860.

but luminous discharges are obtained, which are distinctly separated by the revolving mirror, and are consequently intermittent.

15. No. 319, with 2240 cells of the battery and a resistance introduced in the circuit: the discharge can, by careful manipulations, be modified so as to assume the appearance of a positive and negative discharge, impinging on and intermingling with each other without any dark space intervening (Plate I. fig. 3). Around the negative terminal the luminosity extends to the sides of the tube; from thence to the positive wire the discharge, as represented in the figures, takes place in a line of about 2 inches diameter, emitting a very faint light. The slightest variation in the resistance obtained by raising or depressing the connecting wire in the columns of distilled water (fig. 1) alters the appearance of the discharge to that represented in fig. 4, where the dark portion is clearly defined. As the resistance is reduced, the dark space increases by the positive discharge receding towards the wire, the negative becomes brighter and more clearly defined, at its termination an approach to stratifications is observed, until, as the resistance is further reduced, the discharge will suddenly assume the form (as in fig. 5) of two bright clouds,—the outer edges presenting a greenish-blue colour, crescent-shaped, clearly and sharply defined, of about $\frac{1}{8}$th of an inch diameter. The other portion of the clouds remains of a bright reddish purple, gradually deepening in colour in approaching the other edge, where it becomes less defined, leaving towards the next cloud a dark space. The same gradations of colour are observable in the inner cloud next the positive wire; but not so clearly or distinctly defined. The discharge is at this time continuous at intervals; but, without altering or apparently interfering with these striæ, sudden discharges take place: two or three similar cloud-like striæ will be observed at the positive wire; and at the same instant brilliant stratifications are visible, overlapping the negative (fig. 6)*.

With the 2240 series distinct sounds were heard in the tube; with the whole battery of 3360 series, the overlapping striæ would remain for several seconds, but the sounds were not appreciable until a magnet was presented near to, and in a line with, the overlapping striæ; the action of the magnet causes these striæ to spread along the surface of the glass tube, and in this state of the discharge the sounds were again audible.

* White tongue discharge (Phil. Trans. 1859, p. 140).

When sounds are heard, either with the lesser number of cells (2240) or when the overlapping striæ of the greater number are spread by the magnet, the discharge is resolved by the mirror, and as such is intermittent; but otherwise it is continuous.

Mr. Stewart, Director of Kew Observatory, was present when I made this experiment; he examined the separation of the discharge by the revolving mirror, and heard the sounds, under the conditions of the discharge which I have described.

16. No. 320. The discharge in this tube did not pass until the potash was heated, when a faint luminosity appeared, and immediately afterwards one, and then two, cloud-like striæ came from the positive wire, while round the negative a large brilliant glow was produced; as the discharge continued, the negative wire became red-hot. I have repeated this experiment many times with the same tube; platinum from the negative wire is deposited in a *lateral* direction, on the sides of the tube, as it would have been from the discharge of an induction coil.

17. No. 315 (Plates I. & II. fig. 7, &c.). With 3360 cells, the discharge in this tube is of a dazzling brilliancy, exhibiting 12 or 14 striæ (Dr. Faraday and Dr. Tyndall, who witnessed this experiment, counted 13); that nearest the negative ball, being truncated and of a pale-green colour, impinged on the luminous glow which surrounded that terminal (fig. 13).

With a resistance of the two columns of distilled water (fig. 1), each 18 inches in length, introduced in the circuit (§ 10), certain changes in the form and number of the striæ take place. Some of these I have endeavoured to represent by figs. 7, 8, 9, 10, 11, 19, and 13.

The wires attached to the terminals of the battery are placed inside the two tubes containing distilled water, connected with each other from the bottom; as soon as the wires touch the surface of the water, a faint luminous discharge is observed at each ball of the vacuum-tube. As one wire attached to the negative is slowly depressed, the two luminous discharges appear to travel towards or to attract each other; and at times I have noticed a portion of the positive luminosity to pass over and intermingle with the negative.

I tried the effect of a magnet on the discharge while in this state; but it was always extinguished *, and I could not obtain any satisfactory result.

* Proc. Roy. Soc. Jan. 1860.

Depressing the wire very gradually, the discharge assumes the form of fig. 7, the positive being sharply defined, the negative retaining much of its irregular termination, but each separated from the other by a dark interval of about one inch in length.

As the wire was further depressed in the water, the brilliancy of the positive and negative luminous glows increased; and when about 3 inches of one wire had been immersed in the water, a single clearly defined luminous disk burst forth from the positive, remaining steady and apparently fixed as in fig. 8.

As the wire was again further depressed, the luminous discharge at the positive slowly progressed along the tube until another bright disk appeared, remaining (as long as the wire was not further depressed) stationary as in fig. 9. At this time 13 inches of wire were in the water.

The resistance was again reduced by depressing the wire to 16 inches, when a third luminous disk was developed as in fig. 10; and at 18 inches depression, or the entire length of one column of water, a fourth disk was observed as in fig. 11.

In this state, while the four luminous disks were stationary, the wire attached to the positive terminal of the battery was depressed 4 inches; the luminous disks gradually closing on each other became more compressed, when a fifth was developed, fig. 12.

The luminous glow on the negative ball had gradually assumed a flattened surface towards the positive, appearing as a ring of light; depressing the wire to 7 inches in the water, the luminous disks closed but remained separate, and a 6th was observed; with 11 inches depression another, or the 7th, appeared, the negative glow increasing in brilliancy, retaining its flattened appearance. At this time, probably from the long-continued action, the power of the battery was reduced, and the 7th disk disappeared, retreating to and apparently absorbed by the luminosity on the positive ball; but on further depressing the wire, it was immediately reproduced; and on 15 inches insertion an 8th was obtained, the negative glow increasing in brilliancy, and the part nearest the positive presenting a still more flattened appearance.

Another clearly defined and separated disk of light was elicited, and then three or four came out in quick succession; the whole discharge now became unsteady and flittering, the luminous disks no longer remaining fixed or stationary.

18. From the first appearance of the luminous discharge in No. 315, until when thirteen or fourteen bright disks or separate striæ were observed, the discharge was not resolvable by a vibrating or a rotating mirror; with the full power of the battery, the disk nearest the negative was truncated and impinged on the glow which surrounds that ball (fig. 13): this truncated disk was also distinguishable by its pale green colour; those in its nearest contiguity had more or less a reddish tinge; the round negative glow was brilliant and of a bluish-white colour; minute bright scintillations emanated from the negative ball, while distinct luminous flash discharges took place through the striæ. On examining these intermittent discharges by two revolving mirrors, kindly lent me by Professor Wheatstone, they appeared stratified; but whether this did not arise from the passing of these discharges through the bright and dark portions of the continuous discharge, might have been considered doubtful, had I not in another tube observed a similar stratified discharge under more favourable circumstances. This tube, No. 324, is of the same form and dimensions as 315; on heating it with a spirit-lamp when it was in the circuit of the battery, the luminous discharge showed four clearly defined separated striæ, which remained fixed and steady in their relative positions; in this state momentary stratified discharges were observed at intervals of from five to ten seconds, these striæ assuming a conical form, as in Plate II. fig. 14.

I have observed somewhat similar intermittent discharges with the nitric-acid battery, possibly under more favourable conditions; and with a more extended series than I then used*, the true nature of this discharge may be ascertained.

19. The discharge from an electrical machine when passing through air in the dark, presents the well-known form of a brush at the end of the wire attached to the prime conductor, and of a star at the point of another wire attached to the rubber, or in connexion with the earth. I have shown† that if this discharge is allowed to pass through a vacuum-tube, stratifications will be obtained similar to those from an induction coil, and that the discharge of a Leyden jar, if passed through a wet string and a vacuum tube, is stratified‡; these

* Proc. Roy. Soc. March 1860. † Phil. Trans. 1858, p. 6, sect. 21.

‡ Phil. Trans. 1858, p. 15, sect. 53.

discharges are consequently identical, and only differ in their appearance according to the media through which they are passed.

20. In a former communication to the Royal Society*, I have also shown that the stratified discharge can be obtained by a single disruption of the primary current of an inductive coil, however long may be the vacuum-tube through which the discharge is passed. If no addition is made to the battery with which the primary wire of the apparatus is connected, or no alteration is made in the arrangement of the coil, so as to increase or diminish the intensity of the discharge, the stratifications will always present the same appearance and form, occupying the same spaces and positions in the vacuum-tube; but if any change is made so as to alter the intensity, then a correspondiug alteration will appear in the discharge, *the striæ assuming a different shape, and the bright and dark divisions occupying different positions.*

21. When a galvanometer, a vacuum-tube, and a solution of iodide of potassium are arranged so as to form a continuous circuit with the secondary coil of an inductive coil, not only is a luminous stratified discharge produced, but the needle of the galvanometer will be deflected, and iodine will be evolved by the induced *momentary* action; we thus obtain in this discharge all the indications and conditions of a true voltaic circuit.

In the *continuous* discharge of the battery we have the same indications as that of the *momentary* current of the closed circuit of an induction coil, and neither is resolvable by the rotating mirror.

The stratified discharge from a *single* disruption of the primary wire of the induction coil, and the continuous discharge of the voltaic battery, are therefore identical in their character.

22. With these preliminary observations, I now propose to examine the results obtained from the discharge of an extended series of the voltaic battery in vacua described in this communication.

1st. No. 248. The discharge under certain conditions is continuous, and under other conditions it becomes intermittent. These conditions are, that without any resistance introduced in the circuit, except that inherent in the battery, the discharge cannot be resolved by the rotating mirror, and so far may be considered as continuous; but when a certain given and described resistance is introduced in the

* Phil. Trans. 1858, p. 9, sect. 30.

circuit, the discharge becomes intermittent; with an increased resistance the number of discharges in any given time is reduced, the duration of such intermittent discharges being distinctly resolvable by the rotation or vibration of the plane of a mirror in which they can be reflected.

2nd. No. 70. In this very imperfect vacuum, containing vapour of iodine, the battery does not elicit any striæ, but by the revolving mirror the discharge is found to be intermittent. With the induction coil the discharge in this tube elicits clearly defined but very narrow striæ; from the coil we have a discharge of high intensity which elicits stratifications, although they are not attainable by the lower intensity of the battery.

3rd. No. 319. We obtain evidence of two distinct states of a discharge, of colour in the striæ, and, under certain conditions, of sound.

4th. In No. 320 we have experimental proof that in the more perfect vacuum the discharge will not pass, confirming my former result obtained with the coil (Phil. Trans. 1859, p. 156), that the presence of a certain amount of matter is indispensable, and that during the discharge heat is developed.

5th. In No. 315, under all conditions of resistance described, the discharge of the battery is stratified, but cannot be resolved by the revolving mirror: in this tube we are enabled to determine and regulate the number of striæ, to some extent alter their colour, to fix and determine their position, separating or closing up the dark space between the luminous disks, these changes being entirely due to the amount of resistance introduced in the circuit.

The form, or figuration of the striæ, and the positions they occupy in the vacuum-tube, appear by these experiments to depend upon two separate and distinct conditions:—

1st. The power or energy of the battery.

2nd. The state of tension of the highly attenuated matter through which the discharge is visible.

The striæ can be controlled, their number increased or reduced, and their places or positions in the tubes altered by the introduction of measurable amount of resistance in the circuit; and thus they appear to indicate the amount of force of tension which exists in a *closed* circuit of the battery, as the divergence of the gold leaves of an

electroscope denotes the evidence of tension *before* the circuit is completed.

In my former communications to the Royal Society I have alluded to the direction of a force in the induction discharge from the positive towards the negative (Phil. Trans. 1858, p. 16, sections 57, 58).

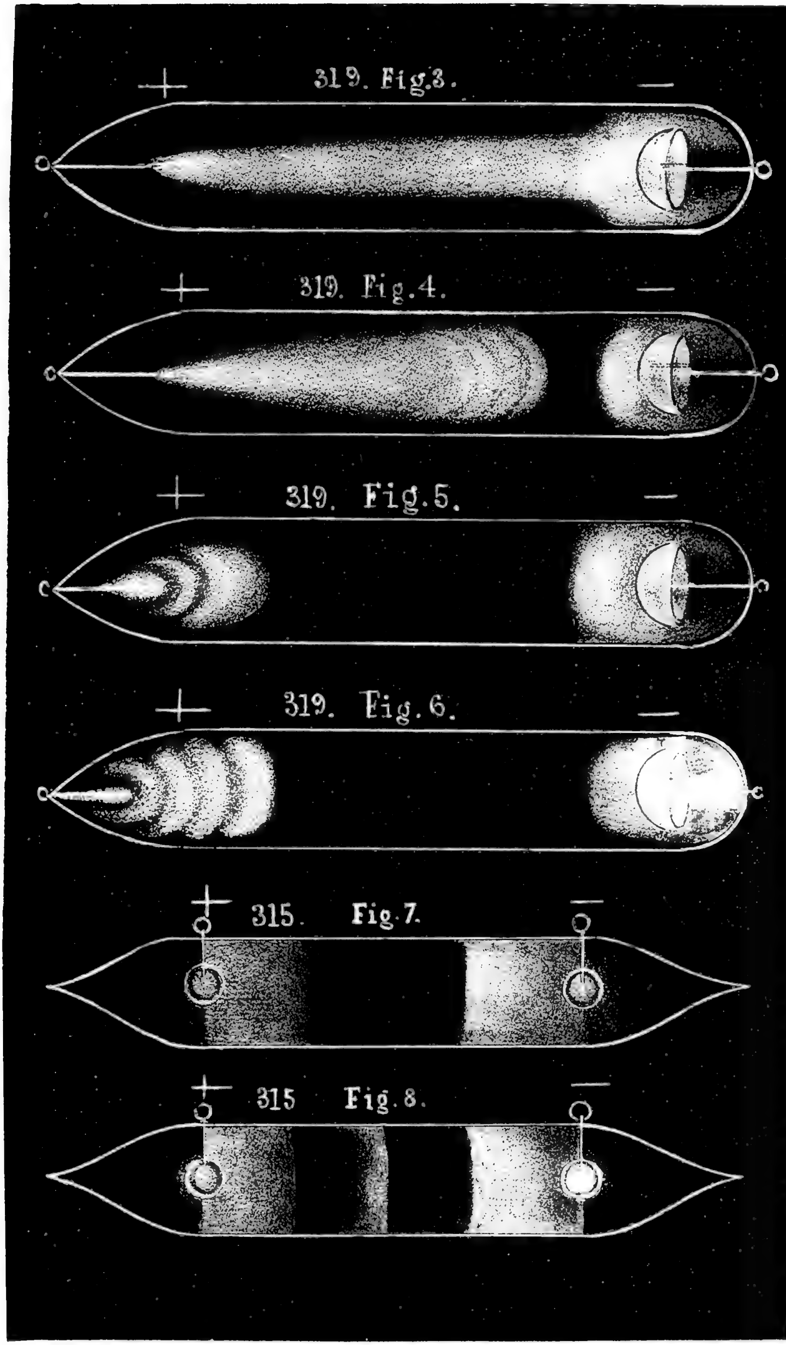
In 1859 I observed that there was also a tendency or indication of a force emanating from the negative wire (Phil. Trans. 1859, pp. 140, 142, 153, sections 68, 72, 99); the actual disruption of the particles from the negative terminal also indicates a force; and this disruption is as freely obtained by the continuous discharge of the battery (§ 16) as it is by the intermittent discharge of the induction coil.

I have always observed that with the lowest state of intensity with which the discharge can be obtained from an induction coil, the striæ are wider apart and the dark space between the positive and the negative is much extended; under some conditions of the discharge it is the negative, and not the positive, that assumes the dominant character.

The form of the striæ in the battery discharge, as observed in No. 315, figs. 7, 8, and 9, presents an appearance somewhat analogous with the stationary undulations which exist in a column of air when isochronous progressive undulations meet each other from opposite directions, and on the surface of water by mechanical impulses similarly interfering with each other.

May not the dark bands be the nodes of undulations arising from similar impulses proceeding from positive and negative discharges?

Or can the luminous stratifications which we obtain in a closed circuit of the secondary coil of an induction apparatus, and in the circuit of the voltaic battery, be the representation of pulsations which pass along the wire of the former and through the battery of the latter, impulses possibly generated by the action of the discharge along the wires?


+
319. Fig.3.
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319. Fig.4.
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319. Fig.5.
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319. Fig.6.
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315. Fig.7.
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315 Fig.8.
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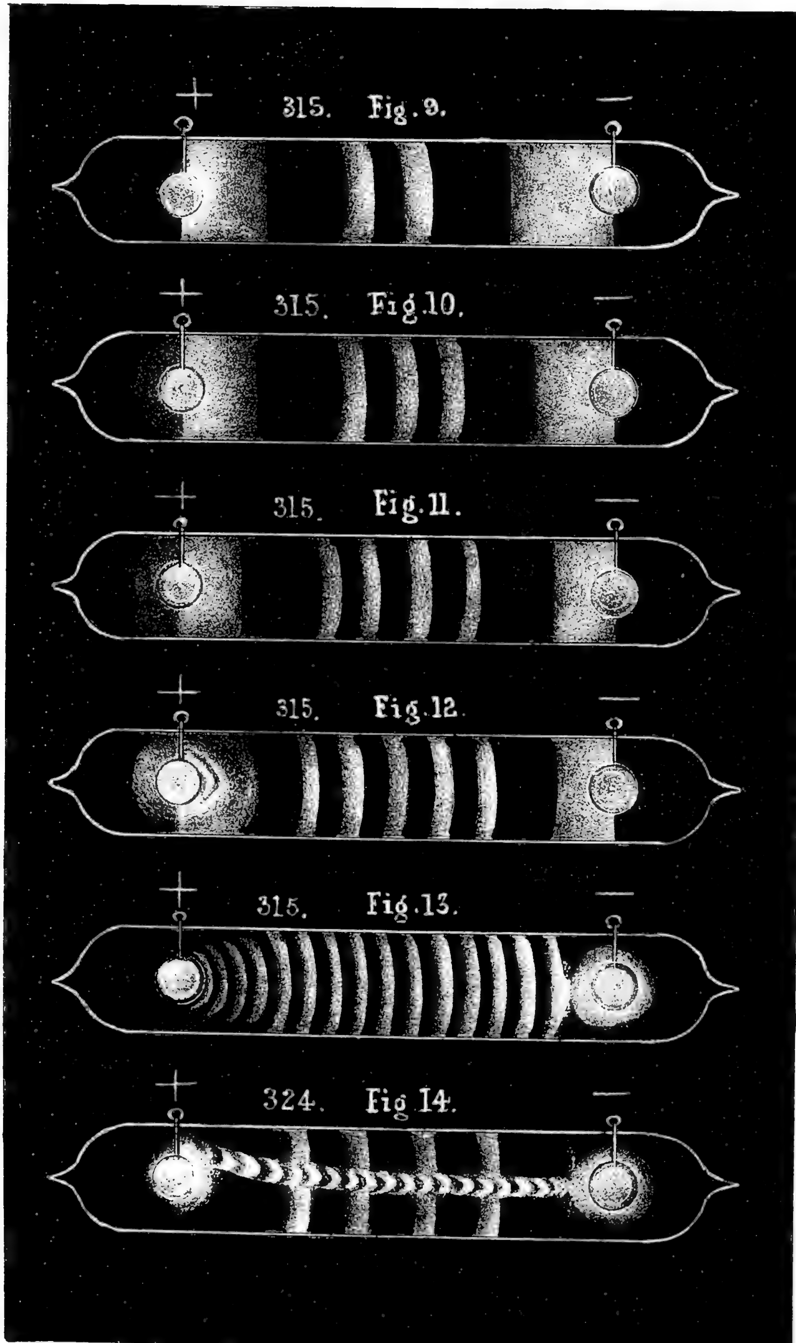


+ 315. Fig. 9. —
+ 315. Fig. 10. —
+ 315. Fig. 11. —
+ 315. Fig. 12. —
+ 315. Fig. 13. —
+ 324. Fig 14. —

January 8, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "Applications of the Theory of the Polyedra to the Enumeration and Registration of Results." By the Rev. THOMAS P. KIRKMAN, M.A., F.R.S., and Honorary Member of the Literary and Philosophical Societies of Manchester and Liverpool. Received November 29, 1862.

The following are a portion of my Tables of polyedra. They comprise all the 6-edra, 7-edra, and 8-edra, with their reciprocals, and all 9-edra of less than 17 edges. It is desirable that examples of results should be before the reader of my work on this theory, if it is so fortunate as to be read at all. More results can easily be added, if it is thought necessary, when the entire treatise is before the world.

The method of computation of these Tables turns to advantage a division of summits and reticulations not mentioned in the theory, as it would have abbreviated nothing, and would have added one more to complications already too numerous, and all inevitable.

Perfect summits and reticulations, that is, such as have no effaced effaceables, are *pyramidal, propyramidal,* or *metapyramidal.*

A *pyramidal* perfect summit or reticulation is one of which every effaceable, primary or secondary, is a base edge of a pyramid, by which the A-gonal base of a pyramid can be cut away in the process of reduction of the reticulation. Such a construction is made either by glueing together by their edges A-gonal, B-gonal, C-gonal, &c. pyramidal bases, the vertices being supposed to hang downwards, or by loading marginal triangles of plane reticulations with such bases so posited.

If a reticulation has no effaceables, it is merely a plane partitioned polygon, and the summit or edge which crowns it completes a polyedron without the employment of any solid charges. I call such a summit or edge *propyramidal.*

If, for one or more of the A-gonal, B-gonal, &c. bases of pyramids

about a pyramidal summit or edge, we substitute an A-gonal, B-gonal, &c. face of any polyedron which is not a pyramidal base, we have a perfect summit or edge of a solid of a greater number of edges, which may or may not have another symmetry. Such a summit or edge is *metapyramidal.*

The most expeditious method of computing the polyedra of N or fewer edges, is first to form and to crown all possible propyramidal and pyramidal perfect reticulations which can be reduced by effacement of effaceables to N or to fewer edges: these are to be registered in tables of perfect edges and summits, which show at a glance what pyramidal bases enter into the constructions registered. Having determined, by inspection of these tables and by effacements, the lower polyedra, we form tables of metapyramidal edges and summits, by merely conceiving the substitution of other A-gons, B-gons, &c. of solids thus far determined, for the A-gonal, B-gonal, &c. pyramidal bases. The edge (MN), or the p-ace, considered, is at once entered as an edge (MN), or as a p-ace, of a polyedron of more edges, in the metapyramidal tables.

Rules are easily laid down for the result of this conceived substitution, as to symmetry, signatures, and the tabular value (of enumeration). We thus escape the enormous toil of separately constructing and crowning the metapyramidal reticulations. These rules will be given in the *supplement of applications,* of which this abstract exhibits a few results.

I observe that a case is unprovided for in art. XLVII. of my second section, namely, the case of a zone which exhibits in some of its forms the symbol 0_p of an epizonal polar edge. Such a zone will of course occur about an amphigrammic, about an edrogrammic, or gonogrammic zoned axis. This polar epizonal is to be included, as part of the number h_{AA}, in the sinister of the equation first read, since this edge 0_p is generally the epizonal edge of a monozone A-gon, never of two different A-gons.

This XLVIIth article is sufficiently corrected by the effacement of the word *non-polar* in the 2nd, 9th, and 13th lines, and by writing *when* for *because* in the 7th line. So read, all cases are covered.

It may appear to the reader at first sight that the Table A, or at most the Tables A, B, and C, would comprise a sufficient solution of the problem of the polyedra. The truth is, that it is impossible to

determine the numbers in the Table A, without complete Tables A, B, C, D of inferior polyedra.

Registration of the **4**-*edron* 4-*acron.*

(*Vide* arts. XXXI. &c. of my memoir "On the Theory of the Polyedra," Phil. Trans. 1862.)

Table A.

One zoned tetrarchaxine, having principal polar triaces and triangles, and amphigrammic secondary axes (art. XXI.). The zone is $^4Z=\{2.1_p, 2.\mathbf{1}_p, \mathbf{0}_p 0_p\}$.

Table B.

Janal polar zoned edge:

$$(\mathbf{33})^{2a.d}_{ja.hom}0\mathbf{2}=1, \ {}^4Z=\{2.1_p, 2.\mathbf{1}_p, \mathbf{0}_{p_1}, 0_{p_1}\}.$$

Table C.

Zoned radical tetrarchipolar face:

$${}^{4}_{(3)}\mathbf{3}^{3mo}_{go.ed}\,1\mathbf{3}=1, \quad {}^4Z=\{2.1_p \ 2.\mathbf{1}_p, \mathbf{0}_p 0_p\}.$$

The 4 prefixed to **3** shows that it is a tetrarchaxine pole: the 3 suffixed shows that the summits of the polar triangle are triaces. It is only in the case of polar triangles that we require, for purposes of construction hereafter, an account of summits about a polar face.

Table D.

Polar zoned edge:

$$(\mathbf{33})^{2a.d}_{am.gr}0\mathbf{2}=1, \quad Z=Z'=\{2, \mathbf{2}, \mathbf{0}_p, 0_p\}.$$

This is the edge above recorded in Table B.

The summit reciprocal to the face in Table C is the one required to complete that Table. The reciprocal of any face is written by exchanging faces for summits, and zonal for epizonal edges, in all the signatures. Hence the summit of the 4-edron is

$$3^{3mo}_{go.ed}1\mathbf{3}=1, \quad Z=\{2.1_p, 2.\mathbf{1}_p, 0_p\mathbf{0}_p\}.$$

Registration of the 5-*edron* 5-*acron.*

Table A.

One 4-zoned monaxine heteroid, the 4-gonal pyramid.

Table C.

Zoned polar face:

$$\mathbf{4}^{4a.d}_{go.ed}1\mathbf{4}=1, \quad Z=\{1_p+2.1, \mathbf{1}_p, \mathbf{0}^{2.1}\},$$
$$Z'=\{1_p, \mathbf{1}_p+2.\mathbf{1}, 0^{2.1}\}.$$

Zoned polar summit:

$$4^{4a.d}_{go.ed}\mathbf{1}4=1, \text{ with the same zones.}$$

Zoned non-polar or monozone face:

$$\mathbf{3}^{mo}2\mathbf{4}=1, \quad Z=\{1, \mathbf{3}, 0^2\}.$$

Zoned summit:

$$3^{mo}\mathbf{2}4=1, \quad Z=\{3, \mathbf{1}, \mathbf{0}^2\}.$$

Table D.

Zonal edge:

$$(\mathbf{33})_{zo}1\mathbf{3}=1, \quad Z=\{3, \mathbf{1}, \mathbf{0}^2\}.$$

Epizonal edge:

$$(\mathbf{43})_{ep}0\mathbf{3}=1, \quad Z=\{1, \mathbf{3}, 0^2\}.$$

Registration of the 5-edron 6-acron.

Table A.

One 3-zoned monarchaxine, having an amphiedral principal axis terminated by triangles, and edrogrammic secondary axes. Its two zones are those next written.

Table B.

Janal zoned polar face:

$$_{(3)}\mathbf{3}^{3mo}_{amed}3\mathbf{4}=1, \quad Z=\{2.1, 2.\mathbf{1}_p+\mathbf{1}_{p'}, \mathbf{0}_{p'}, 0^{2.1}\},$$
$$Z'=3\{\,..\; \mathbf{1}_{p'}, 0_p\}.$$

Table C.

Polar zoned faces:

$$_{(3)}\mathbf{3}^{m)}_{amed}3\mathbf{4}=1, \; Z=\{2, \quad \mathbf{1}_p+\mathbf{2}, \quad \mathbf{0}, \quad 0^2\};$$
$$\mathbf{4}^{2ag}_{edgr}2\mathbf{4}=1, \quad Z=\{2.1, \; \mathbf{1}_p+2.\mathbf{1}, \; \mathbf{0}_p, \; 0^{2.1}\},$$
$$Z'=\{\,.. \quad \mathbf{1}_p+2.\mathbf{1}, \; 0_p, \; 0^{2.1}\}.$$

Table D.

Polar zoned edge:

$$(\mathbf{44})^{2a.d}_{edgr}0\mathbf{3}=1, \quad Z=\{2.1, \; \mathbf{1}_p+2.\mathbf{1}, \; \mathbf{0}_p, \; 0^{2.1}\},$$
$$Z'=\{\,.. \quad \mathbf{1}_p+2.\mathbf{1}, \; 0_p, \; 0^{2.1}\}.$$

Epizonal edge:

$$(\mathbf{34})_{ep}1\mathbf{3}=1, \quad Z=\{2, \; 3, \; \mathbf{0}, \; 0^2\}.$$

We have not here registered the summits of the 5-edron 6-acron, as they are merely the reciprocals of the faces of the 6-edron 5-acron. For a like reason we shall avoid the trouble of registering hereafter any summits.

Registration of the 6-edron 5-acron.

Table A.

One 3-zoned monarchaxine, having an amphigonal principal axis, terminated by triaces, and gonogrammic secondary axes, with the zones first written below in Table D.

Table C.

Zoned non-polar face :

$$\mathbf{3}^{mo}\mathbf{25}=1, \quad Z=\{3,\ \mathbf{2},\ 0,\ \mathbf{0}^2\}.$$

Table D.

Polar zoned edge :

$$(\mathbf{33})^{2a.d}_{gogr}\mathbf{14}=1, \quad Z=\{1_p+2.1,\ 2.\mathbf{1},\ 0_p,\ \mathbf{0}^{2.1}\},$$

$$Z'=\{1_p+2.1,\ \ldots,\ \mathbf{0}_p,\ 0^{2.1}\}.$$

Zonal edge :

$$(\mathbf{33})_{zo}\mathbf{14}=1, \quad Z=\{3,\ \mathbf{2},\ 0,\ \mathbf{0}^2\}.$$

Registration of the 6-edra 6-acra.

Table A.

1. One 5-zoned monaxine heteroid, whose gonoedral axis is terminated by a pentace and a pentagon, having the zone first below written.

2. One 2-ple zoneless monaxine heteroid, having an amphigrammic axis.

Table C.

Polar face :

$$\mathbf{5}^{5mo}_{goed}\mathbf{15}=1, \quad Z=\{1_p+1,\ \mathbf{1}_p+\mathbf{1},\ \mathbf{0},\ 0\}.$$

Zoned non-polar face :

$$\mathbf{3}^{mo}\mathbf{35}=1, \quad Z=\{2,\ \mathbf{2},\ \mathbf{0},\ 0\}.$$

Asymmetric faces :

$$\mathbf{3}_{as}\mathbf{35}=\mathbf{2}, \quad \mathbf{4}_{as}\mathbf{25}=1.$$

Table D.

Zoneless polar edges :

$$(\mathbf{44})^2_{amgr}\mathbf{04}=1, \quad (\mathbf{33})^2_{amgr}\mathbf{24}=1.$$

Zonal edge :

$$(\mathbf{33})_{zo}\mathbf{24}=1, \quad Z=\{2,\ \mathbf{2},\ 0,\ \mathbf{0}\}.$$

Epizonal edge :

$$(\mathbf{35})_{ep}\mathbf{04}=1, \quad Z=\{2,\ \mathbf{2},\ 0,\ \mathbf{0}\}.$$

Asymmetric edges :

$$(\mathbf{34})_{as}\mathbf{14}=3, \quad (\mathbf{33})_{as}\mathbf{24}=1.$$

Registration of the 7-edra 6-acra.

Table A.

1. One 2-zoned monaxine heteroid, having an edrogrammic axis, and the zones first below written in Table C.

2. One monozone, having the zone $Z=\{2, \mathbf{3}, \mathbf{0}, 0^2\}$.

Table C.

Polar zoned face:

$$\mathbf{4}^{2di}_{edgr}\mathbf{26}=1, \quad Z=\{2.1, \mathbf{1}_p+2.\mathbf{1}, 0_p\},$$
$$Z'=\{2.2, \mathbf{1}_p, \mathbf{0}_p, \mathbf{0}^{2.1}\}.$$

Zoned non-polar faces:

$$\mathbf{4}^{ag}\,\mathbf{26}=1, \quad Z=\{2, \mathbf{3}, \mathbf{0}, 0^2\};$$
$$\mathbf{3}^{mo}\mathbf{36}=2, \quad Z=\{2, \mathbf{3}, \mathbf{0}, 0^2\};$$
$$\mathbf{3}^{mo}\mathbf{36}=1, \quad Z=\{2, \mathbf{3}, 0\}.$$

Asymmetric faces:

$$\mathbf{3}_{as}\mathbf{36}=3.$$

Table D.

Polar zoned edge:

$$(\mathbf{33})^{2a.d}_{edgr}\mathbf{25}=1, \quad Z=\{2.1, \mathbf{1}_p+2.\mathbf{1}, 0_p\},$$
$$Z'=\{2.2, \mathbf{1}_p, \mathbf{0}_p, \mathbf{0}^{2.1}\}.$$

Zonal edges:

$$(\mathbf{33})_{zo}\mathbf{25}=1, \quad Z=\{4, \mathbf{1}, \mathbf{0}^3\};$$
$$(\mathbf{33})_{zo}\mathbf{25}=1, \quad Z=\{2, \mathbf{3}, \mathbf{0}, 0^2\}.$$

Epizonal edges:

$$(\mathbf{34})_{ep}1\mathbf{5}=2, \quad Z=\{2, \mathbf{3}, \mathbf{0}, 0^2\}.$$

Asymmetric edges:

$$(\mathbf{33})_{as}\mathbf{25}=4, \quad (\mathbf{34})_{as}1\mathbf{5}=2.$$

Registration of the **6**-*edra 7-acra.*

Table A.

1. One 2-zoned monaxine heteroid, with gonogrammic axis, having the zones first read in Table D below.

2. One monozone, with the zone $Z=\{3, \mathbf{2}, 0, \mathbf{0}^2\}$.

Table C.

Zoned non-polar faces:

$$\mathbf{5}^{mo}\,\mathbf{25}=1, \quad Z=\{3, \mathbf{2}, 0, \mathbf{0}^2\};$$
$$\mathbf{4}^{ag}\,3\mathbf{5}=1, \quad Z=\{1, \mathbf{4}, 0^3\};$$
$$\mathbf{4}^{di}\,3\mathbf{5}=1, \quad Z=\{3, \mathbf{2}, 0\};$$
$$\mathbf{3}^{mo}4\mathbf{5}=1, \quad Z=\{1, \mathbf{4}, 0^3\};$$
$$\mathbf{3}^{mo}4\mathbf{5}=1, \quad Z=\{3, \mathbf{2}, \mathbf{0}^2, 0\}.$$

Asymmetric faces :

$$4_{as}35=1, \quad 3_{as}45=1.$$

Table D.

Zoned polar edge :

$$(44)^{2a.d}_{gogr}14=1, \quad Z=\{1_p+2.1,\ 2.\mathbf{1},\ \mathbf{0}_p\},$$
$$Z'=\{1_p,\ 2.\mathbf{2},\ 0_p,\ 0^{2.1}\}.$$

Zonal edges :

$$\left.\begin{array}{l}(33)_{zo}34=1\\(44)_{zo}14=1\end{array}\right\} Z=\{3,\ \mathbf{2},\ 0,\ \mathbf{0}^2\}.$$

Epizonal edges :

$$(35)_{ep}14=1, \quad Z=\{3,\ \mathbf{2},\ 0,\ \mathbf{0}^2\};$$
$$(34)_{ep}24=1, \quad Z=\{1,\ \mathbf{4},\ 0^{\bar{3}}\}.$$

Asymmetric edges :

$$(53)_{as}14=1, \quad (34)_{as}24=3;$$
$$(45)_{as}04=1, \quad (44)_{as}14=1.$$

Registration of 7-edra 7-acra.

Table A.

1. One 6-zoned monaxine heteroid, having its gonoedral axis terminated by a hexace and a hexagon, and the zones first below written in Table C.

2. Two 3-zoned monaxine heteroids, with gonoedral axes, having one the zone $Z=\{1_p+2,\ \mathbf{1}_p+\mathbf{2},\ 0,\ \mathbf{0}\}$, and the other the zone $Z=\{1_p+2,\ \mathbf{1}_p+\mathbf{2},\ 0^2,\ \mathbf{0}^2\}$. Each axis is terminated by a triace and a triangle.

3. One 2-ple zoneless monaxine heteroid, with gonoedral axis, terminated by a 4-ace and a 4-gon.

4. Two monozones, having one the zone $Z=\{1,\ \mathbf{3},\ 0^2\}$, and the other $Z=\{3,\ \mathbf{1},\ \mathbf{0}^2\}$.

5. Two asymmetric 7-edra 7-acra.

Table C.

Polar zoned faces :

$$6^{6a.d}_{goed}16=1, \quad Z=\{1_p,\ \mathbf{1}_p+2.\mathbf{1},\ 0^2\},$$
$$Z'=\{1_p+2.1,\ \mathbf{1}_p,\ \mathbf{0}^2\};$$
$$_{(3)}3^{3mo}_{goed}46=1, \quad Z=\{1_p+2.1,\ \mathbf{1}_p+2.\mathbf{1},\ 0^2,\ \mathbf{0}^2\};$$
$$_{(4)}3^{3mo}_{goed}46=1, \quad Z=\{1_p+2.1,\ \mathbf{1}_p+2.\mathbf{1},\ 0,\ \mathbf{0}\}.$$

Zoneless polar face:

$$4^{2}_{goed}36=1.$$

Zoned non-polar faces:

$5^{mo}26=1,$ $Z=\{1, 3, 0^2\}$;

$4^{ag}36=1,$ $3^{mo}47=2,$ $Z=\{1, 3, 0^2\}$;

$4^{ag}36=1,$ $3^{mo}47=1,$ $Z=\{3, 3, 0^2, 0^2\}$;

$4^{di}36=1,$ $3^{mo}47=1,$ $Z=\{3, 3, 0, 0\}$;

$4^{di}36=1,$ $Z=\{3, 1, 0^2\}$.

Asymmetric faces:

$5_{as}26=1,$ $4_{as}36=6,$ $3_{as}46=15.$

Table D.

Zonal edges:

$(44)_{zo}15=1,$ $Z=\{3, 3, 0^2, 0^2\}$;

$(44)_{zo}15=1,$ $Z=\{3, 3, 0, 0\}$;

$(44)_{zo}15=1,$ $Z=\{3, 1, 0^2\}$;

$(33)_{zo}15=1,$ $Z=\{3, 3, 0^2, 0^2\}$;

$(33)_{zo}15=2,$ $Z=\{3, 1, 0^2\}$.

Epizonal edges:

$(45)_{ep}05=1,$ $Z=\{1, 3, 0^2\}$;

$(36)_{ep}05=1,$ $Z=\{1, 3, 0^2\}$;

$(34)_{ep}25=2,$ $Z=\{3, 3, 0^2, 0^2\}$;

$(34)_{ep}25=1,$ $Z=\{1, 3, 0^2\}$;

$(33)_{ep}35=1,$ $Z=\{3, 3, 0, 0\}$.

Asymmetric edges:

$(44)_{as}15=4,$ $(45)_{as}05=1,$

$(33)_{as}35=10,$ $(34)_{as}25=20,$ $(35)_{as}15=6.$

Registration of **6**-*edra* 8-*acra.*

Table A.

1. One zoned triarchaxine, having the zones first below read, with three 4-zoned amphiedral janal principal, four objanal amphigonal 3-zoned secondary, and six amphigrammic janal tertiary axes.

2. One 2-zoned monaxine heteroid, with amphigrammic axis, whose zones are read below in Table D.

Table B.

Radical zoned triarchipole:

$$4^{4ad}_{ja.rad}45=1,\begin{cases}{}^3Z=\{4.1_{p_2}, 2.1_{p_1}, 0^{2.1}_{p_3}\},\\ Z'=\{\ldots, 4.1_{p_1}, 0^{4.1}_{p_3}\}.\end{cases}$$

Janal zoned polar edge:

$$(44)^{2a.d}_{ja}24=1,\quad Z=\{2.2, 2.1, 0^2_p\},$$
$$Z'=\{\ldots, 2.2, 0^2_p, 0^2\}.$$

Table C.

Zoned non-polar faces:

$$5^{mo}35=1,\quad Z=\{2, 2, 0, 0\};$$
$$4^{ag}45=1,\quad Z=\{2, 4, 0, 0^3\};$$
$$3^{mo}55=1,\quad Z=\{2, 4, 0, 0^3\}.$$

Table D.

Zoned polar edge:

$$\left.\begin{array}{l}(55)^{2a.d}_{am.gr}04=1,\\ (44)^{2a.d}_{am.gr}24=1,\end{array}\right\}\begin{array}{l}Z=\{2.1, 2.1, 0^*_p, 0_p\};\\ Z'=\{2.1, 2.2, 0_p, 0_p, 0^{2.1}\}.\end{array}$$

$$(44)^{2a.d}_{am.gr}24=1,\quad Z=\{2.2, 2.1, 0^2_p\},$$
$$Z'=\{\ldots, 2.2, 0^2_p, 0^{2.1}\}.$$

Epizonal edge:

$$(34)_{ep}34=1,\quad Z=\{2, 4, 0, 0^3\}.$$

Asymmetric edges:

$$(54)_{as}14=1,\quad (53)_{as}24=1.$$

Registration of 8-edra 6-acra.

Table A.

1. One zoned triarchaxine, whose three principal 4-zoned amphigonal axes carry 4-aces, and have each the zones

$${}^3Z=\{2.1_{p_1}, 4.1_p, 0^{2.1}_{p_3}\},\quad {}^3Z'=\{4.1_{p_1}, \ldots, 0^{4.1}_{p_3}\},$$

whose four objanal amphiedral 3-zoned secondary axes have the zone first below read, and which has six tertiary janal amphigrammic axes, carrying the above-written zones.

2. One 2-zoned monaxine heteroid, with zones read below in Table D, about an amphigrammic axis.

Table B.

Homozone polar face:

$$3^{3mo}_{obja}37=1,\quad {}^{3}Z=\{2.1,\ 2(1_p+1),\ 0^{2.1}\},$$
$$\zeta=6\{0_p\}\ \text{(art. XXII.)}.$$

This 3-zoned secondary pole of the regular octaedron is registered as the termination of a homozone axis; for all janal constructions upon it will be homozones. The six poles registered in the zonoid signature are here *zoned* amphigrammic poles of 2-ple repetition. To see this, we need only crown the hexagon 123456 with a triace above on 135, and with a triace below on 246: the axis this constructed is the reciprocal of the one here recorded.

Janal zoned polar edge:

$$(33)^{2a.d}_{ja}26=1,\quad \begin{cases} Z=\{2.1,\ 2.2,\ 0^{2.1}_p\}, \\ Z'=\{2.2,\ldots,\ 0^{2.1}_p,\ 0^{2.1}\}. \end{cases}$$

Table C.

Zoned non-polar faces:

$$3^{mo}37=1,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$3^{mo}37=1,\quad Z=\{4,\ 2,\ 0^3,\ 0\}.$$

Asymmetric face:

$$3_{as}37=1.$$

Table D.

Zoned polar edges:

$$(33)^{2a.d}_{am.gr}26=2,\quad Z=\{2.1,\ 2.1,\ 0_p,\ 0_p\},$$
$$Z'=\{2.2,\ 2.1,\ 0_p,\ 0_p,\ 0^{2.1}\}.$$

Zonal edge:

$$(33)_{zo}26=1,\quad Z=\{4,\ 2,\ 0^3,\ 0\}.$$

Asymmetric edges:

$$(33)_{as}26=2.$$

Registration of 7-edra 8-acra.

Table A.

1. Two 2-zoned monaxine heteroids, having each an edrogrammic axis, one terminated by the polar hexagon, and the other by the polar tetragon, first below written.

2. Two 2-ple zoneless monaxine heteroids, with edrogrammic axes, both terminated by tetragons.

3. Four monozones: three of them have the zone $Z=\{2, 3, 0^2, 0\}$; the fourth has the zone $Z=\{2, 3, 0\}$.

4. Three asymmetric 7-edra 8-acra.

Table C.

Zoned polar faces:

$$6^{2a.d}_{edgr}26=1, \quad Z=\{\ldots, 1_p+2.1, 0_p, 0^{2.1}\},$$
$$Z'=\{2.2, 1_p \quad , 0_p, 0^{2.1}\}.$$
$$4^{2di}_{edgr}46=1, \quad Z=\{2.2, 1_p+2.1, 0_p, 0^{2.1}\},$$
$$Z'=\{2.2, 1_p, \quad , 0_p, 0^{2.1}\}.$$

Zoneless polar faces:

$$4^{2}_{edgr}46=2.$$

Zoned non-polar faces:

$$6^{ag}\ 26=1, \quad Z=\{2, 3, 0, 0^2\};$$
$$5^{mo}\ 36=2, \quad Z=\{2, 3, 0, 0^2\};$$
$$5^{mo}\ 36=1, \quad Z=\{2, 3, 0\};$$
$$4^{ag}\ 46=1, \quad Z=\{.., 3, 0^3\};$$
$$4^{ag}\ 46=2, \quad Z=\{2, 3, 0, 0^2\};$$
$$4^{di}\ 46=1, \quad Z=\{2, 3, 0\};$$
$$3^{mo}\ 56=4, \quad Z=\{2, 3, 0, 0^2\};$$
$$3^{mo}\ 56=1, \quad Z=\{2, 3, 0\};$$
$$3^{mo}\ 56=1, \quad Z=\{4, 3, 0^2, 0\}.$$

Asymmetric faces:

$$5_{as}36=5, \quad 4_{as}46=14, \quad 3_{as}56=18.$$

Table D.

Zoned polar edges:

$$(44)^{2a.d}_{edgr}25=1, \quad Z=\{2.2, 1_p, \quad 0_p, 0^{2.1}\},$$
$$Z'=\{\ldots, 1_p+2.1, 0, 0^{2.1}\};$$
$$(33)^{2a.d}_{edgr}45=1, \quad Z=\{2.2, 1_p, \quad 0_p, 0^{2.1}\},$$
$$Z'=\{2.2, 1_p+2.1, 0_p, 0^{2.1}\}.$$

Zoneless polar edges:

$$(55)^{2}_{edgr}05=1, \quad (44)^{2}_{edgr}25=1.$$

Zonal edges:

$$(44)_{zo}25=2, \quad (33)_{zo}45=1, \quad Z=\{2, 3, 0, 0^2\};$$
$$(44)_{zo}25=1, \quad Z=\{4, 3, 0^2, 0\};$$
$$(44)_{zo}25=1, \quad (33)_{zo}45=1, \quad Z=\{4, 1, 0^3\}.$$

Epizonal edges:

$(46)_{ep}05=1,\quad Z=\{\ldots, 3, 0^3\}$;

$(45)_{ep}15=2,\quad (36)_{ep}15=2,\quad (34)_{ep}35=2,\quad Z=\{2, 3, 0, 0^2\}$;

$(35)_{ep}25=1,\quad Z=\{2, 3, 0\}$.

Asymmetric edges:

$(64)_{as}05=1,\quad (55)_{as}05=1,\quad (45)_{as}15=11,\quad (36)_{as}15=2$;

$(44)_{as}25=11,\quad (35)_{as}25=17,\quad (34)_{as}35=25,\quad (33)_{as}45=8$.

Registration of 8-edra 7-acra.

Table A.

1. Two 2-zoned monaxine heteroids, with gonogrammic axes, terminated by a hexace having the zones first below read in Table D, and by a tessarace with the zones next there written.

2. Two 2-ple zoneless monaxine heteroids with gonogrammic axes, both terminated by tessaraces.

3. Four monozones; three having the zone $Z=\{3, 2, 0^2, 0\}$, and one with the zone $Z=\{3, 2, 0\}$.

4. Three asymmetric 8-edra 7-acra.

Table C.

Zoned non-polar faces:

$5^{mo}27=2,\quad Z=\{3, 2, 0^2, 0\}$;

$4^{di}37=2,\quad Z=\{3, 2, 0\}$;

$4^{ag}37=2,\quad Z=\{1, 4, 0^3\}$;

$3^{mo}47=4,\quad Z=\{3, 2, 0^2, 0\}$;

$3^{mo}47=2,\quad Z=\{1, 4, 0^3\}$;

$3^{mo}47=1,\quad Z=\{3, 4, 0, 0^2\}$.

Asymmetric faces:

$4_{as}37=9,\quad 3_{as}47=36$.

Table D.

Zoned polar edges:

$$(44)^{2a.d}_{go.gr}16=1,\quad Z=\{1_p+2.1,\ 2.2,\ 0_p,\ 0^{2.1}\},\quad Z'=\{1_p,\ 2.2,\ 0_p,\ 0^{2.1}\};$$

$$(44)^{2a.d}_{go.gr}16=1,\quad Z=\{1+2.1,\ \ldots,\ 0_p,\ 0^{2.1}\},\quad Z'=\{1_p,\ 2.2,\ 0_p,\ 0^{2.1}\}.$$

Zoneless polar edges:

$(33)^{2}_{go.gr}46=2$.

Zonal edges:

$(\mathbf{44})_{zo}\mathbf{16}=1,\quad (\mathbf{33})_{zo}36=5,\quad Z=\{3,\ \mathbf{2},\ \mathbf{0}^2,\ 0\};$

$(\mathbf{33})_{zo}36=1,\quad Z=\{3,\ \mathbf{2},\ \mathbf{0}\};$

$(\mathbf{33})_{zo}36=1,\quad Z=\{3,\ ..,\ 0^3\}.$

Epizonal edges:

$(\mathbf{35})_{ep}\mathbf{16}=2,\quad (\mathbf{33})_{ep}\mathbf{36}=1,\quad Z=\{3,\ \mathbf{2},\ \mathbf{0}^2,\ 0\};$

$(\mathbf{34})_{ep}26=2,\quad Z=\{1,\ \mathbf{4},\ 0^3\};$

$(\mathbf{33})_{ep}\mathbf{36}=1,\quad Z=\{3,\ \mathbf{4},\ \mathbf{0},\ 0^2\}.$

Asymmetric edges:

$(\mathbf{35})_{as}\mathbf{16}=4,\quad (\mathbf{33})_{as}\mathbf{36}=34;$

$(\mathbf{44})_{as}\mathbf{16}=3,\quad (\mathbf{34})_{as}\mathbf{26}=35.$

Registration of **7**-*edra* 9-*acra*.

Table A.

1. Two 2-zoned monaxine heteroids, with gonoedral axes; one carrying a hexagon and a tessarace, and the zones first below read; the other carrying a tetragon and a tessarace, with the zones next written.

2. Four monozones; one with the zone $Z=\{1,\ \mathbf{3},\ 0^2\}$, one with $Z=\{3,\ \mathbf{3},\ 0^2,\ \mathbf{0}^2\}$, and two with $Z=\{3,\ \mathbf{3},\ \mathbf{0},\ 0\}$.

3. Two asymmetric **7**-edra 9-acra.

Table C.

Zoned polar faces:

$\mathbf{6}^{2a.d}_{goed}\mathbf{36}=1,\quad Z=\{1_p+2.1,\ \mathbf{1}_p+\mathbf{2.1},\ \mathbf{0}^{2.1},\ 0^{2.1}\},$

$Z'=\{1_p+2.1,\ \mathbf{1}_p,\ \mathbf{0}^{2.1}\};$

$\mathbf{4}^{2ag}_{goed}\mathbf{56}=1,\quad Z=\{1_p,\ \mathbf{1}_p+\mathbf{2.2},\ 0^{2.2}\},$

$Z'=\{1_p,\ \mathbf{1}_p+2.\mathbf{1},\ 0^{2.1}\}.$

Zoned non-polar faces:

$\mathbf{6}^{ag}\mathbf{36}=1,\quad Z=\{1,\ \mathbf{3},\ 0^2\};$

$\mathbf{5}^{mo}\mathbf{46}=2,\quad Z=\{1,\ \mathbf{3},\ 0^2\};$

$\mathbf{5}^{mo}\mathbf{46}=2,\quad Z=\{3,\ \mathbf{3},\ \mathbf{0},\ 0\};$

$\mathbf{5}^{mo}\mathbf{46}=1,\quad Z=\{3,\ \mathbf{3},\ \mathbf{0}^2,\ 0^2\};$

$\mathbf{4}^{di}\mathbf{56}=2,\quad Z=\{3,\ \mathbf{3},\ \mathbf{0},\ 0\};$

$\mathbf{4}^{ag}\mathbf{56}=1,\quad Z=\{3,\ \mathbf{3},\ \mathbf{0}^2,\ 0^2\};$

$\mathbf{4}^{ag}\mathbf{56}=1,\quad Z=\{1,\ \mathbf{5},\ 0^4\};$

$\mathbf{3}^{mo}\mathbf{66}=2,\quad Z=\{3,\ \mathbf{3},\ \mathbf{0},\ 0\};$

$\mathbf{3}^{mo}\mathbf{66}=2,\quad Z=\{3,\ \mathbf{3},\ \mathbf{0}^2,\ 0^2\};$

$\mathbf{3}^{mo}\mathbf{66}=1,\quad Z=\{1,\ \mathbf{3},\ 0^2\};$

$\mathbf{3}^{mo}\mathbf{66}=1,\quad Z=\{1,\ \mathbf{5},\ 0^4\}.$

Asymmetric faces:

$$6_{as}36=1, \quad 5_{as}46=5, \quad 4_{as}56=9, \quad 3_{as}66=8.$$

Table D.

Zonal edges:

$$(55)_{zo}15=1, \quad (44)_{zo}35=1, \quad Z=\{3, 3, 0, 0\};$$
$$(33)_{zo}55=1, \quad (55)_{zo}15=1, \quad (44)_{zo}35=1, \quad Z=\{3, 3, 0^2, 0^2\};$$
$$(44)_{zo}35=1, \quad Z=\{3, 1, 0^2\}.$$

Epizonal edges:

$$(54)_{ep}25=1, \quad (36)_{ep}25=1, \quad (34)_{ep}45=1, \quad Z=\{3, 3, 0^2, 0^2\};$$
$$(56)_{ep}05=1, \quad (36)_{ep}25=1, \quad (45)_{ep}25=1, \quad Z=\{1, 3, 0^2\};$$
$$(35)_{ep}35=2, \quad Z=\{3, 3, 0, 0\};$$
$$(44)_{ep}35=1, \quad (34)_{ep}45=1, \quad Z=\{1, 5, 0^4\}.$$

Asymmetric edges:

$$(65)_{as}05=1, \quad (55)_{as}15=3, \quad (64)_{as}15=4,$$
$$(63)_{as}25=4, \quad (54)_{as}25=13, \quad (53)_{as}35=13,$$
$$(44)_{as}35=12, \quad (43)_{as}45=10, \quad (33)_{as}55=1.$$

Registration of 9-edra 7-acra.

Table A.

1. Two 2-zoned monaxine heteroids, having gonoedral axes, one carrying a hexace and a tetragon, with the zones first below read, and the other carrying a tetragon and a tessarace, with the next-written zones.

2. Four monozones; one having the zone $Z=\{3, 1, 0^2\}$, one having $Z=\{3, 3, 0^2, 0^2\}$, and two having $Z=\{3, 3, 0, 0\}$.

3. Two asymmetric 9-edra 7-acra.

Table C.

Zoned polar faces:

$$4^{2ag}_{go.ed}38=1, \quad Z=\{1_p+2.1, 1_p+2.1, 0^{2.1}, 0^{2.1}\},$$
$$Z'=\{1_p, \quad 1_p+2.1, 0^{2.1}\};$$
$$4^{2di}_{go.ed}38=1, \quad Z=\{1_p+2.2, 1_p, \quad 0^{2.2}\},$$
$$Z'=\{1_p+2.1, 1_p, \quad 0^{2.1}\}.$$

Zoned non-polar faces:

$$4^{di}38=1, \quad Z=\{3, 1, 0^2\};$$
$$4^{di}38=2, \quad Z=\{3, 3, 0, 0\};$$
$$4^{ag}38=1, \quad Z=\{3, 3, 0^2, 0^2\};$$
$$3^{mo}48=3, \quad Z=\{3, 3, 0^2, 0^2\};$$
$$3^{mo}48=4, \quad Z=\{3, 3, 0, 0\};$$
$$3^{mo}48=1, \quad Z=\{1, 3, 0^2\}.$$

Asymmetric faces:

$$4_{as}38=2, \quad 3_{as}48=32.$$

Table D.

Zonal edges:

$$(33)_{zo}37=3, \quad Z=\{3, 3, 0^2, 0^2\};$$
$$(33)_{zo}37=2, \quad Z=\{3, 3, 0, 0\};$$
$$(33)_{zo}37=3, \quad Z=\{3, 1, 0^2\};$$
$$(33)_{zo}37=2, \quad Z=\{5, 1, 0^4\}.$$

Epizonal edges:

$$(33)_{ep}37=2, \quad Z=\{3, 3, 0, 0\};$$
$$(34)_{ep}27=3, \quad Z=\{3, 4, 0^2, 0^2\};$$
$$(34)_{ep}27=1, \quad Z=\{1, 3, 0^2\}.$$

Asymmetric edges:

$$(34)_{as}27=16, \quad (33)_{as}37=39.$$

Registration of 8*-edra* 8*-acra.*

Table A.

1. One zoned triaxine, having three amphigrammic axes, whose poles and zones are read below in Table B.

2. Two homozone triaxines, having one a zoned amphigrammic and two zoneless amphiedral, and the other a zoned amphigrammic and two zoneless amphigonal, axes. The poles and zones are written below in Table B.

3. One 7-zoned monaxine heteroid, viz. the pyramid on 7-gonal base.

4. Five 2-ple zoneless monaxine heteroids, with amphigrammic axes.

5. Eleven monozones, of which

3	have the zone	$Z=\{2, 2, 0, 0\}$,
2	,,	$Z=\{4, 2, 0^3, 0\}$,
2	,,	$Z=\{2, 4, 0, 0^3\}$,
2	,,	$Z=\{2, 4, 0^2\}$,
2	,,	$Z=\{4, 2, 0^2\}$.

6. Twenty-two asymmetric 8-edra 8-acra.

Table B.

Janal zoneless polar face: $4^2_{ja}47=1$, the zoneless pole of one homozone triaxine. The reciprocal summit is the zoneless pole of the other homozone triaxine.

Heterozone janal polar edges:

$$(44)^{2a.d}_{ja}26=1, \quad \{Z_1, Z_2, Z_3\},$$
$$(44)^{2a.d}_{ja}26=1, \quad \{Z_2, Z_3, Z_1\},$$
$$(33)^{2a.d}_{ja}46=1, \quad \{Z_3, Z_1, Z_2\},$$

where

$$Z_1=\{2.2, \ \ldots, \ 0^2_p, \ 0^2_p\},$$
$$Z_2=\{\ \ldots, \ 2.2, \ 0^2_p, \ 0^2_p\},$$
$$Z_3=\{2.2, \ 2.2, \ 0_p, \ 0^2_p\},$$

and the two zones first named after an edge are those about its axis.

Homozone janal polar edges:

$$(44)^{2a.d}_{ja}26=1, \quad Z=\{2.1, \ 2.2, \ 0_p, \ 0_p, \ 0^{2.1}\}, \quad \zeta=4\{1_p\};$$
$$(33)^{2a.d}_{ja}46=1, \quad Z=\{2.2, \ 2.1, \ 0_p, \ 0_p, \ 0^{2.1}\}, \quad \zeta=4\{1_p\}.$$

Table C.

Zoned polar face:

$$7^{7mo}17=1, \quad Z=\{1_p+1, \ 1_p+1, \ 0, \ 0\}.$$

Zoneless polar face:

$$4^2_{amgo}47=1.$$

Zoned non-polar faces:

$$5^{mo}37=4, \quad Z=\{2, \ 2, \ 0, \ 0\};$$
$$5^{mo}37=2, \quad Z=\{4, \ 2, \ 0^3, \ 0\};$$
$$5^{mo}37=1, \quad Z=\{2, \ 4, \ 0^2\};$$
$$4^{ag}47=5, \quad Z=\{2, \ 4, \ 0^3, \ 0\};$$
$$4^{ag}47=1, \quad Z=\{2, \ 4, \ 0^2\};$$
$$4^{ag}47=1, \quad Z=\{.., 4, \ 0^4\};$$
$$4^{di}47=4, \quad Z=\{4, \ 2, \ 0^2\};$$
$$4^{di}47=1, \quad Z=\{2, \ 4, \ 0^2\};$$
$$3^{mo}57=3, \quad Z=\{2, \ 2, \ 0, \ 0\};$$
$$3^{mo}57=3, \quad Z=\{4, \ 2, \ 0^3, \ 0\};$$
$$3^{mo}57=5, \quad Z=\{2, \ 4, \ 0^2\};$$
$$3^{mo}57=5, \quad Z=\{2, \ 4, \ 0, \ 0^3\};$$
$$3^{mo}57=1, \quad Z=\{4, \ 4, \ 0^2, \ 0^2\}.$$

Asymmetric faces:

$$6_{as}27=2,\quad 5_{as}37=16,\quad 4_{as}47=70,\quad 3_{as}57=137.$$

The reciprocals of all these faces are the summits of Table C; but we omit them, as we never have occasion, in our processes of construction, to inspect those summits.

Table D.

Zoned polar edges:

$$(44)^{2a.d}_{am.gr}26=1,\quad Z=\{2.2,\ ..,\ 0^2_p,\ 0^{2.1}\},\quad Z'=\{\ ..,\ 2.2,\ 0^2_p,\ 0^{2.1}\};$$
$$(44)^{2a.d}_{am.gr}26=1,\quad Z=\{\ ..,\ 2.2,\ 0^2_p,\ 0^{2.1}\},\quad Z'=\{2.2,\ 2.2,\ 0^2_p,\ 0^{2.1}\};$$
$$(33)^{2a.d}_{am.gr}46=1,\quad Z=\{2.2,\ 2.2,\ 0^2_p,\ 0^{2.1}\},\quad Z'=\{2.2,\ ..,\ 0^2_p,\ 0^{2.1}\};$$
$$(44)^{2a.d}_{am.gr}26=1,\quad Z=Z'=\{2.1,\ 2.2,\ 0_p,\ 0_p,\ 0^{2.1}\};$$
$$(33)^{2a.d}_{am.gr}46=1,\quad Z=Z'=\{2.2,\ 2.1,\ 0_p,\ 0_p,\ 0^{2.1}\}.$$

Zoneless polar edges:

$$(55)^2_{am.gr}06=2,\quad (44)^2_{am.gr}26=4,\quad (33)^2 46=4.$$

Zonal edges:

$$(44)_{zo}26=1,\quad (33)_{zo}46=3,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(44)_{zo}26=3,\quad (33)_{zo}46=4,\quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$(44)_{zo}26=1,\quad (33)_{zo}46=1,\quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$(44)_{zo}26=2,\quad (33)_{zo}46=2,\quad Z=\{4,\ 2,\ 0^2\}.$$

Epizonal edges:

$$(55)_{ep}06=1,\quad (37)_{ep}06=1,\quad (35)_{ep}26=2,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(35)_{ep}26=2,\quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$(44)_{ep}26=2,\quad (34)_{ep}36=5,\quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$(35)_{ep}26=1,\quad (34)_{ep}36=2,\quad (33)_{ep}46=1,\quad Z=\{2,\ 4,\ 0^2\}.$$

Asymmetric edges:

$$(64)_{as}06=2,\quad (63)_{as}16=10,\quad (54)_{as}16=27;$$
$$(53)_{as}26=65,\quad (44)_{as}26=45;$$
$$(43)_{as}36=169,\quad (33)_{as}46=85.$$

Registration of 7-edra 10-acra.

Table A.

1. One 5-zoned monarchaxine, with principal polar pentagons and edrogrammic secondary axes. This is the pentagonal prism.

2. Two 3-zoned monaxine heteroids, one with polar hexagon and triace, the other with polar triace and triangle. The zones are read in Table C below.

3. One 2-zoned monaxine heteroid, with edrogrammic axis, carrying a polar tetragon, with the zones

$$Z=\{\ ..,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\},\quad Z'=\{2.1,\ 1_p+2.2,\ 0_p,\ 0^{2.2}\}.$$

4. One 2-ple zoneless monaxine heteroid, with edrogrammic axis terminated by a hexagon.

Table B.

Janal zoned pole:

$$5^{5mo}\,56=1,\quad Z=\{2.1,\ 1_p+2.1_{p'},\ 0_{p'},\ 0^{2.1}\},$$
$$Z''=\{\ ..,\ 5.1_{p'},\qquad 0^5_{p'}\}.$$

Table C.

Zoned polar faces:

$$6^{3ag}_{goed}46=1,\quad Z=\{1_p+1,\ 1_p+2,\ 0,\ 0^2\};$$
$$(3)3^{3mo}_{goed}76=1,\quad Z=\{1_p+3,\ 1_p+2,\ 0^2,\ 0\};$$
$$4^{2ag}_{edgr}66=1,\quad Z=\{\ ..,\quad 1_p+2.1,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{2.1,\quad 1_p+2.2,\ 0_p,\ 0^{2.2}\};$$
$$4^{2ag}_{edgr}66=1,\quad Z=\{2.1,\quad 1_p+2.1,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{\ ..,\quad 1_p+2.2,\ 0_p,\ 0^{2.2}\}.$$

Zoneless polar face:

$$6^2_{edgr}46=1.$$

Zoned non-polar faces:

$$5^{mo}56=1,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$$
$$5^{mo}56=1,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$$
$$6^{ag}46=1,\quad Z=\{..,\ 3,\ 0^2\};$$
$$4^{di}\,66=1,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$$
$$4^{ag}\,66=1,\quad Z=\{2,\ 5,\ 0,\ 0^4\};$$
$$3^{mo}76=1,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$$
$$3^{mo}76=1,\quad Z=\{2,\ 5,\ 0,\ 0^4\}.$$

Asymmetric faces:

$$5_{as}56=1,\quad 4_{as}66=1,\quad 3_{as}76=1.$$

Table D.

Zoned polar edges:

$$(44)^{2a.d}_{edgr}45=1,\quad Z=\{2.1,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{\ ..,\ 1_p+2.2,\ 0_p,\ 0^{2.2}\};$$
$$(66)^{2a.d}_{edgr}05=1,\quad Z=\{2.1,\ 1_p+2.1,\ 0_p,\ 0^{2.2}\},$$
$$Z'=\{\ ..,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\}.$$

Zoneless polar edge:

$$(55)^2_{edgr}\,25=1.$$

Zonal edges:

$$(55)_{zo}25=1, \quad Z=\{2,\ 3,\ 0,\ 0^2\};$$

$$(44)_{zo}45=1, \quad (55)_{zo}25=1, \quad Z=\{4,\ 3,\ 0^2,\ 0\}.$$

Epizonal edges:

$$(56)_{ep}15=1, \quad (36)_{ep}35=1, \quad (45)_{ep}35=1, \quad Z=\{2,\ 3,\ 0,\ 0^2\};$$

$$(46)_{ep}25=1, \quad Z=\{..,\ 3,\ 0^3\};$$

$$(34)_{ep}55=1, \quad Z=\{2,\ 5,\ 0,\ 0^2\}.$$

Asymmetric edges:

$$(56)_{as}15=1, \quad (46)_{as}25=2, \quad (36)_{as}35=2;$$

$$(45)_{as}35=3, \quad (35)_{as}45=2, \quad (34)_{as}55=1.$$

Registration of the 10-edra 7-acra.

Table A.

1. One 5-zoned monarchaxine, viz. the double pentagonal pyramid.

2. Two 3-zoned monaxine heteroids, one with polar hexace and triangle, the other with polar triace and triangle. The zones are read in order in Table C.

3. One 2-zoned monaxine heteroid, whose gonogrammic axis carries a polar tessarace, with the zones

$$Z=\{1_p+2.1,\ ..,\ 0_p,\ 0^{2.1}\}, \quad Z'=\{1_p+2.2,\ 2.1,\ 0_p,\ 0^{2.2}\}.$$

4. One 2-ple zoneless monaxine heteroid, whose gonogrammic axis carries a hexace.

Table C.

Zoned polar faces:

$$(5)3^{3mo}_{go.ed}49=1, \quad Z=\{1_p+2,\ 1_p+1,\ 0^2,\ 0\};$$

$$(4)3^{3mo}49=1, \quad Z=\{1_p+2,\ 1_p+3,\ 0,\ 0^2\}.$$

Zoned non-polar faces:

$$3^{mo}49=3, \quad Z=\{3,\ 4,\ 0,\ 0^2\};$$

$$3^{mo}49=2, \quad Z=\{3,\ 2,\ 0^2,\ 0\};$$

$$3^{mo}49=1, \quad Z=\{5,\ 2,\ 0^4,\ 0\}.$$

Asymmetric faces:

$$3_{as}49=8.$$

Table D.

Zoned polar edges:

$$(\mathbf{33})^{2a.d}_{go.gr}\mathbf{38}=1,\quad Z=\{1_p+2.2,\ \ldots,\ \mathbf{0}_p,\ \mathbf{0}^{2.1}\},$$
$$Z'=\{1_p+2.1,\ 2.\mathbf{1},\ 0_p,\ \mathbf{0}^{2.2}\};$$
$$(\mathbf{33})^{2a.d}_{go.gr}\mathbf{38}=1,\quad Z=\{1_p+2.2,\ 2.\mathbf{1},\ 0_p,\ \mathbf{0}^{2.2}\},$$
$$Z'=\{1_p+2.1,\ \ldots,\ \mathbf{0}_p,\ \mathbf{0}^{2.1}\}.$$

Zoneless polar edge:

$$(\mathbf{33})^{2}_{go.gr}\mathbf{38}=1.$$

Zonal edges:

$$(\mathbf{33})_{zo}\mathbf{38}=3,\quad Z=\{3,\ \mathbf{2},\ \mathbf{0}^2,\ 0\};$$
$$(\mathbf{33})_{zo}\mathbf{38}=1,\quad Z=\{3,\ldots,\mathbf{0}^3\};$$
$$(\mathbf{33})_{zo}\mathbf{38}=2,\quad Z=\{5,\ \mathbf{2},\ \mathbf{0}^4,\ 0\};$$
$$(\mathbf{33})_{zo}\mathbf{38}=1,\quad Z=\{3,\ \mathbf{4},\ \mathbf{0},\ 0\}.$$

Epizonal edges:

$$(\mathbf{33})_{ep}\mathbf{38}=2,\quad Z=\{3,\ \mathbf{4},\ \mathbf{0},\ 0^2\};$$
$$(\mathbf{33})_{ep}\mathbf{38}=1,\quad Z=\{3,\ \mathbf{2},\ \mathbf{0}^2,\ 0\}.$$

Asymmetric edges:

$$(\mathbf{33})_{as}\mathbf{38}=11.$$

Registration of **8**-*edra* 9-*acra.*

Table A.

1. One 3-zoned monarchaxine, with principal polar triangles, and gonogrammic secondary axes. The zones are read in Table B.

2. Two 3-zoned monaxine heteroids, one of which has a polar hexagon and triangle, and the other two polar triangles. The zones are the two first read in Table C.

3. One 2-zoned monaxine heteroid, whose gonogrammic axis carries a hexace, with the zones

$$Z=\{1_p+2.1,\ 2.\mathbf{1},\ \mathbf{0}_p\},\quad Z'=\{1_p+2.1,\ 2.\mathbf{1},\ 0_p,\ \mathbf{0}^{2.1}\}.$$

4. Five zoneless 2-ple monaxine heteroids, with gonogrammic axes.

5. Seventeen monozones; of which

6 have the zone $Z=\{3,\ \mathbf{2},\ \mathbf{0}^2,\ 0\}$,
3 have the zone $Z=\{3,\ \mathbf{2},\ \mathbf{0}\}$,
1 has the zone $Z=\{3,\ \mathbf{4},\ \mathbf{0}^2,\ 0^3\}$,
1 has the zone $Z=\{5,\ \mathbf{2},\ \mathbf{0}^3\}$,
3 have the zone $Z=\{3,\ \mathbf{4},\ \mathbf{0},\ 0^2\}$,
3 have the zone $Z=\{1,\ \mathbf{4},\ 0^3\}$.

6. Forty-eight asymmetric **8**-edra 9-acra.

Table B.

Zoned janal polar face:

$$3^{3mo}_{ja}67=1, \quad Z=\{1_p+2.1,\ 2_p+2.1,\ 0^{2.1},\ 0_{p''},\ 0^{2.1}\};$$
$$Z''=3\{1_{p'},\quad \ldots \quad,\ 0_{p'}\}.$$

Table C.

Zoned polar faces:

$$6^{3ag}_{amed}37=1, \quad Z=\{1,\ 2_p+2,\ 0^3\};$$
$$(4)3^{3mo}_{amed}67=1, \quad (3)3^{3mo}_{amed}67=1, \quad Z=\{3,\ 2_p+2,\ 0,\ 0^2\};$$
$$(4)3^{3mo}_{amed}67=1, \quad Z=\{1,\ 2_p+2,\ 0^3\};$$
$$(3)3^{3mo}_{amed}67=1, \quad Z=\{3,\ 2_p+2,\ 0^2,\ 0^3\}.$$

Zoned non-polar faces:

$$7^{mo}27=2, \quad Z=\{3,\ 2,\ 0^2,\ 0\};$$
$$6^{ag}37=1, \quad Z=\{3,\ 2,\ 0^2,\ 0\};$$
$$6^{di}37=2, \quad Z=\{3,\ 2,\ 0\};$$
$$5^{mo}47=2, \quad Z=\{3,\ 4,\ 0,\ 0^2\};$$
$$5^{mo}47=6, \quad Z=\{3,\ 2,\ 0^2,\ 0\};$$
$$5^{mo}47=2, \quad Z=\{1,\ 4,\ 0^3\};$$
$$4^{ag}57=3, \quad Z=\{3,\ 4,\ 0^2,\ 0^3\};$$
$$4^{ag}57=2, \quad Z=\{3,\ 4,\ 0,\ 0^2\};$$
$$4^{ag}57=6, \quad Z=\{1,\ 4,\ 0^3\};$$
$$4^{di}57=5, \quad Z=\{3,\ 2,\ 0\};$$
$$4^{di}57=2, \quad Z=\{3,\ 4,\ 0,\ 0^2\};$$
$$4^{di}57=2, \quad Z=\{5,\ 2,\ 0^3\};$$
$$3^{mo}67=2, \quad Z=\{3,\ 4,\ 0^2,\ 0^3\};$$
$$3^{mo}67=8, \quad Z=\{3,\ 4,\ 0,\ 0^2\};$$
$$3^{mo}67=5, \quad Z=\{3,\ 2,\ 0^2,\ 0\};$$
$$3^{mo}67=5, \quad Z=\{1,\ 4,\ 0^3\}.$$

Asymmetric faces:

$$6_{as}37=13, \quad 5_{as}47=67, \quad 4_{as}57=156, \quad 3_{as}67=213.$$

Table D.

Zoned polar edge:

$$(44)^{2a.d}_{go.gr}36=1, \quad Z=\{1_p+2.1,\ 2.2,\ 0_p,\ 0^{2.1},\ 0^{2.1}\},$$
$$Z'=\{1_p+2.1,\ \ldots,\ 0_p,\ 0^{2.1}\}.$$

Zoneless polar edges:

$$(55)^2_{go.gr}16=1, \quad (44)^2_{go.gr}36=3, \quad (33)^2_{go.gr}56=1.$$

Zonal edges:

$(55)_{zo}16=2$, $(44)_{zo}36=2$, $Z=\{3, 4, 0, 0^2\}$;

$(55)_{zo}16=1$, $(44)_{zo}36=6$, $(33)_{zo}56=6$, $Z=\{3, 2, 0^2, 0\}$;

$(55)_{zo}16=1$, $(44)_{zo}36=1$, $(33)_{zo}56=1$, $Z=\{3, 4, 0^2\, 0^3\}$;

$(44)_{zo}36=2$, $(33)_{zo}56=1$, $Z=\{5, 2, 0^3\}$;

$(44)_{zo}36=2$, $(33)_{zo}56=1$, $Z=\{3, 2, 0\}$.

Epizonal edges:

$$\left.\begin{array}{ll}(64)_{ep}16=2, & (63)_{ep}26=2,\\ (45)_{ep}26=2, & (34)_{ep}46=4,\end{array}\right\} Z=\{1, 4, 0^3\};$$

$(73)_{ep}16=2$, $(55)_{ep}16=1$, $Z=\{3, 2, 0^2, 0\}$;

$(53)_{ep}36=2$, $(43)_{ep}46=4$, $(33)_{ep}56=2$, $Z=\{3, 4, 0, 0^2\}$;

$(44)_{ep}36=1$, $(33)_{ep}56=3$, $Z=\{3, 4, 0^2, 0^3\}$.

Asymmetric edges:

$(74)_{as}06=2$, $(73)_{as}16=4$, $(65)_{as}06=6$, $(64)_{as}16=28$,

$(63)_{as}26=52$, $(55)_{as}26=20$, $(54)_{as}36=127$,

$(53)_{as}36=177$, $(44)_{as}36=113$, $(43)_{as}46=254$, $(33)_{as}56=81$.

Registration of 9-edra 8-acra.

Table A.

1. One 3-zoned monarchaxine, with principal polar triaces, and edrogrammic secondary axes. The zones are those first read in Table D below.

2. Two 3-zoned monaxine heteroids, of which one has a polar hexace and triace, and the other two polar triaces. The zones are the reciprocals of the two first read in the above Table C.

3. One 2-zoned monaxine heteroid, whose edrogrammic axis carries a hexagon, with the zones first written below in Table C.

4. Five zoneless 2-ple monaxine heteroids, with edrogrammic axes.

5. Seventeen monozones; of which

6 have the zone $Z=\{2, 3, 0, 0^2\}$,
3 have the zone $Z=\{2, 3, 0\}$,
1 has the zone $Z=\{4, 3, 0_3, 0^2\}$,
1 has the zone $Z=\{2, 5, 0^3\}$,
3 have the zone $Z=\{4, 3, 0^2, 0\}$,
3 have the zone $Z=\{4, 1, 0^3\}$.

6. Forty-eight asymmetric 9-edra 8-acra.

Table C.

Zoned polar faces:

$\underline{6}^{2a.d}_{edgr}\,28=1,\quad Z=\{2.1,\ 1_p+2.1,\ 0_p\},$
$\qquad Z'=\{2.1,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\};$

$4^{2ag}_{edgr}\,48=1,\quad Z=\{2.2,\ 1_p+2.1,\ 0_p,\ 0^{2.1},\ 0^{2.1}\},$
$\qquad Z'=\{\ldots,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\}.$

Zoneless polar face:

$4^{2}_{edgr}\,48=5.$

Zoned non-polar faces:

$6^{ag}\,28=1,\quad Z=\{\ldots,\ 3,\ 0^3\};$
$6^{di}\,28=1,\quad Z=\{4,\ 1,\ 0^3\};$
$5^{mo}\,38=2,\quad Z=\{2,\ 3,\ 0\};$
$5^{mo}\,38=3,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$
$4^{di}\,48=3,\quad Z=\{2,\ 3,\ 0\};$
$4^{di}\,48=4,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$
$4^{di}\,48=3,\quad Z=\{4,\ 1,\ 0^3\};$
$4^{ag}\,48=5,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$
$4^{ag}\,48=1,\quad Z=\{4,\ 3,\ 0^3,\ 0^2\};$
$4^{ag}\,48=1,\quad Z=\{2,\ 5,\ 0^3\};$
$3^{mo}\,58=5,\quad Z=\{2,\ 3,\ 0\};$
$3^{mo}\,58=10,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$
$3^{mo}\,58=3,\quad Z=\{4,\ 3,\ 0^3,\ 0^2\};$
$3^{mo}\,58=8,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$
$3^{mo}\,58=4,\quad Z=\{2,\ 5,\ 0^3\}.$

Asymmetric faces:

$5_{as}38=19,\quad 4_{as}48=121,\quad 3_{as}58=367.$

Table D.

Zoned polar edges:

$(44)^{2a.d}_{ed.gr}27=1,\quad Z=\{2.2,\ 1_p+2.1,\ 0_p,\ 0^{2.1},\ 0^{2.1}\},$
$\qquad Z'=\{\ldots,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\};$

$(33)^{2a.d}_{ed.gr}47=1,\quad Z=\{2.1,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\},$
$\qquad Z'=\{2.1,\ 1_p+2.1,\ 0_p\}.$

Zoneless polar edges:

$(44)^{2}_{ed.gr}27=2,\quad (33)^{2}_{edgr}47=3.$

Zonal edges:

$$(44)_{zo}27=4,\quad (33)_{zo}47=4,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$$
$$(44)_{zo}27=3,\quad (33)_{zo}47=9,\quad Z=\{4,\ 1,\ 0^3\};$$
$$(44)_{zo}27=1,\quad (33)_{zo}47=5,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$$
$$(44)_{zo}27=1,\quad (33)_{zo}47=3,\quad Z=\{4,\ 3,\ 0^2,\ 0\}.$$

Epizonal edges:

$$(63)_{ep}17=3,\quad (54)_{ep}17=3,\quad (43)_{ep}37=7,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$$
$$(53)_{ep}27=2,\quad (33)_{ep}47=1,\quad Z=\{2,\ 3,\ 0\};$$
$$(43)_{ep}37=3,\quad Z=\{4,\ 3,\ 0^3,\ 0^2\};$$
$$(33)_{ep}47=4,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$$
$$(43)_{ep}37=2,\quad (33)_{ep}47=1,\quad Z=\{2,\ 5,\ 0^3\}.$$

Asymmetric edges:

$$(63)_{as}17=6,\quad (53)_{as}27=90,\quad (54)_{as}17=15;$$
$$(44)_{as}27=62,\quad (43)_{as}37=371,\quad (33)_{as}47=320.$$

Registration of 10*-edra* 8*-acra.*

Table A.

1. One 4-zoned monarchaxine homozone, with principal polar tetragons, and amphigrammic zoneless axes. The zone is first read in Table B below.

2. One homozone triaxine, with zoned tetragon poles, and amphigrammic zoneless axes, with zone next read in Table B.

3. One 2-ple monaxine monozone, with amphigrammic axis, with the zone $Z=\{2.2,\ 2.1,\ 0^{2.1}\}$.

4. Two 2-zoned monaxine heteroids, one with amphigrammic axis, having the zones

$$Z=\{2.1,\ 2.2,\ 0_p,\ 0_p,\ 0^{2.1}\},\quad Z'=\{2.1,\ 2.1,\ 0_p,\ 0_p\},$$

the other with amphigonal axis, carrying a hexace and a tessarace, with the zones

$$Z=\{2.1_p,\ 2.2,\ 0^{2.1}\},\quad Z'=\{2.1_p+2.1,\ 2.1,\ 0^{2.1}\}.$$

5. Eight zoneless 2-ple monaxine heteroids, one having an amphigonal axis, and seven with amphigrammic axes.

6. Nineteen monozones, of which

6 have the zone $Z=\{4,\ 2,\ 0^3,\ 0\}$,
1 has the zone $Z=\{4,\ 2,\ 0^2\}$,
2 have the zone $Z=\{4,\ 4,\ 0^2,\ 0^2\}$,
3 have the zone $Z=\{2,\ 4,\ 0^2\}$,

2 have the zone $Z=\{2,\ 4,\ 0,\ 0^3\}$,
4 have the zone $Z=\{2,\ 2,\ 0,\ 0\}$,
1 has the zone $Z=\{4, \ldots, 0^4\}$.

7. Forty-four asymmetric 10-edra 8-acra.

Table B.

Homozone janal polar faces:

$$4^{4a.d}_{ja.hom}49=1,\quad Z=\{2.1,\ 2_p+2.1,\ 0^{2.1}\},$$
$$\zeta=8\{0_p\};$$
$$4^{2di}_{ja.hom}49=1,\quad Z=\{2.2,\ 2_p,\ 0^{2.1}\},$$
$$\zeta=4\{0_p\}.$$

Janal zoneless amphigrammic poles:

$$(33)^2_{ja}48=2,\quad (33)^2_{co.ja}48=1.$$

Table C.

Zoned non-polar faces:

$$5^{mo}39=3,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$5^{mo}39=3,\quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$5^{m}\,39=2,\quad Z=\{2,\ 4,\ 0^2\};$$
$$4^{di}\,49=4,\quad Z=\{4,\ 2,\ 0^2\};$$
$$4^{di}\,49=1,\quad Z=\{2,\ 4,\ 0^2\};$$
$$4^{ag}\,49=5,\quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$4^{ag}\,49=1,\quad Z=\{2,\ 4,\ 0^2\};$$
$$3^{mo}59=9,\quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$3^{mo}59=6,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$3^{mo}59=8,\quad Z=\{4,\ 4,\ 0_2,\ 0\};$$
$$3^{mo}59=9,\quad Z=\{2,\ 4,\ 0^2\}.$$

Objanal monozone face:

$$4^{di}_{obj}49=1,\quad Z=\{2.2,\ 2.1,\ 0^{2.1}\}.$$

Asymmetric faces:

$$5_{as}39=4,\quad 4_{as}49=95,\quad 3_{as}59=456.$$

Janal anaxine faces:

$$3_{ja.an}59=2.$$

Table D.

Zoned polar edges:

$$(33)^{2a.d}_{am.gr}48=1,\quad (44)^{2a.d}_{am.gr}28=1,\quad Z=\{2.1,\ 2.2,\ 0_p,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{2.1,\ 2.1,\ 0_p,\ 0_p\}.$$

Zoneless polar edges:

$$(33)^2_{am.gr}48=12, \quad (44)^2_{am.gr}28=5.$$

Objanal zonal edge:

$$(33)_{zo.obj}48=1, \quad Z=\{2.2,\ 2.1,\ 0^{2.1}\}.$$

This edge is also enumerated below among the zonals of the signature $\{4,\ 2,\ 0^2\}$ (*vide* note to art. XLIX.).

Zonal non-polar edges:

$$(44)_{zo}28=1, \quad (33)_{zo}48=3, \quad Z=\{4, \ldots, 0^4\};$$
$$(44)_{zo}28=3, \quad (33)_{zo}48=15, \quad Z=\{4,\ 2,\ 0^3,\ 0^2\};$$
$$(44)_{zo}28=2, \quad (33)_{zo}48=2, \quad Z=\{4,\ 4,\ 0^2,\ 0^2\};$$
$$(33)_{zo}48=4, \quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(33)_{zo}48=2, \quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$(33)_{zo}48=5, \quad Z=\{4,\ 2,\ 0^2\}.$$

Epizonal edges:

$$(53)_{ep}28=3, \quad (33)_{ep}48=1, \quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(53)_{ep}28=3, \quad (33)_{ep}48=3, \quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$(44)_{ep}28=2, \quad (34)_{ep}38=4, \quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$(53)_{ep}28=2, \quad (33)_{ep}48=3, \quad Z=\{2;\ 4,\ 0^2\};$$
$$(33)_{ep}48=4, \quad Z=\{4,\ 4,\ 0^2,\ 0^2\}.$$

Asymmetric edges:

$$(53)_{as}28=36, \quad (44)_{as}28=22;$$
$$(34)_{as}38=342, \quad (33)_{as}48=493.$$

Janal anaxine edges:

$$(33)_{ja.an}48=2, \quad (43)_{ja.an}38=1.$$

Registration of 8-*edra* 10-*acra*.

Table A.

1. One 4-zoned monarchaxine homozone, with principal polar tessaraces, and amphigrammic zoneless axes. The zone is

$$Z=\{2_p+2.1,\ 2.1,\ 0^{2.1}\}.$$

2. One homozone triaxine, with zoned polar tessaraces, and amphigrammic zoneless axes. The zone is $Z=\{2_p,\ 2.2,\ 0^{2.1}\}$.

3. One 2-ple monaxine monozone, with amphigrammic axis. The zone is $Z=\{2.1,\ 2.2,\ 0^{2.1}\}$.

4. Two 2-zoned monaxine heteroids, one with amphigrammic axis, having the zones

$$Z=\{2.2,\ 2.1,\ 0_p,\ 0_p,\ 0^{2.1}\}, \quad Z'=\{2.1,\ 2.1,\ 0_p,\ 0_p\},$$

the other having an amphiedral axis, with the zones

$$Z=\{2.2,\ 2.1_p,\ 0^{2.1}\},\quad Z'=\{2.1,\ 2.1_p+2.1,\ 0^{2.1}\}.$$

5. Eight zoneless 2-ple monaxine heteroids, one having an amphiedral axis, and seven with amphigrammic axes.

6. Nineteen monozones, of which

6 have the zone $Z=\{2, 4, 0, 0^3\}$,
1 has the zone $Z=\{2, 4, 0^2\}$,
2 have the zone $Z=\{4, 4, 0^2, 0^2\}$,
3 have the zone $Z=\{4, 2, 0^2\}$,
2 have the zone $Z=\{4, 2, 0^3, 0\}$,
4 have the zone $Z=\{2, 2, 0, 0\}$,
1 has the zone $Z=\{.., 4, 0^4\}$.

7. Forty-four asymmetric 8-edra 10-acra.

Table B.

Janal zoneless amphigrammic poles :

$$(55)^2_{ja}26=1,\quad (44)^2_{ja}46=1,\quad (44)^2_{co.ja}46=1.$$

Table C.

Zoned polar faces :

$$\left.\begin{array}{l} 6^{2a.d}_{am.ed}47=1, \\ 4^{2.di}_{am.ed}67=1, \end{array}\right\} \left\{\begin{array}{l} Z=\{2.2,\ 2_p,\ 0^{2.1}\}, \\ Z'=\{2.1,\ 2_p+2.1,\ 0^{2.1}\}. \end{array}\right.$$

Zoneless polar face :

$$4^2_{am.ed}67=2.$$

Zoned non-polar faces :

$7^{mo}37=1$, $Z=\{4, 2, 0^3, 0\}$;
$7^{mo}37=3$, $Z=\{2, 2, 0, 0\}$;
$6^{ag}47=1$, $Z=\{.., 4, 0^4\}$;
$6^{ag}47=3$, $Z=\{2, 4, 0, 0^3\}$;
$6^{di}47=2$, $Z=\{4, 2, 0^2\}$;
$5^{mo}57=1$, $Z=\{4, 4, 0^2, 0^2\}$;
$5^{mo}57=2$, $Z=\{4, 2, 0^3\}$;
$5^{mo}57=3$, $Z=\{2, 4, 0, 0^3\}$;
$5^{mo}57=3$, $Z=\{2, 4, 0^2\}$;
$4^{di}67=5$, $Z=\{4, 2, 0^2\}$;
$4^{di}67=1$, $Z=\{4, 4, 0^2, 0^2\}$;
$4^{di}67=1$, $Z=\{2, 4, 0^2\}$;

$$4^{ag}67=1, \quad Z=\{2,\ 4,\ 0^2\};$$
$$4^{ag}67=9, \quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$4^{ag}67=3, \quad Z=\{..,\ 4,\ 0^4\};$$
$$4^{ag}67=1, \quad Z=\{4,\ 4,\ 0^2,\ 0^2\};$$
$$3^{mo}77=2, \quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$3^{mo}77=9, \quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$3^{mo}77=4, \quad Z=\{2,\ 4,\ 0^2\};$$
$$3^{mo}77=5, \quad Z=\{4,\ 4,\ 0^2,\ 0^2\};$$
$$3^{mo}77=1, \quad Z=\{2,\ 2,\ 0,\ 0\}.$$

Objanal monozone faces:

$$5^{mo}_{obj}57=1, \quad 3^{mo}_{obj}77=1, \quad Z=\{2.1,\ 2.2,\ 0^{2.1}\}.$$

These are also entered above.

Janal anaxine face:

$4_{ja.an}67=1$, which is also entered below.

Asymmetric faces:

$$7_{as}37=2, \quad 6_{as}47=30;$$
$$5_{as}57=90, \quad 4_{as}67=146, \quad 3_{as}77=165.$$

Table D.

Zoned polar edges:

$$(55)^{2a.d}_{am.gr}26=2, \quad Z=\{2.2,\ 2.1,\ 0_p,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{2.1,\ 2\cdot1,\ 0_p,\ 0_p\}.$$

Zoneless polar edges:

$$(66)^2_{am.gr}06=2, \quad (55)^2_{am.gr}26=5;$$
$$(33)^2_{am.gr}66=2, \quad (44)^2_{am.gr}46=8.$$

Zonal non-polar edges:

$$(55)_{zo}26=1, \quad (44)_{zo}46=4, \quad (33)_{zo}66=2, \quad Z=\{4,\ 2,\ 0^2\};$$
$$(55)_{zo}26=3, \quad (44)_{zo}46=2, \quad (33)_{zo}66=1, \quad Z=\{2,\ 4,\ 0,\ 0^3\};$$
$$(55)_{zo}26=2, \quad (44)_{zo}46=2, \quad Z=\{4,\ 4,\ 0^2,\ 0^2\};$$
$$(55)_{zo}26=1, \quad (44)_{zo}46=3, \quad (33)_{zo}66=3, \quad Z=\{4,\ 2,\ 0^3,\ 0\};$$
$$(44)_{zo}46=3, \quad (33)_{zo}66=1, \quad Z=\{2,\ 2,\ 0,\ 0\}.$$

Epizonal edges:

$$(57)_{ep}06=2, \quad (55)_{ep}26=1, \quad (37)_{ep}26=1, \quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(53)_{ep}46=1, \quad (43)_{ep}56=2, \quad (33)_{ep}66=1, \quad Z=\{4,\ 2,\ 0^2\};$$
$$(73)_{ep}26=1, \quad (53)_{ep}46=1, \quad Z=\{4,\ 2,\ 0^3,\ 0\};$$

$(63)_{ep}36=1$, $(54)_{ep}36=1$, $(53)_{ep}46=2$,
$(43)_{ep}56=1$, $Z=\{2, 4, 0^2\}$;

$(63)_{ep}36=3$, $(54)_{ep}36=3$, $(44)_{ep}46=3$,
$(64)_{ep}26=3$, $(34)_{ep}56=6$, $\Big\}\ Z=\{2, 4, 0, 0^3\}$;

$(53)_{ep}46=1$, $(43)_{ep}56=2$, $(33)_{ep}66=1$, $Z=\{4, 4, 0^2, 0^2\}$.

Asymmetric edges:

$(75)_{as}06=2$, $(66)_{as}06=2$, $(74)_{as}16=10$;
$(65)_{as}16=32$, $(73)_{as}26=14$, $(64)_{as}26=69$;
$(55)_{as}26=44$, $(63)_{as}36=88$, $(54)_{as}36=169$;
$(53)_{as}46=174$, $(44)_{as}46=91$, $(43)_{as}56=165$, $(33)_{as}66=33$.

Janal anaxine edges:

$(54)_{ja.an}36=2$, $(43)_{ja.an}56=1$.

Registration of 9-edra 9-acra.

Table A.

1. One 8-zoned monaxine heteroid, with gonoedral axis, viz. the octagonal pyramid.

2. Two 4-zoned monaxine heteroids, with gonoedral axes, each carrying a tessarace and a tetragon, with zones below read in Table C.

3. Eight zoneless 2-ple monaxine heteroids, with gonoedral axes.

4. Forty-eight monozones, of which

11	have the zone	$Z=\{3, 3, 0^2, 0^2\}$,
15	,,	$Z=\{3, 3, 0, 0\}$,
7	,,	$Z=\{1, 3, 0^2\}$,
7	,,	$Z=\{3, 1, 0^2\}$,
2	,,	$Z=\{3, 5, 0, 0^3\}$,
2	,,	$Z=\{5, 3, 0^3, 0\}$,
2	,,	$Z=\{1, 5, 0^4\}$,
2	,,	$Z=\{5, 1, 0^4\}$.

5. Two hundred and thirty-seven asymmetrical 9-edra 9-acra.

Table C.

Zoned polar faces:

$8^{8a.d}_{go.ed}18=1$, $Z=\{3, 1, 0^2\}$, $Z'=\{1, 3, 0^2\}$;

$4^{4a.d}_{go.ed}58=1$, $Z=\{1_p+2.1, 1_p+2.1\}$,
$Z'=\{1_p+2.1, 1_p+2.1, 0^{2.1}, 0^{2.1}\}$;

$4^{4a.d}_{go.ed}58=1$, $Z=\{1_p, 1_p+2.2, 0^{2.2}\}$,
$Z'=\{1_p+2.2, 1_p, 0^{2.2}\}$.

Zoneless polar faces:

$$6^{2}_{go.ed}38=2, \quad 4^{2}_{go.ed}58=6.$$

Zoned non-polar faces;

$$
\begin{array}{ll}
7^{mo}28=1, & Z=\{1,\ 3,\ 0^2\}; \\
6^{ag}38=3, & Z=\{1,\ 3,\ 0^2\}; \\
6^{ag}38=3, & Z=\{3,\ 3,\ 0^2,\ 0^2\}; \\
6^{di}38=2, & Z=\{3,\ 3,\ 0,\ 0\}; \\
6^{di}38=3, & Z=\{3,\ 1,\ 0^2\}; \\
6^{di}38=1, & Z=\{5,\ 1,\ 0^4\}; \\
5^{mo}48=6, & Z=\{1,\ 3,\ 0^2\}; \\
5^{mo}48=10, & Z=\{3,\ 3,\ 0,\ 0\}; \\
5^{mo}48=6, & Z=\{3,\ 3,\ 0^2,\ 0^2\}; \\
4^{ag}58=4, & Z=\{1,\ 3,\ 0^2\}; \\
4^{ag}58=8, & Z=\{3,\ 3,\ 0^2,\ 0^2\}; \\
4^{ag}58=2, & Z=\{3,\ 5,\ 0,\ 0^3\}; \\
4^{ag}58=7, & Z=\{1,\ 5,\ 0^4\}; \\
4^{di}58=13, & Z=\{3,\ 3,\ 0,\ 0\}; \\
4^{di}58=1, & Z=\{3,\ 3\}; \\
4^{di}58=4, & Z=\{1,\ 3,\ 0^2\}; \\
4^{di}58=1, & Z=\{5,\ 1,\ 0^4\}; \\
4^{di}58=2, & Z=\{5,\ 3,\ 0^3,\ 0\}; \\
3^{mo}68=17, & Z=\{3,\ 3,\ 0^2,\ 0^2\}; \\
3^{mo}68=20, & Z=\{3,\ 3,\ 0,\ 0\}; \\
3^{mo}68=8, & Z=\{1,\ 3,\ 0^2\}; \\
3^{mo}68=4, & Z=\{5,\ 3,\ 0^2,\ 0\}; \\
3^{mo}68=8, & Z=\{3,\ 5,\ 0,\ 0^3\}; \\
3^{mo}68=5, & Z=\{1,\ 5,\ 0^4\}.
\end{array}
$$

Asymmetric faces:

$$7_{as}28=2, \quad 6_{as}38=30,$$

$$5_{as}48=221, \quad 4_{as}58=717, \quad 3_{as}68=1344.$$

The summits of the 9-edra 9-acra are the reciprocals of the foregoing faces.

Table D.

Zonal non-polar edges:

$(\mathbf{55})_{zo}1\mathbf{7}=1$,	$(\mathbf{44})_{zo}3\mathbf{7}=6$,	$(\mathbf{33})_{zo}5\mathbf{7}=8$,	$Z=\{3, \mathbf{1}, \mathbf{0}^2\}$;
$(\mathbf{55})_{zo}1\mathbf{7}=1$,	$(\mathbf{44})_{zo}3\mathbf{7}=1$,		$Z=\{3, \mathbf{5}, \mathbf{0}, 0^3\}$;
$(\mathbf{55})_{zo}1\mathbf{7}=1$,	$(\mathbf{44})_{zo}3\mathbf{7}=9$,	$(\mathbf{33})_{zo}5\mathbf{7}=13$,	$Z=\{3, \mathbf{3}, \mathbf{0}^2, 0^2\}$;
$(\mathbf{55})_{zo}1\mathbf{7}=1$,	$(\mathbf{44})_{zo}3\mathbf{7}=8$,	$(\mathbf{33})_{zo}5\mathbf{7}=6$,	$Z=\{3, \mathbf{3}, \mathbf{0}, 0\}$;
	$(\mathbf{44})_{zo}3\mathbf{7}=4$,	$(\mathbf{33})_{zo}5\mathbf{7}=6$,	$Z=\{5, \mathbf{1}, \mathbf{0}^4\}$;
	$(\mathbf{44})_{zo}3\mathbf{7}=4$,	$(\mathbf{33})_{zo}5\mathbf{7}=2$,	$Z=\{5, \mathbf{3}, \mathbf{0}^3, 0\}$.

Epizonal edges:

$(\mathbf{54})_{ep}2\mathbf{7}=6$,	$(\mathbf{43})_{ep}4\mathbf{7}=11$,	$(\mathbf{63})_{ep}2\mathbf{7}=6$,	$Z=\{3, \mathbf{3}, \mathbf{0}^2, 0^2\}$;
$(\mathbf{53})_{ep}3\mathbf{7}=10$,	$(\mathbf{33})_{ep}5\mathbf{7}=5$,		$Z=\{3, \mathbf{3}, \mathbf{0}^2, 0^2\}$;
$(\mathbf{56})_{ep}0\mathbf{7}=2$,	$(\mathbf{54})_{ep}2\mathbf{7}=4$,	$(\mathbf{63})_{ep}2\mathbf{7}=4$,	$Z=\{1, \mathbf{3}, \mathbf{0}^2\}$;
$(\mathbf{83})_{ep}0\mathbf{7}=1$,	$(\mathbf{74})_{ep}0\mathbf{7}=1$,	$(\mathbf{43})_{ep}4\mathbf{7}=3$,	
$(\mathbf{43})_{ep}4\mathbf{7}=4$,	$(\mathbf{33})_{ep}5\mathbf{7}=2$,		$Z=\{3, \mathbf{5}, \mathbf{0}, 0^3\}$;
	$(\mathbf{33})_{ep}5\mathbf{7}=2$,		$Z=\{5, \mathbf{3}, \mathbf{0}^3, 0\}$;
$(\mathbf{44})_{ep}3\mathbf{7}=5$,	$(\mathbf{43})_{ep}4\mathbf{7}=5$,		$Z=\{1, \mathbf{5}, 0^4\}$.

Asymmetric edges:

$(\mathbf{73})_{as}1\mathbf{7}=15$,	$(\mathbf{74})_{as}0\mathbf{7}=2$,	$(\mathbf{63})_{as}2\mathbf{7}=158$;
$(\mathbf{64})_{as}1\mathbf{7}=54$,	$(\mathbf{65})_{as}0\mathbf{7}=5$,	$(\mathbf{55})_{as}1\mathbf{7}=38$;
$(\mathbf{54})_{as}2\mathbf{7}=339$,	$(\mathbf{53})_{as}3\mathbf{7}=719$,	$(\mathbf{44})_{as}3\mathbf{7}=487$;
$(\mathbf{43})_{as}4\mathbf{7}=1532$,	$(\mathbf{33})_{as}5\mathbf{7}=808$.	

Registration of the **8**-*edra* 11-*acra.*

Table A.

1. Four 2-zoned monaxine heteroids, with gonogrammic axes carrying tessaraces, with the zones first read below in Table D.

2. One 2-ple zoneless monaxine heteroid, with gonogrammic axis carrying a tessarace.

3. Twelve monozones, of which

4 have the zone $Z=\{3, \mathbf{4}, \mathbf{0}, 0^2\}$,
2 have the zone $Z=\{3, \mathbf{4}, \mathbf{0}^2, 0^3\}$,
2 have the zone $Z=\{3, \mathbf{2}, \mathbf{0}^2, 0\}$,
1 has the zone $Z=\{3, \mathbf{2}, \mathbf{0}\}$,
1 has the zone $Z=\{5, \mathbf{2}, \mathbf{0}^3\}$,
2 have the zone $Z=\{1, \mathbf{4}, \mathbf{0}^3\}$.

4. Twenty-one asymmetric **8**-edra 11-acra.

Table C.

Zoned non-polar faces:

$7^{mo}47=2,\quad Z=\{3,\ 2,\ 0^2,\ 0\};$

$6^{di}57=2,\quad Z=\{3,\ 2,\ 0^2,\ 0\};$

$6^{di}57=1,\quad Z=\{5,\ 2,\ 0^3\};$

$6^{ag}57=1,\quad Z=\{3,\ 4,\ 0,\ 0^2\};$

$6^{ag}57=1,\quad Z=\{3,\ 4,\ 0^2,\ 0^2\};$

$6^{ag}57=3,\quad Z=\{1,\ 4,\ 0^3\};$

$5^{mo}67=3,\quad Z=\{1,\ 4,\ 0^3\};$

$5^{mo}67=6,\quad Z=\{3,\ 4,\ 0,\ 0^2\};$

$5^{mo}67=1,\quad Z=\{3,\ 4,\ 0^2,\ 0^3\};$

$5^{mo}67=2,\quad Z=\{3,\ 2,\ 0^2,\ 0\};$

$4^{di}77=1,\quad Z=\{3,\ 2,\ 0^2,\ 0\};$

$4^{di}77=2,\quad Z=\{5,\ 2,\ 0^3\};$

$4^{di}77=3,\quad Z=\{3,\ 4,\ 0,\ 0^2\};$

$4^{ag}77=3,\quad Z=\{1,\ 4,\ 0^3\};$

$4^{ag}77=2,\quad Z=\{1,\ 6,\ 0^5\};$

$4^{ag}77=3,\quad Z=\{3,\ 4,\ 0^2,\ 0^3\};$

$4^{ag}77=2,\quad Z=\{3,\ 4,\ 0,\ 0^2\};$

$3^{mo}87=3,\quad Z=\{1,\ 4,\ 0^3\};$

$3^{mo}87=1,\quad Z=\{1,\ 6,\ 0^5\};$

$3^{mo}87=1,\quad Z=\{3,\ 2,\ 0^2,\ 0\};$

$3^{mo}87=8,\quad Z=\{3,\ 4,\ 0,\ 0^2\};$

$3^{mo}87=3,\quad Z=\{3,\ 4,\ 0^2,\ 0^3\}.$

Asymmetric faces:

$7_{as}47=5,\quad 6_{as}57=24,\quad 5_{as}67=47;$

$4_{as}77=63,\quad 3_{as}87=62.$

Table D.

Zoned polar edges:

$(66)^{2a.d}_{go.gr}16=1,\quad Z=\{1_p+2.1,\ 2.2,\ 0_p,\ 0^{2.1}\},$

$Z'=\{1_p,\ 2.2,\ 0_p,\ 0^{2.1}\};$

$(44)^{2a.d}_{go.gr}56=1,\quad Z=\{1_p+2.1,\ 2.2,\ 0_p,\ 0^{2.1}\},$

$Z'=\{1_p,\ 2.2,\ 0_p,\ 0^{2.1}\};$

$(55)^{2a.d}_{go.gr}36=1,\quad Z=\{1_p+2.2,\ 2.1,\ 0_p,\ 0^{2.1}\},$

$Z'=\{1_p+2.1,\ 2.1,\ 0_p,\ 0^{2.1}\};$

$(44)^{2a.d}_{go.gr}56=1,\quad Z=\{1_p+2.1,\ 2.1,\ 0_p\},$

$Z'=\{1_p,\ 2.3,\ 0_p,\ 0^{2.2}\}.$

Zoneless polar edge:

$$(55)^2_{go.gr}36=1.$$

Zonal non-polar edges:

$(66)_{zo}16=1$, $(55)_{zo}36=2$, $(44)_{zo}56=1$, $Z=\{3, 4, 0, 0^2\}$;

$(44)_{zo}56=1$, $(66)_{zo}16=1$, $(55)_{zo}36=1$, $(33)_{zo}76=1$, $Z=\{3, 4, 0^2, 0^3\}$;

$(55)_{zo}36=1$, $(44)_{zo}56=2$, $(33)_{zo}76=1$, $Z=\{5, 2, 0^3\}$;

$(55)_{zo}36=2$, $(44)_{zo}56=2$, $(33)_{zo}76=1$, $Z=\{3, 2, 0^2, 0\}$;

$(55)_{zo}36=1$, $Z=\{3, 2, 0\}$.

Epizonal edges:

$(53)_{ep}56=4$, $(54)_{ep}46=2$, $(63)_{ep}46=2$, $(43)_{ep}66=2$, $Z=\{3, 4, 0, 0^2\}$;

$(63)_{ep}46=1$, $(54)_{ep}46=1$, $(64)_{ep}36=1$, $(44)_{ep}56=1$, $(43)_{ep}66=2$, $Z=\{3, 4, 0^2, 0^3\}$;

$(75)_{ep}16=1$, $(73)_{ep}36=1$, $Z=\{3, 2, 0^2, 0\}$;

$(65)_{ep}26=1$, $(64)_{ep}36=2$, $(63)_{ep}46=2$, $(54)_{ep}46=2$, $(34)_{ep}66=1$, $Z=\{1, 4, 0^3\}$;

$(44)_{ep}56=1$, $(34)_{ep}66=1$, $Z=\{1, 6, 0^5\}$.

Asymmetric edges:

$(73)_{as}36=17$, $(74)_{as}26=15$, $(76)_{as}06=2$;
$(66)_{as}16=6$, $(65)_{as}26=40$, $(64)_{as}36=53$;
$(63)_{as}36=54$, $(55)_{as}36=27$, $(54)_{as}46=82$;
$(43)_{as}66=50$, $(53)_{as}56=68$, $(44)_{as}56=34$, $(33)_{as}76=5$.

Registration of 11*-edra* 8*-acra.*

1. Four 2-zoned monaxine heteroids, with edrogrammic axes, carrying tetragons and zones below written in Table C.

2. One 2-ple zoneless monaxine heteroid, with edrogrammic axis, carrying a tetragon.

3. Twelve monozones, of which

4 have the zone $Z=\{4, 3, 0^2, 0\}$,
2 have the zone $Z=\{4, 3, 0^3, 0^2\}$,
2 have the zone $Z=\{2, 3, 0, 0^2\}$,
1 has the zone $Z=\{2, 3, 0\}$,
1 has the zone $Z=\{2, 5, 0^3\}$,
2 have the zone $Z=\{4, 1, 0^3\}$.

4. Twenty-one asymmetric 11-edra 8-acra.

Table C.

Zoned polar faces:

$$4^{2di}_{ed.gr}410=1,\quad Z=\{2.3,\ 1_p,\quad 0_p,\ 0^{2.2}\},$$
$$Z'=\{2.1,\ 1_p+2.1,\ 0_p\};$$
$$4^{2di}_{ed.gr}410=2,\quad Z=\{2.2,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{2.2,\ 1_p,\quad 0_p,\ 0^{2.1}\};$$
$$4^{2ag}_{ed.gr}410=1,\quad Z=\{2.1,\ 1_p+2.2,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{2.1,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\}.$$

Zoneless polar face:

$$4^{2}_{ed.gr}410=1.$$

Zoned non-polar faces:

$$4^{di}\,410=2,\quad Z=\{4,\ 1,\ 0^3\};$$
$$4^{di}\,410=4,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$$
$$4^{ag}\,410=2,\quad Z=\{4,\ 3,\ 0^3,\ 0^2\};$$
$$4^{ag}\,410=2,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$$
$$4^{di}\,410=1,\quad Z=\{2,\ 3,\ 0\};$$
$$4^{ag}\,410=1,\quad Z=\{2,\ 5,\ 0^3\};$$
$$3^{mo}510=10,\quad Z=\{4,\ 3,\ 0^2,\ 0\};$$
$$3^{mo}510=4,\quad Z=\{4,\ 3,\ 0^3,\ 0^2\};$$
$$3^{mo}510=2,\quad Z=\{2,\ 3,\ 0\};$$
$$3^{mo}510=6,\quad Z=\{2,\ 3,\ 0,\ 0^2\};$$
$$3^{mo}510=6,\quad Z=\{2,\ 5,\ 0^3\}.$$

Asymmetric faces:

$$4_{as}410=21,\quad 3_{as}510=271.$$

Table D.

Zoned polar edges:

$$(33)^{2a.d}_{ed.gr}49=2,\quad Z=\{2.2,\ 1_p,\ 0^2,\ 0_p\},$$
$$Z'=\{2.2,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\};$$
$$(33)^{2a.d}_{ed.gr}49=1,\quad Z=\{2.1,\ 1_p+2.1,\ 0_p,\ 0^{2.1}\},$$
$$Z'=\{2.1,\ 1_p+2.2,\ 0_p,\ 0^{2.1}\};$$
$$(33)^{2a.d}_{ed.gr}49=1,\quad Z=\{2.3,\ 1_p,\quad 0_p,\ 0^{2.2}\},$$
$$Z'=\{2.1,\ 1_p+2.1,\ 0_p\}.$$

Zoneless polar edge:

$$(33)^{2}_{ed.gr}49=1.$$

Zonal non-polar edges:

$(33)_{zo}49=10,$	$Z=\{4,\ 3,\ 0^2,\ 0\};$
$(33)_{zo}49=6,$	$Z=\{4,\ 3,\ 0^3,\ 0^2\};$
$(33)_{zo}49=2,$	$Z=\{2,\ 3,\ 0,\ 0^2\};$
$(33)_{zo}49=8,$	$Z=\{4,\ 1,\ 0^3\};$
$(33)_{zo}49=2,$	$Z=\{6,\ 1,\ 0^5\}.$

Epizonal edges:

$(33)_{ep}49=4,$	$Z=\{4,\ 3,\ 0^2,\ 0\};$
$(34)_{ep}39=4,$	$Z=\{4,\ 3,\ 0^3,\ 0^2\};$
$(34)_{ep}39=5,$	$Z=\{2,\ 3,\ 0,\ 0^2\};$
$(33)_{ep}49=1,$	$Z=\{2,\ 3,\ 0\};$
$(34)_{ep}39=3,\quad (33)_{ep}49=1,$	$Z=\{2,\ 5,\ 0^3\}.$

Asymmetric edges:

$$(34)_{as}39=108,\quad (33)_{as}49=352.$$

Registration of 8*-edra* 12*-acra.*

Table A.

1. One zoned tetrarchaxine, having for principal poles hexagons and triangles, with amphigrammic secondary axes. The zone is

$${}^4Z=\{2.1,\ 2_p+2_p,\ 0^{2.1},\ 0_{p1},\ 0_{p1}\}.$$

2. One 3-zoned homozone monarchaxine, with zoned polar triangles, and zoneless amphigrammic axes. The signatures are read in Table B below.

3. One homozone triaxine, with zoned and zoneless axes all amphigrammic, and signatures next read in Table B.

4. One 6-zoned heterozone monarchaxine, with principal polar hexagons, and secondary axes amphiedral and amphigrammic. The zones are first read in Table B below.

5. Three 2-zoned monaxine heteroids, one having polar hexagon and tetragon, with zones

$$Z=\{2.2,\ 2_p+2.1,\ 0^{2.1},\ 0^{2.1}\},$$
$$Z'=\{2.2,\ 2_p,\qquad 0^{2.1}\},$$

and the others having amphigrammic axes, with the zones

$$Z=\{\ ..,\ 2.2,\ 0^2_p,\ 0^{2.1}\},\qquad Z'=\{2.2,\ 2.2,\ 0^2_p,\ 0^{2.1}\};\ \text{and}$$
$$Z=\{2.1,\ 2.3,\ 0_p,\ 0_p,\ 0^{2.1}\},\quad Z'=\{2.1,\ 2.1,\ 0_p,\ 0_p\}.$$

6. One zoneless 2-ple monaxine heteroid, with amphigrammic axis.

7. Four monozones, of which

2 have the zone $Z=\{2, 4, 0, 0^3\}$,
1 has the zone $Z=\{2, 2, 0, 0\}$, and
1 has the zone $Z=\{4, 4, 0^2, 0^2\}$.

8. Two asymmetric 8-edra 12-acra.

Table B,

Heterozone polar faces:

$$6_{ja}^{6a.d}67=1;$$

$$Z=\{\ldots, 2_p+2_{p_{\prime}}, 0^{2.2}\},\ Z'=\{2.2, 2_p, 0^2_{p_{\prime\prime}}\},\ Z''=\{\ldots, 6.1_{p_{\prime}}, 0^6_{p_{\prime\prime}}\};$$

$$4_{ja}^{2ag}87=1,$$

$$Z=\{2.2, 2_p, 0^2_{p_{\prime}}\},\ Z'=\{\ldots, 2_p+2.2, 0^2_{p_{\prime}}, 0^{2.2}\},\ Z''=\{\ldots, 2_p+2_{p_{\prime\prime}}, 0^{2.2}\}.$$

Homozone polar face:

$$_{(3)}3_{obj}^{3mo}97=1,\quad Z=\{2.2,\ 2.1_p+2.1,\ 0^{2.1},\ 0^{2.1}\},$$
$$\zeta=6\{0_p\}.$$

Homozone polar edges:

$$(44)_{ja}^{2a.d}66=1,\quad Z=\{2.1,\ 2.2,\ 0_p,\ 0_p,\ 0^{2.1}\},$$
$$\zeta=4\{0_p\};$$

$$(66)_{ja}^{2a.d}26=1,\quad {}^4Z=\{2.1,\ 2_p+2_p,\ 0^{2.1},\ 0_{p_{\prime}},\ 0_{p_{\prime}}\}.$$

Heterozone polar edge:

$$(44)_{ja}^{2a.d}66=1,$$

$$Z=\{\ldots, 2_p+2.2, 0^2_p\},\ Z'=\{2.2, 2_p, 0^2_p\},\ Z''=\{\ldots, 2_p+2_p, 0^{2.2}\}.$$

Janal zoneless polar edges:

$$(55)^2_{ja}46=1,\quad (55)^2_{co.ja}46=1.$$

Table C.

The polar faces of Table B above are not here repeated.

Zoned tetrarchipoles:

$$6_{am.ed.rad}^{3ag}67=1,\quad _{(3)}3_{am.ed}^{3mo}97=1,\quad {}^4Z=\{2.1, 2_p+2_p, 0^{2.1}, 0_{p_{\prime}}, 0_p\}.$$

These poles are not repeated below.

Zoned polar faces:

$$6_{am.ed}^{2a.d}67=1,\quad (44)_{am.ed}^{2di}87=1,\quad Z=\{2.2,\ 2.1_p+2.1,\ 0^{2.1},\ 0^{2.1}\},$$
$$Z'=\{2.2,\ 2_p,\ 0^{2.1}\}.$$

Zoned non-polar faces:

$$
\begin{aligned}
&7^{mo}57=2, && Z=\{2,\ 2,\ 0,\ 0\};\\
&6^{ag}67=2, && Z=\{2,\ 4,\ 0,\ 0^3\};\\
&6^{ag}67=1, && Z=\{..,\ 4,\ 0^4\};\\
&5^{mo}77=3, && Z=\{4,\ 4,\ 0^2,\ 0^2\};\\
&5^{mo}77=3, && Z=\{2,\ 4,\ 0,\ 0^3\};\\
&5^{mo}77=1, && Z=\{2,\ 2,\ 0,\ 0\};\\
&4^{di}87=1, && Z=\{4,\ 4,\ 0^2,\ 0^2\};\\
&4^{ag}87=3, && Z=\{2,\ 4,\ 0,\ 0^3\};\\
&4^{ag}87=1, && Z=\{..,\ 4,\ 0^4\};\\
&4^{ag}87=1, && Z=\{4,\ 4,\ 0^2,\ 0^2\};\\
&4^{ag}47=2, && Z=\{2,\ 6,\ 0,\ 0^5\};\\
&3^{mo}97=2, && Z=\{2,\ 4,\ 0,\ 0^3\};\\
&3^{mo}97=1, && Z=\{2,\ 6,\ 0,\ 0^5\};\\
&3^{mo}97=3, && Z=\{4,\ 4,\ 0^2,\ 0^2\}.
\end{aligned}
$$

Objanal monozone face:

$$5^{mo}_{obj}77=1,\quad Z=\{2.2,\ 2.2,\ 0^{2.1},\ 0^{2.1}\},$$

which is also above enumerated.

Asymmetric faces:

$$7_{as}57=2,\quad 6_{as}67=5;$$
$$5_{as}77=7,\quad 4_{as}87=8,\quad 3_{as}97=8.$$

Table D,

not containing the edges of the preceding Table B.

Zoned polar edges:

$$
\begin{aligned}
&(66)^{2a.d}_{am.gr}26=1,\quad (44)^{2a.d}_{am.gr}66=1, && Z=\{2.2,\ 2.2,\ 0^2_p,\ 0^{2.1}\},\\
& && Z'=\{\ ..\ ,\ 2.2,\ 0^2_p,\ 0^{2.1}\};\\
&(77)^{2a.d}_{am.gr}06=1,\quad (44)^{2a.d}_{am.gr}=1, && Z=\{2.1,\ 2.3,\ 0_p,\ 0_p,\ 0^{2.1}\},\\
& && Z'=\{2.1,\ 2.1,\ 0_p,\ 0_p\}.
\end{aligned}
$$

Zoneless polar edges:

$$(66)^2_{am.gr}26=1,\quad (55)^2_{am.gr}46=1.$$

Zonal non-polar edges:

$$
\begin{aligned}
&(44)_{zo}66=1,\quad (66)_{zo}26=1,\quad (55)_{zo}46=1, && Z=\{2,\ 4,\ 0,\ 0^3\};\\
&(55)_{zo}46=1, && Z=\{2,\ 2,\ 0,\ 0\};\\
&(66)_{zo}26=1,\quad (55)_{zo}46=2, && Z=\{4,\ 4,\ 0^2,\ 0^2\};\\
&(55)_{zo}46=1, && Z=\{4,\ 2,\ 0^2\}.
\end{aligned}
$$

Objanal zonal edge:

$(55)_{zo.ob}46=1,$ $Z=\{2.2, 2.2, 0^{2.1}, 0^{2.1}\}.$

This edge is one of the zonals above entered.

Epizonal edges:

$(63)_{ep}56=2,$ $(54)_{ep}56=2,$ $(65)_{ep}36=1,$
$(64)_{ep}46=2,$ $(43)_{ep}76=1,$ $Z=\{2, 4, 0, 0^3\};$

$(57)_{ep}26=1,$ $Z=\{2, 2, 0, 0\};$

$(63)_{ep}56=1,$ $(53)_{ep}66=2,$ $(54)_{ep}56=1,$ $(34)_{ep}76=1,$ $Z=\{4, 4, 0^2, 0^2\};$

$(64)_{ep}46=2,$ $Z=\{.., 4, 0^4\};$

$(44)_{ep}66=1,$ $(34)_{ep}76=1,$ $Z=\{2, 6, 0, 0^5\}.$

Asymmetric edges:

$(73)_{as}46=7,$ $(74)_{as}36=7,$ $(75)_{as}26=4,$ $(76)_{as}16=2;$
$(66)_{as}26=1,$ $(65)_{as}36=10,$ $(64)_{as}46=11,$ $(63)_{as}56=9;$
$(55)_{as}46=3,$ $(54)_{as}56=12,$ $(53)_{as}66=9;$
$(44)_{as}66=3,$ $(43)_{as}76=5.$

Registration of 12-edra 8-acra.

Table A.

1. One zoned tetrarchaxine, having for principal poles hexaces and triaces, with amphigrammic secondary axes. The zone is

$$^4Z=\{2_p+2_p, 2.1, 0_{p_i}, 0_{p_i}, 0^{2.1}\}.$$

2. One 3-zoned homozone monarchaxine, with zoned polar triaces and zoneless amphigrammic axes. The zonal and zonoid signatures are

$$Z=\{2.1_p+2.1, 2.2, 0^{2.1}, 0^{2.1}\}, \zeta=6\{0_p\}.$$

3. One homozone triaxine with zoned and zoneless amphigrammic axes, and zonal signature first below read in Table B.

4. One 6-zoned monarchaxine heterozone, with principal polar hexaces, and secondary axes amphigonal and amphigrammic. The zonal signatures are

$$Z=\{2_p+2_{p'}, 0^{2.2}\}, \quad Z'=\{2_p, 2.2, 0^2_{p_{\prime\prime}}\}, \quad Z''=6\{1_{p'}, .., 0_{p_{\prime\prime}}\}.$$

5. Three 2-zoned monaxine heteroids, one having polar hexace and tessarace, with the zones

$$Z=\{2_p+2.1, 2.2, 0^{2.1}, 0^{2.1}\}, \quad Z'=\{2_p, 2.2, 0^{2.1}\},$$

and two having amphigrammic axes, with the signatures

$$Z=\{2.2, .., 0^2_p, 0^{2.1}\}, \qquad Z'=\{2.2, 2.2, 0^2_p, 0^{2.1}\}, \text{ and}$$
$$Z=\{2.3, 2.1, 0_p, 0_p, 0^{2.1}\}, \quad Z'=\{2.1, 2.1, 0_p, 0_p\}.$$

6. One zoneless 2-ple monaxine heteroid, with amphigrammic axis.

7. Four monozones, of which

2 have the zone $Z=\{4, 2, 0, 0^3\}$,
1 has the zone $Z=\{2, 2, 0, 0\}$,
1 has the zone $Z=\{4, 4, 0^2, 0^2\}$.

8. Two asymmetric 12-edra 8-acra.

Table B.

Homozone polar edges:

$$(33)^{2a.d}_{ja}410=2, \quad Z=\{2.2, 2.1, 0_p, 0_p, 0^{2.1}\}.$$

Heterozone janal polar edge:

$$(33)^{2a.d}_{ja}410=1,$$

$$Z=\{2_p+2.2, .., 0^2_p\}, \quad Z'=\{2_p, 2.2, 0^2_p\}, \quad Z''=\{2_p+2_p, .., 0^{2.2}\}.$$

Janal zoneless polar edges:

$$(33)^2_{ja}410=1, \quad (33)^2_{co.ja}410=1.$$

Table C.

Zoned non-polar faces:

$$3^{mo}511=10, \quad Z=\{4, 4, 0^2, 0^2\};$$
$$3^{mo}511=3, \quad Z=\{2, 4, 0^2\};$$
$$3^{mo}511=5, \quad Z=\{4, 2, 0^2, 0^2\};$$
$$3^{mo}511=1, \quad Z=\{4, 2, 0^3, 0\};$$
$$3^{mo}511=3, \quad Z=\{2, 2, 0, 0\};$$
$$3^{mo}511=1, \quad Z=\{6, 2, 0, 0^5\}.$$

Objanal monozone faces:

$$3^{mo}511=2, \quad Z=\{2.2, 2.2, 0^{2.1}, 0^{2.1}\},$$

which are also above entered.

Asymmetric face:

$$3_{as}511=55.$$

Table D,

not comprising the edges in the above Table B.

Zoned polar edges:

$$(33)^{2a.d}_{am.gr}410=2, \quad Z=\{2.2, 2.2, 0^2_p, 0^{2.1}\},$$
$$Z'=\{2.2, .., 0^2_p, 0^{2.1}\};$$
$$(33)^{2a.d}_{am.gr}410=2, \quad Z=\{2.3, 2.1, 0_p, 0_p, 0^{2.1}\},$$
$$Z'=\{2.1, 2.1, 0_p, 0_p\}.$$

Zoneless polar edge:

$$(33)^2_{am.gr}410=2.$$

Zonal non-polar edges:

$$(33)_{zo}410=8,\quad Z=\{4,\ 2,\ 0,\ 0^3\};$$
$$(33)_{zo}410=1,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(33)_{zo}410=5,\quad Z=\{4,\ 4,\ 0^2,\ 0^2\};$$
$$(33)_{zo}410=2,\quad Z=\{4,\ ..,\ 0^4\};$$
$$(33)_{zo}410=2,\quad Z=\{6,\ 2,\ 0,\ 0^5\}.$$

Objanal zonal edge:

$$(33)_{zo.ob}410=1,\ Z=\{2.2,\ 2.2,\ 0^{2.1},\ 0^{2.1}\},$$

which is also above entered as zonal.

Epizonal edges:

$$(33)_{ep}410=3,\quad Z=\{4,\ 2,\ 0,\ 0^3\};$$
$$(33)_{ep}410=1,\quad Z=\{2,\ 2,\ 0,\ 0\};$$
$$(33)_{ep}410=3,\quad Z=\{4,\ 4,\ 0^2,\ 0^2\};$$
$$(33)_{ep}410=1,\quad Z=\{2,\ 4,\ 0^2\}.$$

Asymmetric edge:

$$(33)_{as}410=83.$$

I may be permitted to remark here that these results were in my possession early in 1858, when the prize question of the French Academy was published for the competition of 1861: "*Perfectionner en quelque point important la théorie géométrique des polyèdres.*" My work on this theory was first completely composed in the French language, in its present form, with the intention of presenting it to the Academy in 1861. Any person, who cares to know the reasons why I altered its destination, may read them at page 352 of the 'Memoirs of the Literary and Philosophical Society of Manchester,' 3rd series, vol. i. 1862, beginning at the second line *ab infra*.

II. "Contributions towards the History of the Monamines.—No. VI. Note on the Action of Iodide of Methyl on Ammonia." By A. W. Hofmann, LL.D., F.R.S. Received December 2, 1862.

When studying, many years ago, the action of iodide of methyl upon ammonia, I pointed out the existence of dimethylamine among the products of the reaction. The amount of iodide of dimethylam-

monium is very small when compared with the quantities of the iodides of methyl, trimethyl, and more especially of tetramethylammonium, not to speak of iodide of ammonium itself, so that I was compelled to be satisfied with approximative platinum determinations in order to establish the formation of dimethylamine under these circumstances. Having lately had to prepare a specimen of the dimethylated ammonia, and remembering the small quantity which is formed by means of iodide of methyl, I resolved to avail myself of an observation made in the mean time, according to which dimethylamine is formed by the action of soda-lime upon the sulphite of aldehyde ammonia,

$$C_2H_4, H_3N, SO_3 + Ca_2O = C_2H_7N + Ca_2SO_4.$$

According to M. Gossmann*, this reaction furnishes ethylamine, and indeed in such quantities as to render this process a convenient mode of preparing the substance. Re-examined subsequently by M. Petersen†, the ethylamine of M. Gossmann proved to be dimethylamine, which is isomeric with the former,

$$C_2H_7N = \left.\begin{matrix} C_2H_5 \\ H \\ H \end{matrix}\right\} N = \left.\begin{matrix} CH_3 \\ CH_3 \\ H \end{matrix}\right\} N.$$

Nor does M. Petersen appear to share the enthusiasm of his predecessor for the facility and elegance of this reaction. Indeed, a glance at the analysis published by this chemist is sufficient to show how small a quantity of the base produced by this process must have been obtained.

In repeating this experiment, I have indeed obtained a minute quantity of an inflammable ammonia; but, though varying the process, and working on a tolerably large scale, I was unable to procure a sufficient amount for a single analysis.

Under these circumstances I was compelled to return to the reaction by which I had originally obtained dimethylamine, viz. by the action of iodide of methyl upon ammonia.

By availing myself of the method of separating the ethyl-bases, which some time ago I submitted to the Royal Society‡, I succeeded

* Ann. Chem. Pharm. vol. xc. p. 122.

† Ibid. vol. cii. p. 317.

‡ Proceedings of the Royal Society, vol. xi. p. 66.

without difficulty in separating the dimethylamine from the mixture of ammonias which this reaction produces. An alcoholic solution of ammonia, gently heated with iodide of methyl in a flask provided with a condenser, rapidly solidifies into a crystalline mixture of the iodides of ammonium, methyl-, dimethyl-, trimethyl-, and tetramethylammonium. The more soluble iodides separated from the difficultly soluble iodide of tetramethylammonium are evaporated and distilled with potash, and the bases evolved carefully dried by passing over hydrate of potassium, and conveyed through a powerfully refrigerated serpentine in which dimethylamine and trimethylamine, together with a portion of methylamine, are condensed, the ammonia and the remainder of the methylamine being carried off as gas and condensed in water. The mixture of the three methylated bases is then brought in contact with oxalate of ethyl, when the methylamine immediately solidifies into a crystalline mass of dimethyloxamide,

$$C_4H_8N_2O_2 = \left.\begin{matrix}(C_2O_2)'' \\ (CH_3)_2 \\ H_2\end{matrix}\right\} N_2,$$

the dimethylamine being converted into dimethyloxamate of ethyl,

$$C_6H_{11}NO_3 = \left.\begin{matrix}[(C_2O_2)''(CH_3)_2N] \\ (C_2H_5)\end{matrix}\right\} O,$$

a liquid boiling between 250° and 260°, while the trimethylamine remaining unchanged may be expelled from the mixture by gently heating in the water-bath. Dimethyloxamate of ethyl being easily soluble in water, is separated from the dimethyloxamide by treatment of the mixture with cold water. Distilled with hydrate of potash, dimethyloxamate of ethyl yields a mixture of alcohol and dimethylamine, oxalate of potassium remaining behind. Evaporated with hydrochloric acid, the distillate furnishes a crystallized residue of chloride of dimethylammonium, from which, on addition of an alkali, the pure dimethylamine is liberated.

Dimethylamine is a powerfully alkaline liquid of a strongly ammoniacal odour, easily soluble in water, and possessing the general characters of this class of compounds. Its boiling-point, strange to say, very nearly coincides with that of trimethylamine,—the boiling-point of the former being between 8° and 9°, that of the latter 9°. To eliminate the influence of changes of pressure, the boiling-points of the two substances were determined at the same time.

I have fixed the composition of dimethylamine by the analysis of the platinum-salt and gold-salt. The former is one of the finest salts which I have ever examined, crystallizing in long splendid needles, shooting through the liquid from one side of the vessel to the other. It contains

$$C_2 H_8 N Pt Cl_3 = [(C H_3)_2 H_2 N] Cl, Pt Cl_2.$$

The gold-salt, which likewise crystallizes very well, has an analogous composition, viz.

$$C_2 H_8 N Au Cl_4 = [(CH_8)_2 H_2 N] Cl, Au Cl_3.$$

If the products obtained by distilling the sulphite of aldehyde-ammonia with lime had contained the minutest trace of dimethylamine, the formation of the beautiful characteristic platinum-salt would have revealed it. In none of the experiments did I observe the formation of this compound.

III. "Contributions towards the History of the Monamines.—No. VII. Transformation of Aniline into Benzoic Acid." By A. W. Hofmann, LL.D., F.R.S. Received December 3, 1862.

In a Note recently addressed to the Royal Society, I have described a new organic base which is formed of a secondary product in the manufacture of aniline upon a large scale. This substance, paraniline,

$$C_{12} H_{14} N_2,$$

is isomeric with aniline, and owes its origin evidently to the action of heat, under circumstances not yet precisely determined, upon that body. I have not yet succeeded in producing this compound from aniline, but the experiments made with the view of accomplishing this transformation have led me to an observation which I beg leave to mention briefly to the Royal Society. The vapour of aniline, when passed through a red-hot glass tube, undergoes decomposition; the tube becomes coated with a film of carbon, a brown liquid collects in the receiver, and a colourless gas burning with a luminous flame is evolved; if this be allowed to pass through water, the latter becomes charged with a considerable amount of cyanide of ammonium.

The brown distillate contains a large proportion of aniline which has escaped decomposition, and which may be readily separated by treatment of the distillate with an acid. On rectifying the portion of the oil which is insoluble in acid, the thermometer becomes stationary at 80°, when a colourless transparent liquid distils, possessing all the properties of benzol; it was identified, moreover, by transformation into nitrobenzol and aniline. The thermometer then rapidly rises, becoming stationary again at between 190° and 195°; a limpid oil lighter than water passes over, which by its odour is at once recognized as benzonitrile*. To remove every doubt, this oil was boiled with an alcoholic solution of potash, when torrents of ammonia were evolved, benzoate of potassium remaining as a residue. The benzoic acid was separated from the salt by addition of hydrochloric acid, and converted into the silver-salt, which was identified by analysis.

The formation, under these circumstances, of benzonitrile is probably due to a reaction at a higher temperature between aniline and the hydrocyanic acid generated during the destruction of another portion of this substance,

$$C_6 H_7 N + H C N = C_7 H_5 N + H_3 N.$$

The action of heat upon aniline gives rise, in addition, to the formation of small quantities of a crystalline indifferent substance, and an oily base boiling at a very high temperature; the nature of both these substances I have not yet determined.

The transformation of aniline into benzonitrile is thus seen to be far from elegant; and if it have any claims to notice, it is merely because there are at present comparatively few reactions known which permit a passage from a hydrocarbon, $Cn\,H_2n-6$, to an acid,

$$Cn+1\,H_2n-6\,O_2.$$

This transformation may possibly be used for the production of several of the higher terms of the series of aromatic acids which have not yet been obtained.

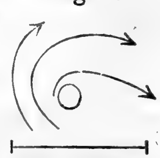
* I have lately had an opportunity of observing that benzonitrile solidifies in a mixture of solid carbonic acid and ether. The beautifully crystalline mass fuses again at −17°.

IV. "A Development of the Theory of Cyclones." By Francis Galton, F.R.S. Received December 25, 1862.

Most meteorologists are agreed that a circumscribed area of barometric depression is usually a locus of light ascending currents, and therefore of an indraught of surface winds which create a retrograde whirl (in our hemisphere), because they bring to their destination a lateral impulse, partly due to the greater easterly speed of the earth's surface whence the southern portion of the indraught took its departure, and partly due to the less easterly, or we may say greater westerly, speed of its northern portion.

Conversely, we ought to admit that a similar area of barometric elevation is usually a locus of dense descending currents, and therefore of a dispersion of a cold dry atmosphere, plunging from the higher regions upon the surface of the earth, which, flowing away radially on all sides, becomes at length imbued with a lateral motion due to the above-mentioned cause, though acting in a different manner and in opposite directions. The currents necessarily travel with diminished radial speed as they widen out from their central area of dispersion, and the eastward tendency of the northern portion of the system and the westward tendency of the southern become more overpowering. It may be presumed, on consideration of the extreme mobility of the air, that a continuous dispersion of currents would result in the yielding of the east and west winds, which had no tangential movement of their own, to the curvature of the others, and that we should witness a disposition of currents like those in the annexed diagram, which is copied from an actual occurrence on December 2, 1861. The appearance is that of a centre of calms whence currents flow in radial lines, rapidly curving to the right and forming a sort of "anticyclone."

Fig. 1.



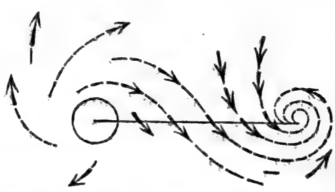
Scale 1000 miles.

Dove's law of gyration is so fertile in result, that it accounts for the same direct rotation of a cold wind by a wholly different process. As an antithesis to his theory of cyclones being due to an equatorial current pressing against quiescent air, he adds (Law of Storms), with a view of illustrating his position, and not of meeting cases that practically occur, polar cyclones, "if they exist," would have a direct rotation.

It is not necessary to allude further to his well-known theory—it is sufficient to show that two separate causes cooperate in producing a rotation or curvature of currents such as I have described. I have not the slightest doubt that a strong curvature of atmospheric currents to the right does frequently exist, owing to the descent of cold air from above; for in lately charting the weather of Europe thrice daily during a month, I found it more or less present on from fifty to sixty occasions. Its existence is consonant to what we should expect. It is hardly possible to conceive masses of air rotating in a retrograde sense in close proximity, as cyclonogists suppose, without an intermediate area of direct rotation, which would, to use a mechanical simile, be in gear with both of them, and make the movements of the entire system correlative and harmonious.

The result I have thus far arrived at, and which I should look for hereafter, is that whenever the barometer shows circumscribed areas of marked elevation and depression at distances not exceeding 1500 miles apart, a line drawn from the locus of highest to that of lowest barometer would be cut by parallel wind-currents at an angle of about 45°, in the way shown in the diagram.

Fig. 2.



I doubt if it be of advantage to investigate the changes of wind produced by a system of indraught and dispersion passing over any locality, because the barometrical sections vary so rapidly as to make the incoming portion unsymmetrical with that which has already passed over.

V. "On the Immunity enjoyed by the Stomach from being digested by its own Secretion during Life." By Frederick W. Pavy, M.D. Communicated by Dr. Sharpey, Sec. R.S. Received December 11, 1862.

(Abstract.)

The author referred to the communication by John Hunter "On the Digestion of the Stomach after Death," published in the 'Philo-

sophical Transactions' for 1772. In this communication Hunter notices that in occasional instances, especially in persons who have died of sudden and violent deaths, the stomach is found on inspection to have undergone solution, to the extent of perforation, from the action of its own secretion upon it. Hunter considered that this could only have taken place after death; and to account for why the same occurrence did not ensue during life, he adduced the living principle as constituting the protecting agent. The fact that parts of living animals, as shown by Claude Bernard of Paris, are susceptible of digestion when introduced through a fistulous opening into a digesting stomach, proved that Hunter's explanation does not stand the test of experiment. The author corroborated Bernard's results upon frogs, and referred to an experiment in which he had also obtained the digestion of the extremity of the ear of a living rabbit.

The view at present most generally entertained is, that the epithelial lining or mucus protects the stomach from undergoing digestion during life. This it is supposed is acted upon and dissolved, but being as constantly renewed, the stomach escapes injury. There being no longer the power of producing epithelium after death, accounts for the occurrence of the solution that may then be observed.

To test this view, the author removed a patch of mucous membrane about the size of a crown piece from the stomach of the dog. Food was afterwards digested without, however, the denuded stomach showing the slightest sign of attack. It thus appearing that the stomach resisted digestion notwithstanding the assumed protecting layer had been removed, it became evident that something besides the epithelial lining was required to account for the security enjoyed.

Seeing that the question was still open for explanation, the following was the view propounded by the author. The existence of acidity, it was first remarked, is an absolutely essential condition for the accomplishment of the act of digestion. During life the walls of the stomach are most freely permeated by a current of alkaline blood. Under such circumstances it would appear impossible that any digestive action could be effected. There would be one condition that would neutralize the other. Acidity is needful for digestion, and alkalinity is a constant character of the blood. As long therefore as so free a circulation of this alkaline fluid should be maintained (and

this happens to be one of the necessary conditions of life), the stomach will be supplied with a source of protection competent to afford it the security from attack by its own secretion that it enjoys.

Digestion of the stomach may be effected after death, because the blood, being then stagnant, is incapable of offering the barrier produced by a circulating current.

Experiments were mentioned in which the circulation through the stomach had been arrested during life so as to imitate the condition, as far as the stomach was concerned, that exists after death. Although this was effected whilst the process of digestion was actively proceeding, yet it was only in some cases that the mucous membrane of the stomach was attacked. On repeating the experiment, however, having previously introduced a dilute non-corrosive acid (the phosphoric and citric were the acids employed) into the stomach, the result was solution and perforation in a short space of time.

The author had expected, when he commenced his experiments, to have obtained the same result upon arresting the circulation through the stomach as occurs after death ; but it became evident to him on reflection that although the circulation through the stomach may be stopped by ligatures during life, yet the conditions are not thereby rendered completely identical with those prevailing after death. There is still a circulation all around the stomach, and from the facility with which the permeation of fluids takes place, a certain amount of counteractive influence would still be exerted. By the artificial introduction, however, of an acid into the cavity of the stomach before its vessels were ligatured, the surrounding circulation became inadequate to afford the required neutralizing power, and perforation therefore quickly resulted.

It did not appear to the author that the digestion of the living tissues of animals referred to in the first part of his paper formed any valid objection to his view. In the case of the frog's legs, he considered it might be fairly taken that the amount of blood possessed by the animal would be inadequate to furnish the required means of resistance. In the case of the rabbit's ear, the vascularity of the part being so much less than that of the walls of the stomach, he thought there was nothing unreasonable in conceiving that, whilst the one might receive protection through the circulating alkaline

current, the other might be unable to resist attack. There was no comparison between the position of the stomach and that of the rabbit's ear, and the question, according to his view, resolved itself into degree of power possessed by the acidity of the contents of the stomach on the one hand, and the alkalinity of the circulating current on the other.

The author concluded by adducing experimental evidence to show that pepsine was contained in the walls of the stomachs of persons who had died from severe diseases, as well as in the normal fasting and digesting stomach.

January 15, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read :—

I. "Notes of Researches on the Poly-Ammonias.—No. XXII. Secondary Products formed in the Manufacture of Aniline." By A. W. HOFMANN, LL.D., F.R.S. Received December 18, 1862.

In a short paper submitted to the Royal Society some weeks ago, I have recorded some experiments on the basic compounds distilling at very high temperatures, which are formed as secondary products in the manufacture of aniline, and which are known in the ateliers of MM. Collin and Coblenz as *queues d'aniline*. I have mentioned that the bases which distil above 330°, when treated with dilute sulphuric acid, furnish a soluble sulphate, the sulphate of paraniline, the history of which I have already traced, and a sulphate remarkable for its insolubility in water. It is this insoluble sulphate, and the base from which it is derived, that form the subject of the following notice.

The insoluble sulphate which is formed on treating the *queues d'aniline* boiling above 330° with cold dilute sulphuric acid, separates as a yellowish semisolid crystalline mass, contaminated with considerable quantities of the oily sulphates of other bases. Ebullition with alcohol removes these substances pretty well, and the sulphate becomes more crystalline and nearly white. A further purifi-

cation is effected by dissolving this mass in a large quantity of boiling water, and filtering from insoluble oily substances; on cooling, the sulphate separates in white needles, which, on boiling with alcohol, become perfectly pure.

In order to liberate the base, the sulphate is suspended in weak alcohol, and submitted to the action of caustic soda: a solution is thus obtained, which, on the addition of water, deposits the new base in scaly crystals. They require only to be washed with water, to be redissolved in alcohol, and to be reprecipitated by addition of water.

Thus obtained, this substance presents itself in small white needles or scales, which are apt to assume a greyish tint on drying, very slightly soluble in boiling water, easily in alcohol and ether. This base fuses at 45°, and boils at 322°, distilling without the slightest alteration.

The numbers obtained in the analysis of this substance may be translated into the formula

$$C_{12}\,H_{11}\,N\,;$$

and this expression is unmistakeably corroborated by the examination of several well-defined saline compounds, more especially the sulphate, chloride, and nitrate.

Sulphate.—This salt, repeatedly mentioned, is remarkable for its very sparing solubility in cold, and even boiling water; the boiling aqueous solution deposits small needles of the composition

$$C_{24}\,H_{24}\,N_2\,SO_4 = \left.\begin{matrix} C_{12}\,H_{11}\,N \\ C_{12}\,H_{11}\,N \end{matrix}\right\}\,H_2\,SO_4.$$

This salt is somewhat more soluble in alcohol.

Nitrate.—Large white plates, moderately soluble in water, which are obtained by dissolving the base in dilute nitric acid, and recrystallizing the first crop of crystals from boiling water. Composition:

$$C_{12}\,H_{12}\,N_2\,O_3 = C_{12}\,H_{11}\,N,\,HNO_3.$$

The *chloride* is readily obtained in small white needles, somewhat more soluble in water than the sulphate. It was found to contain at 100°,

$$C_{12}\,H_{11}\,N,\,HCl.$$

The *platinum-salt* falls as a precipitate of a pale yellowish colour with a tint of grey, which under the microscope is found to consist

of small crystalline needles. This salt contains water of crystallization, which it does not lose even at 150°. Dried respectively at 110°, 120°, 130°, and 150°, it invariably exhibited the composition

$$C_{24}H_{26}N_2O, Pt_2Cl_6 = 2[C_{12}H_{11}N, HCl, PtCl_2] + H_2O.$$

The interpretation of the formula

$$C_{12}H_{11}N,$$

established by these well-concordant analyses, appeared to present no difficulties. The origin of the compound, although uncertain, is surrounded by phenylic associations; and nothing, in fact, could have been more natural than to consider the new base as *diphenylamine,*

$$C_{12}H_{11}N = \left.\begin{matrix} C_6H_5 \\ C_6H_5 \\ H \end{matrix}\right\} N;$$

this view, moreover, appeared to be countenanced by the deportment of the compound under the influence of iodide of ethyl. To secure at once the last term of ethylation, the base was repeatedly submitted in alcoholic solution to the alternate action of iodide of ethyl and oxide of silver. The product of the reaction was by appropriate treatment converted into a platinum-salt, which was found to be but slightly crystalline, insoluble in water, but readily soluble in alcohol. Both combustion and platinum-determination proved this salt to contain

$$C_{16}H_{20}NPtCl_3 = [C_{12}H_{10}(C_2H_5)_2N]Cl, PtCl_2.$$

This salt was decomposed by sulphuretted hydrogen, and successively treated with oxide of silver and iodide of ethyl, chloride of silver and dichloride of platinum, so as to produce in succession the chloride and oxide and, lastly, the iodide, chloride, and platinum-salt of a higher ethylated body. But the platinum-salt thus obtained was found to have still the same composition, which was, moreover, confirmed by the analysis of a fine *bromide,* crystallizing in prisms, difficultly soluble in water and ether, but easily soluble in alcohol, and a well-defined *iodide,* crystallizing in plates and having properties similar to those of the bromide. These salts contained respectively,

$$\text{Bromide } C_{16}H_{20}NBr = [C_{12}H_{10}(C_2H_5)_2N]Br, \text{ and}$$
$$\text{Iodide } C_{16}H_{20}NI = [C_{12}H_{10}(C_2H_5)_2N]I.$$

But before admitting these substances to be truly diethyl-dipheny-

lated ammonium compounds, it appeared desirable to prepare the oxide corresponding to these salts. On treating the alcoholic solution of the bromide or iodide with oxide of silver, a liquid was formed which showed *no* alkaline reaction, and which, on evaporation, deposited white needles, insoluble in water, moderately soluble in alcohol, easily soluble in ether, which fused at 100° and volatilized at a higher temperature without decomposition.

These are not the properties of a tetrasubstituted ammonium base; moreover the combustion led, instead of to the formula

$$C_{16}H_{21}NO = \left.\begin{matrix}[(C_6H_5)_2(C_2H_5)_2N] \\ H\end{matrix}\right\} O$$

flowing out of the above conception, to the expression

$$C_{16}H_{19}N,$$

incompatible with this conception, and revealing at once the true nature both of the original base and its ethylated derivative. The former is a primary, the latter a tertiary monamine:

$$\text{Original base} \quad . \quad . \quad C_{12}H_{11}N = \left.\begin{matrix}C_{12}H_9 \\ H \\ H\end{matrix}\right\} N;$$

$$\text{Ethylated derivative} \quad C_{16}H_{19}N = \left.\begin{matrix}C_{12}H_9 \\ C_2H_5 \\ C_2H_5\end{matrix}\right\} N.$$

To remove a last doubt which might have been entertained, it became desirable to prove that this latter base could fix another molecule of the iodide of an alcohol radical. Having failed with iodide of ethyl, I tried the action of iodide of methyl, which stands so much closer to hydriodic acid, and was delighted to find that the base is attacked by this compound, the product being an iodide which, when treated with oxide of silver, yielded a powerfully alkaline solution, possessing all the characters of the free ammonium bases. Converted into a chloride and precipitated by dichloride of platinum, this substance furnished a difficultly soluble platinum-salt crystallizing in needles, the combustion and platinum-determination of which gave numbers unequivocally fixing the formula

$$C_{17}H_{22}NPtCl_3 = [(C_{12}H_9)(C_2H_5)_2(CH_3)N]Cl, PtCl_2.$$

These results show how much preferable, on the whole, for fixing the degree of substitution in ammonias is iodide of methyl, although

the numbers which the methylated compounds furnish are of necessity less characteristic than those of the ethylated derivatives.

The new base, for which I propose the name Martylamine, is remarkable for the disinclination with which it goes through the series of performances which are generally expected from well-defined monamines. Chlorine, bromine, and oxidizing agents in general, give rise to the formation of dark-coloured compounds, which appear to possess but little tendency to crystallize. An exception is formed by its behaviour with nitrous acid. On passing this gas through an alcoholic solution, the liquid becomes warm, and soon solidifies into a mass of red crystals, which are insoluble in water, difficultly soluble in alcohol, easily soluble in ether, which contain

$$C_{24} H_{19} N_3 = (C_{12} H_9)_2 N'''H, N_2,$$

showing that this substance is formed by simple nitrogen substitution, which in this case links two molecules of martylamine together,

$$2\,C_{12} H_{11} N + HNO_2 = C_{24} H_{19} N_3 + 2\,H_2 O.$$

Treated with acids, this substance is easily reconverted into martylamine with simultaneous formation of an aromatic compound, which I intend to examine more minutely by-and-by.

I cannot at present offer any observation on the reaction which in the manufacture of aniline gives rise to the formation of the new base, although MM. Collin and Coblenz have most kindly furnished me with a detailed account of the several phases of their operations. It might, in fact, at the first glance appear to be waste of time to examine a compound which, however well defined, may owe its formation to a combination of conditions which are not easily realized again. My friends Mr. Nicholson and Mr. Perkin, of well-known experience in matters connected with the manufacture of aniline, have never observed this compound in their operations. Martylamine, nevertheless, is endowed with an interest of its own. Though of obscure origin, this body, from the place it occupies on the ladder of carbon, and from its very composition, establishes at once ties of parentage with some of the most distinguished families in the domain of organic chemistry. A glance at the formula of martylamine suffices to point out the close connexion of this compound with *benzidine*, the remarkable base obtained by Zinin from azobenzol, and the true constitution of which was lately made out in

my laboratory by Dr. P. W. Hofmann. These two compounds stand in the same relation to each other as ethylamine and ethylene-diamine, as phenylamine and phenylene-diamine:

$$\text{Martylamine} \left.\begin{matrix} C_{12}H_9 \\ H \\ H \end{matrix}\right\} N; \quad \begin{matrix}\text{Benzidine-} \\ \text{Martylene-diamine}\end{matrix} \left.\begin{matrix} (C_{12}H_8)'' \\ H_2 \\ H_2 \end{matrix}\right\} N_2.$$

This is not merely a relationship existing on paper; whoever has had these compounds in his hand will at once recognize the necessity of placing them side by side; but I may be allowed to point more particularly to the remarkable similarity of the deportment of benzidine under the influence of iodide of ethyl, this base exhibiting the same reluctance to pass from the state of tertiary substitution to the state of ammonium base—a passage which, in the case of benzidine, exactly as in the case of martylamine, had to be accomplished by means of iodide of methyl.

II. "On the Form of Crystals of Peroxide of Benzoyl." By William Hallows Miller, M.A., For. Sec. R.S., Professor of Mineralogy in the University of Cambridge. Received December 18, 1862.

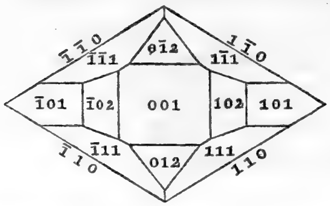
The peroxide of benzoyl, $C_{14}H_{10}O_4$, or carbon 69·42, hydrogen 4·13, oxygen 26·45, was discovered by Professor (now Sir B. C.) Brodie, and described by him in the 'Proceedings' of the Royal Society, vol. ix. p. 361. The crystals were obtained from a solution in ether of the product of the mutual decomposition of equivalent proportions of chloride of benzoyl and peroxide of barium mixed in water. The faces of the crystals, though brilliant, were not very even, so that, in order to obtain a moderately accurate result, it was necessary to measure a large number of crystals. The column headed 'observation' contains the means of the observed angles; the column headed 'calculation,' the most probable values of the angles, taking into account the quality of the faces containing them, and the number of the observations in each case.

System prismatic:—

$$1\,0\,1, 0\,0\,1 = 33^\circ\ 24'; \quad 1\,1\,0, 1\,0\,0 = 57^\circ\ 50'{\cdot}5.$$

Observed forms :—

0 1 0, 0 0 1, 0 1 2, 1 0 2, 1 0 1, 1 1 0, 1 1 1.



Angles.	Calculated.		Observed.	
	°	′	°	′
0 1 2, 0 1̄ 2	55	21	55	20
1 0 1, 1̄ 0 1	66	48	66	49
1 0 2, 1̄ 0 2	36	30		
0 1 0, 0 0 1	90	0		
0 0 1, 1 1 0	90	0		
1 1 0, 1̄ 1 0	64	19	64	18
1 1 1, 1̄ 1 1	48	56		
1 1 1, 1 1̄ 1	82	24		
1 1 1, 1 1 1̄	77	49		
0 0 1, 1 1 1	51	5	51	3
1 1 1, 0 1 2	30	26		
1 0 1, 0 1 2	42	19		
1 0 1, 1 1 1	41	12		
1 1 0, 0 1 2	66	51		
1 1 0, 1 0 1	72	58		

No cleavage observable.

The minimum deviations of the brightest part of the solar spectrum were observed through the faces 0 1 2, 0 1 2̄, through the faces 1 0 1, 1 0 1̄, and through the faces 1 1 0, 1 1̄ 0, the crystal being immersed in water contained in a vessel bounded by plates of glass parallel to the plane bisecting the dihedral angle formed by the refracting faces in each case. From these observations it appears that for a ray in the plane 1 0 0, and polarized in that plane, the index of refraction is about 1·837; for a ray in the plane 0 1 0, and polarized in that plane, the index of refraction is between 1·545 and 1·546; and for a ray in the plane 0 0 1, and polarized in that plane, the index of refraction is about 1·545. Hence the optic axes are in

the plane 0 1 0, and they make with each other a small angle which is bisected by the line [1 0 0].

A crystal having two opposite faces of the form 1 1 0 much larger than the two remaining faces, being immersed in oil for which $\mu=1{\cdot}4793$, and placed in a polarizing apparatus, the rings surrounding the optic axes were seen through the large faces of the form 1 1 0. The angle included between the directions of the optic axes within the oil was about 4°.

III. "On the Synthesis of Leucic Acid." By Dr. Edward Frankland, F.R.S. Received December 26, 1862.

When oxalic ether is mixed with more than its own weight of zincethyl, the temperature of the mixture slowly rises, and soon considerable quantities of gas begin to be evolved, unless the heat be moderated by plunging the vessel, in which the reaction takes place, into cold water. The gas consists of equal volumes of ethylene and hydride of ethyl; and as it is the product of a secondary decomposition, its evolution should be avoided as much as possible in the manner just indicated. The final application of a gentle heat completes the reaction.

The mixture generally continues fluid, but it becomes of a light straw-colour, and of an oily consistency. On being heated to 130° C. in a retort, no distillate passes over. If, after cooling, its own volume of water be very gradually added to it, torrents of hydride of ethyl, derived from excess of zincethyl, are evolved. By subsequent distillation in a water-bath, weak alcohol containing an ethereal oil in solution passes over; and a further quantity of the oil may be obtained by adding water to the residue in the retort, and continuing the distillation upon a sand-bath. The ethereal oil was precipitated from the alcoholic distillate by the addition of water, and was added to that which floated upon the surface of the aqueous distillate. It was then dried over chloride of calcium, and rectified. A very large proportion of the liquid distilled between 174° and 176° C., and was collected apart.

Numerous analyses of this liquid agree closely with the formula*

$$C\ H_{16}\ O_3.$$

* C=12, O=16.

The liquid is in fact the ethylic ether of an acid possessing the same composition as the leucic acid obtained by Strecker* in acting on leucin by nitrous acid. Upon the oxalic acid type its formula may be thus expressed:

$$C_2\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ O \\ HO \\ C_2H_5O. \end{array}\right.$$

Leucic ether cannot be directly derived from the action of zinc-ethyl upon oxalic ether, but it is produced when water is added to the result of that reaction. There can scarcely be a doubt that the body first formed is *zincoleucic ether*,

$$C_2\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ O \\ ZnO \\ C_2H_5O. \end{array}\right.$$

I have not succeeded in isolating this body; but, on the assumption of its production, the action of zincethyl upon oxalic ether may be thus formulated:

$$\underbrace{C_2\left\{\begin{array}{l} O \\ O \\ C_2H_5O \\ C_2H_5O \end{array}\right.}_{\text{Oxalic ether.}} + \underbrace{Zn_2\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \end{array}\right.}_{\text{Zincethyl.}} = \underbrace{C_2\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ O \\ ZnO \\ C_2H_5O \end{array}\right.}_{\text{Zincoleucic ether.}} + \underbrace{\left.\begin{array}{l} Zn \\ C_2H_5 \end{array}\right\} O.}_{\text{Ethylate of zinc.}}$$

In contact with water zincoleucic ether is decomposed with the formation of leucic ether and hydrated oxide of zinc:

$$\underbrace{C_2\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ O \\ ZnO \\ C_2H_5O \end{array}\right.}_{\text{Zincoleucic ether.}} + \left.\begin{array}{l} H \\ H \end{array}\right\} O = \underbrace{C_2\left\{\begin{array}{l} C_2H_5 \\ C_2H_5 \\ O \\ HO \\ C_2H_5O \end{array}\right.}_{\text{Leucic ether.}} + \left.\begin{array}{l} Zn \\ H \end{array}\right\} O.$$

Leucic ether is a colourless, transparent, and somewhat oily liquid, possessing a peculiar and penetrating ethereal odour and a sharp taste. It is insoluble in water, but readily soluble in alcohol or ether. Its specific gravity is ·9613 at 18°·7 C.; it boils at 175° C.,

* Ann. der Chem. u. Pharm., Bd. lxviii. 54.

and distils unchanged. A determination of the density of its vapour gave the number 5·241: the above formula, corresponding to 2 vols. of vapour*, requires the number 5·528.

When leucic ether is treated with solution of hydrate of baryta, it gradually dissolves even in the cold; on heating the solution in a water-bath, a liquid having the properties of alcohol distils off; and on separating the excess of baryta by carbonic acid and filtration, the solution yields, on evaporation, a crystallizable baryta-salt which, after drying at 100° C., gives on analysis numbers closely corresponding with the formula of leucate of baryta:

$$C_2\left\{\begin{matrix} C_2H_5 \\ C_2H_5 \\ O \\ HO \\ BaO. \end{matrix}\right.$$

Leucate of potash is similarly produced when leucic ether is treated with an aqueous solution of caustic potash. It separates as a semisolid soap upon the surface of the potash solution, if the latter be concentrated. All the salts of this acid appear to be soluble in water.

Leucic acid in solution is obtained when dilute sulphuric acid is added to leucate of baryta; the acid has a sour taste, reddens litmus strongly, and is readily soluble both in water and alcohol. It can be boiled with water without decomposition, and traces only of the acid distil off with the water.

So far as I have studied its properties, leucic acid thus obtained appears to be identical with that derived from leucin; but it will be necessary to establish this identity by a more rigorous comparison of the two acids.

The production of leucic acid from oxalic acid, as just described, obviously affords an insight into the molecular constitution of the class of organic acids of which lactic acid is the representative; I refrain, however, from offering any opinion upon a point which has already given rise to so many hypotheses, until I have completed the study of this reaction, and extended it to other homologous bodies.

* $H_2O = 2$ volumes.

IV. "On the Artificial Production of Fibrin from Albumen." By Alfred Hutchison Smee, Junior, Student of St. Bartholomew's Hospital. Communicated by W. S. Savory, Esq. Received January 15, 1863.

The condition in which fibrin exists in the blood and other fluids, and the deviation in quantity and quality in certain cases of disease from that of normal blood, has been to physiologists a subject of great interest. From the close resemblance of fibrin to albumen, I was induced to undertake a series of experiments, which appear to me to have some value in determining the conditions under which fibrin is derived from albumen, and which have resulted in the discovery of the general principle by which the direct conversion of albumen into fibrin may be effected. On referring to Lehmann's 'Chemistry,' in which the analyses of albumen and fibrin are quoted, it will be observed, on comparing them, that the difference appears to be the substitution of 1·5 part of oxygen per 100 for a similar amount of carbon, hydrogen, nitrogen, sulphur, phosphorus conjoined.

The following are the analyses quoted :—

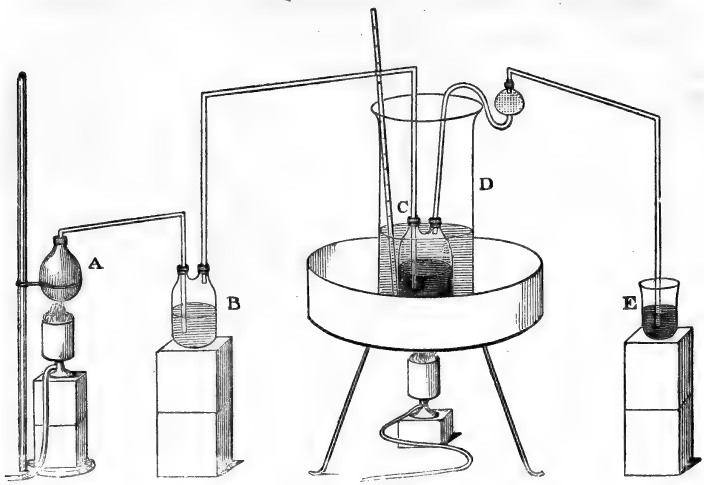
Albumen.		Fibrin.
53·5	Carbon	52·7
7·0	Hydrogen	6·9
15·5	Nitrogen	15·4
1·6	Sulphur	1·2
0·4	Phosphorus	0·3
22·0	Oxygen	23·5
100·0		100·0

The analyses made by Scherer give comparatively the same results.

From these analyses I was induced to make some experiments to endeavour to convert albumen into fibrin by the direct addition of oxygen gas, by which I anticipated that not only might the oxygen be imparted to the albumen, but also that the other elements might be oxidized and carried off.

In my first experiments I used blood from which the fibrin had been carefully whipped during the period of its coagulation, so that the serum might contain as many blood-cells as possible, upon the

supposition that the cells would afford a large amount of surface to the action of the gas.



The serum, after being whipped, was permitted to stand for twenty-four hours, that any fibrin which it might contain, and which had not coagulated during the process of whipping, might do so.

The apparatus used in all cases will be easily understood by referring to the annexed diagram. It consists, first, of a copper flask containing black oxide of manganese, from which the oxygen was slowly given off by the action of heat.

The gas was conveyed thence by tubes into a wash-bottle, B (containing a dilute solution of potass), for the absorption of impurities.

From the bottle B the gas passed into the flask C, which contained the defibrinated blood. This flask was placed in a vessel (D) containing water at a temperature varying between 95° and 100° Fahr.; and I had no difficulty in preserving that heat continuously by a small gas-flame placed under a sand-bath. After the gas had escaped from the blood, I generally passed it through a second portion of defibrinated blood contained in another vessel (E).

For all these experiments pig's blood was invariably chosen, on account of its richness in blood-cells.

My apparatus being ready, and oxygen being slowly given off, the whipped blood, from which every particle of fibrin had been previously removed, was introduced into the flask C.

The blood employed was arterial, and not venous. At first the bright scarlet colour of the blood increased somewhat, but after twelve hours the bright scarlet began to assume more the colour of venous blood: the cells at the same time began to shrivel. From this time the blood began rapidly to grow darker and darker, when, after thirty-six hours, it was almost black*.

Virchow has shown that, by acting on the hæmatin contained in the blood-cells by acetic acid, and subsequently boiling it, a substance is formed to which he gives the name of hæmine, which he considers to be a product in an intermediate stage between hæmatin and pigment. The black substance formed by oxidation may probably be found to be analogous to, if not identical with, Virchow's hæmin. I found in this blood, at the end of thirty-six hours, small masses, which had, under the microscope, the appearance of fibrin. A small portion of the same blood as that used in the experiment was set aside till the completion of the experiment, when it was examined, but no fibrin was found.

Likewise in the glass E, although the gas was passed through it without heat, no fibrin was found, proving that temperature had also an effect on the production of fibrin. This experiment was repeated many times: in all cases the blood assumed the black colour, but I did not invariably find fibrin.

The appearance of fibrin in some cases, and the non-appearance in many others, seemed at first sight to be inexplicable, though I shall be able to demonstrate, in a later portion of this paper, that the result may be explained upon the hypothesis that the alkaline salts were in relative excess in those cases where the fibrin did not appear.

In my next series of experiments, the white of an egg was added to about 4 oz. of the defibrinated blood. The egg-albumen at first had a tendency to separate and float at the top of the blood-serum, although well agitated together. In these cases the blood assumed the same dark colour as when it was subjected to experiment alone, though it did not appear until after the serum and egg-albumen had completely coalesced, which took place about ten hours after the subjection to temperatures between 95° and 100° Fahr., and the action of oxygen gas. At the end of thirty-six hours, the time when the experiment was stopped, masses of substances were found float-

* Crawford, about fifty years back, found that, after immersing animals in hot water, no difference could be discerned between arterial and venous blood.

ing, and also adhering to the bottom and sides of the vessel. These clots were in sufficient quantity to be collected and washed in the filter to free them from blood-cells and other impurities. The washed portion, under the microscope, had the distinct appearance of fibrin.

In cases where the albumen had not sufficient time to be mingled with the blood, little or no fibrin was formed. The time occupied for its absorption varied from about ten to twenty hours.

In the experiments which I conducted with albumen alone, I experienced at first some difficulty in obtaining the albumen perfectly pure, on account of the presence of chalazæ and other foreign matter. To obviate this difficulty, I found that, by adding one drop of glacial acetic acid to every white of egg employed, and then by well beating up the albumen, I obtained, on subsequent filtration, a clear solution which gave to litmus-paper a slightly acid reaction. On placing this transparent albumen in the ordinary apparatus, I found, after the passage of oxygen gas for four hours at the temperature before stated, that fibrin began to be formed. I found that, by placing coils of platinum wire in the albumen whilst undergoing oxidation, the formation of fibrin was not only greatly facilitated, but its subsequent separation from the rest of the albumen was accomplished with greater ease, as the fibrin hung in threads upon the platinum wire. When platinized platinum was used, the formation of fibrin, as might have been expected, was slightly improved.

Fibrin produced artificially in these experiments, and especially that formed on platinum wire, had a beautiful and regular arrangement, mostly being deposited in parallel lines. The fibrin likewise was whiter, and had a more delicate consistence than the common fibrin in blood.

I next tried the effect of adding a small quantity of a strong solution of ammonia to the albumen, which had naturally a slightly alkaline reaction; and then it was subjected to the influence of a current of oxygen in the same manner as in the preceding experiments. I found that fibrin was formed to a much smaller extent than when acid albumen was employed. The ammonia in all cases appears to be driven off to some extent by the oxygen, but was never entirely removed. The fibrin in this case formed on the surface of the liquid, and did not appear to be dissolved as the experiment was progressing.

It is worthy of particular observation, that fibrin was formed in the liquid which still contained ammonia in appreciable quantity.

My father suggested to me that it would be desirable to try the effect of the decomposition of water by electricity on albumen, as by that process the effect of hydrogen and oxygen in a nascent state is presented to different parts of the same fluid. For the purposes of this experiment I employed four cells (of the test-tube form) of Smee's battery, in which the negative pole consisted of a platinized platinum wire. This battery generated a continuous, but feeble, current of electricity; and the smallest perceptible bubbles were evolved from the platinized platinum wire when in operation for the experiment.

The albumen was placed in the decomposition trough, where a very large positive pole was employed, but a smaller negative one, and the temperature was maintained as in former experiments. After the passage of the electric current for some time, the positive pole of the decomposition cell was coated with a hard gelatinous mass, which, being immersed in water at 90° Fahr. for a few hours, unravelled itself into long fibres, which had, under the microscope, the appearance of fibrin.

On the negative pole, however, a frothy deposit alone was formed; but great care must be taken to stop the experiment before the products of the two poles grow together, to which they have a great tendency. The moment this takes place the albumen begins to coagulate, and in a very short time the whole becomes converted into an almost semisolid mass. The fibrin is not so perfect when made by this method, and is much more difficult to form than when made from neutral or slightly acid albumen by the ordinary process of oxidation.

In my experiments with egg-albumen to which a solution of potass had been added before it was subjected to the action of oxygen, the temperature ranging between 95° and 110° Fahr., no fibrin was found when the experiment was stopped.

In one case oxygen was passed through a solution of potass and albumen for three days and nights, and yet not the slightest trace of fibrin was found. The albumen became of a dark red hue, but two days after the experiment ceased it resumed its normal colour. A few transparent hard substances were found, insoluble in water and

weak acids which had separated from the albumen. A few other small white substances were noticed, which had all the appearance of carbonate of lime, and which were soluble in acid. Albumen was then mixed with gastric juice, and kept at the normal temperature of the body for the space of twelve hours, to produce artificial digestion, when it was subjected to experiment. I should here state that the gastric juice was procured from a dog, which had a fistulous opening made into its stomach by Professor Savory. All symptoms of inflammation and irritation had fully ceased; the dog, in fact, was in perfect health, and beginning to get fat, when the gastric juice was procured; so that the latter must be considered as healthy gastric juice. From the dog large quantities of gastric juice were obtainable; and I have to tender my best thanks to Professor Savory for his great kindness in placing whatever I required at my disposal. After the albumen had been digested for twelve hours and filtered, that the solution might be perfectly clear, it was subjected to the action of oxygen for a few hours, when fibrin was formed, though not in so large an amount as in albumen to which one drop of the glacial acetic acid had been added. The filaments of the fibrin, however, were of a more delicate constitution.

From a consideration of the above results, I thought that fibrin might be formed from the albumen which, after digestion with gastric juice, had passed through a membrane made of the parchment paper of Messrs. De la Rue and Gaines. In some experiments* to which I had been led from a study of Professor Graham's elegant researches on dialysis, and which I had formerly been conducting, on the passage of various fluids through membranes, it was observed that albumen, after digestion with gastric juice, dialysed to a certain extent. Three ounces of albumen were digested for the space of twelve hours, at the temperature of the body. It was then placed on the dialyser: it should be remarked that gastric juice does not coagulate the albumen during its conversion into albuminose.

The digested albumen was kept for ten hours on the dialyser at the temperature of 98° to 110° Fahr.

* These experiments, although carried on upon an extensive scale, are not quite in order for publication in detail; nevertheless I may state that, after artificial digestion, pure albumen, coagulated albumen, cheese, and, most remarkable of all, cod-liver oil were capable of passing through the dialyser into water to a large extent. I trust on a future occasion to elucidate this curious action.

The water (one and a half pint) into which the digested albumen had passed was concentrated at a temperature of not more than 80° Fahr.; and the concentrated solution being afterwards oxidized, I found that fibrin was formed, notwithstanding the changes it had undergone by digestion, which had rendered it capable of dialysis. During the process of passing oxygen into albumen, I found that carbonic acid was evolved. This was ascertained by passing the oxygen, after it had escaped from the albumen, through lime-water. I also found that phosphoric acid was evolved, by subjecting the effluent oxygen to the molybdate-of-ammonia test. Carbonic acid and phosphoric acid were also found when blood-serum was used, by the same tests as those employed when egg-albumen was the material used for experiment. In some cases common air was driven through albumen in the place of oxygen, at a temperature between 95° and 110° Fahr., and then I found the formation of fibrin differed but little from the quantity produced when oxygen alone was used. To ascertain whether the formation of fibrin was due really to oxygen alone, I tried hydrogen gas in the place of oxygen or common air, and at the same temperature. When hydrogen was passed into blood-cells sulphur was evolved. This was detected by passing the hydrogen, after it had traversed the serum, into a solution of lead-salt, and also by suspending over the serum strips of lead paper, when they soon became blackened by the sulphur.

When egg-albumen was employed instead of the blood-serum, sulphur was again detected.

Fibrin was not formed by the action of hydrogen on blood-serum or egg-albumen, although in some cases the hydrogen was passed continuously for forty-eight hours through the fluids. The action of carbonic acid gas on egg-albumen under the same condition of temperature produces no fibrin, but sulphur was again detected by suspending strips of lead paper over the albumen, which in a few hours became tinged.

The same result was obtained when defibrinated blood was used; but in this case, in addition to the sulphur, a minute trace of phosphoric acid was found. Not the slightest trace of fibrin was detected.

I conceived, from the result of my experiments on the oxidation of albumen, that, if oxygen was passed into milk, fibrin might be

formed, from the fact that the analyses of albumen of egg, and the casein which the milk contains, differ little from each other, and because the analysis of the milk of an animal, a few days before and after parturition, shows that albumen is found in the place of casein. On subjecting, however, milk to experiment, no fibrin was found after the lapse of twenty-four hours.

This may be due to either of two causes: first, the casein in the milk may not be in a fit state for undergoing the change before it has been acted on by the various digestive secretions, or, secondly, because in the dilute and fluid state in which it occurs in milk it does not offer sufficient resistance to the passage of the bubbles of oxygen to retain the gas sufficiently long for each bubble to have time to produce an effect. In all my experiments I have found (other conditions being equal) the slower the bubbles passed through the liquid material, and the more viscid the fluid was, the greater was the amount of fibrin produced. This may possibly in some degree account for the non-formation of fibrin when oxygen was passed through milk. I tried the effect of oxygen upon fresh grape-juice, but was unable to form any fibrin from it. Further experiments are required upon various vegetable juices.

I next experimented upon the oxidation of gluten, which was obtained from wheat-flour by the ordinary method. This was digested in gastric juice for twelve hours, and then filtered. After the clear liquid had been subjected to oxidation for some hours, small threads of a substance were formed. When a portion of this was placed under the microscope, no difference could be detected between it and ordinary fibrin.

From these experiments, it seems to me that the following conclusions may be drawn:—

First, that fibrin is produced by the direct action of oxygen on albumen.

Secondly, that the alkalies and alkaline salts prevent the appearance of fibrin when albumen is acted upon by oxygen.

Thirdly, that the formation of fibrin from albumen is accompanied by the evolution of sulphur, phosphorus, and carbonic acid.

Fourthly, That a temperature ranging between 98° and 110° Fahr. promotes the artificial formation of fibrin.

Fifthly, that the greatest amount of fibrin appears when the albumen is neutral or slightly acid.

Sixthly, that the viscidity of the material employed promotes the formation of fibrin.

Seventhly, that albumen, artificially digested in gastric juice, produces fibrin by its subsequent oxidation, even after dialysis.

Eighthly, that gluten dissolved in gastric juice, and then oxidized at the ordinary temperature, yields fibrin.

The formation of fibrin in the human body, and its relation to albumen, has long been a vexed question. I venture to put forward these experiments in connexion with this important and interesting inquiry.

V. "Note on the Spectrum of Thallium." By Professor WILLIAM ALLEN MILLER, M.D., LL.D., Treasurer and V.P.R.S. Received January 15, 1863.

My friend Mr. Crookes, the discoverer of the new metal thallium*, having kindly put into my hands a small quantity of the metal, which he believes to be chemically pure, I have been enabled to make some experiments upon its spectrum, the results of which may not be without interest to the members of the Royal Society.

Thallium, as is well known, when examined in the usual way by the spectroscope, yields a spectrum of remarkable simplicity, furnishing a single intense green line, the occurrence of which, as is familiar to chemists, led Mr. Crookes to the discovery of the metal, and suggested to him the name by which it is known. In order to try the effect of a progressively increasing temperature upon the spectrum furnished by the metal and its compounds, the following experiments were made.

* It has been made the subject of question abroad, whether Mr. Crookes or M. Lamy was the first to recognize the metallic nature of thallium, and thus to dispute the claim of Mr. Crookes to the full credit due to him for his investigation (with only about twenty grains of the element) of its leading characters where no previous clue existed to guide him. It may be sufficient to state in answer to this suggestion, that Mr. Crookes had exhibited it at the International Exhibition, and marked as *metallic* his scanty store, though in the form of a precipitate, in the beginning of May, unquestionably before M. Lamy had published anything relating to thallium.

Portions of metallic thallium, as well as of an alloy formed by fusing a bead of thallium upon the end of a platinum wire, and portions of the sulphate of the metal were introduced successively, first, into the flame of burning hydrogen, and then into the oxyhydrogen jet, and were in each case viewed by the spectroscope. As the temperature increased in intensity, the brilliancy of the thallium green line increased also, but no new lines made their appearance.

Two pieces of stout thallium wire were then arranged as electrodes to the secondary wire of an induction coil. A continuous torrent of sparks was maintained without melting the wires or producing very rapid oxidation, or volatilization of the metal; the light, however, was much whiter than its ordinary monochromatic character would have led us to expect. Mr. Crookes, who was with me during the experiments, projected the image of the points by means of a lens upon a distant white screen, when it was at once obvious that the extremities of the spark were of a fine green colour, whilst the flickering luminous arc, which filled up the interval, due chiefly to ignited air, was much whiter.

On viewing the sparks from the induction-coil by the spectroscope, several new lines, independently of well-marked air-lines, made their appearance. These lines were distinguished from air-lines by the peculiar character which distinguishes most metallic lines, viz. the much greater intensity of their extremities than of their central portions. Besides the usual intense line in the green, five others were particularly observable: first, a very faint one in the orange; next, two of nearly equal intensity in the green, more refrangible than Tlα, with a third much fainter, these three lines in the green being nearly equidistant; whilst, 5th, in the blue was a bright well-defined line: all these were strong at each extremity and evanescent in the central portions.

The induction-spark of thallium was then observed when produced in a current of hydrogen gas. The air-lines disappeared, the peculiar lines of hydrogen were very manifest, particularly the line in the red and one of the lines in the blue; whilst the new thallium lines were preserved, with the exception of the feeblest, though all were reduced in intensity.

Finally, a photographic impression of the thallium spectrum upon collodion was obtained by the method which I have described in a

paper communicated to the Royal Society in June last. An impression extending to about division 154 of the scale then adopted was obtained. This spectrum contains several very characteristic groups of lines; it recalls the features of the spectra of cadmium and zinc, and less strongly that of lead.

Measuring by the scale already adopted in my former paper, it is found that there are two strong groups of lines at about 103 and 106. At 116, 121, and 126 are three groups—the first two less intense than the third, which is of about the same strength as the earliest two. Several feebler pairs of dots follow, and the spectrum terminates rather abruptly with four nearly equidistant groups, commencing respectively at 136, 141, 145, and 151. The first of these groups is very strongly marked, the others are fainter, but of nearly equal intensity.

The remarkable way in which a spectrum at low temperatures so simple becomes increased in complexity, both in the visible and in the extra-visible portions, is of high interest considered in relation to the physical cause of these phenomena; and it is not without interest in a chemical sense, from its bearing upon the view supported by Dumas, that thallium belongs to the alkaline group. Potassium and sodium exhibit no new lines in the induction-spark, merely a diffuse light filling up the air-lines, and lithium but a single strong group at about 124. This physical character, added to the more purely chemical ones of the insolubility of the sulphide, the chromate, the iodide, the sparing solubility of the chloride, the phosphate, the oxalate, the ferrocyanide, the occurrence of a powerfully basic oxide, and of a higher feebly acid oxide, may therefore assist in showing the resemblance of thallium to silver or to lead, which latter metal in density, colour, softness, and external appearance it so closely simulates.

It would be easy to point out other particulars in which the properties of thallium are in strong contrast with those of the alkali metals. The chemical energy of these metals, lithium, sodium, potassium, rubidium, and cæsium, increases in the order mentioned, which is that of their equivalents. Thallium, with a higher equivalent than any of these, shows a greatly diminished chemical activity. The metal is readily reduced by zinc from its solutions. Its oxide, instead of being like that of all the alkalies, excessively deliquescent, is perma-

nent in air, and forms a closely adhering coat like that which is produced upon the surface of zinc or lead, protecting the metal beneath from further change.

In many points the chemical reactions of thallium resemble those of silver, to which metal it is also further approximated by the circumstance that the atomic heat of the metal, like that of silver, is double that of the series to which lead belongs. Although therefore in other physical properties thallium differs greatly from silver, it seems to be more closely allied to that metal than to any other.

January 22, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read :—

I. "Researches on some of the Artificial Colouring Matters.—No. I. On the Composition of the Blue Derivatives of the Tertiary Monamines derived from Cinchonine." By A. W. HOFMANN, LL.D., F.R.S. Received December 18, 1862.

The chemical visitors of the International Exhibition will not easily forget the magnificent collection of products displayed in the French court by M. Menier of Paris. Among these compounds, equally remarkable for their variety and beauty, the large crystals of cyanine, rivalling in splendour and purity Mr. Nicholson's acetate of rosaniline, have attracted general attention. M. Menier, who has produced this new dye on a very large scale, has most liberally placed at my disposal some of the finest of these crystals for examination, hoping that their more minute investigation might perhaps lead to a method of giving solidity to this new colour, which in brilliancy and purity of tint is second to none of the several blues lately derived from coal-tar. The composition of cyanine and its mode of formation having hitherto remained unknown, I have gladly availed myself of this opportunity of performing some experiments with this interesting compound. I am sorry to say that, in a practical point of view, these experiments have failed entirely; but my studies have led me to some observations on this substance which, as a contribution to the history of cyanine, deserve to be recorded, and which I beg leave to communicate to the Royal Society.

The discovery of the blue compounds from chinoline and its homologues dates as far back as 1856. In that year Mr. G. Williams engaged in a renewed examination of the base extracted by Runge from coal-tar and obtained by Gerhardt from the alkaloids of the cinchona bark, the identity in composition of which I had established in one of my earlier researches. Among the numerous compounds of these bases most carefully examined by Mr. G. Williams on this occasion, were also their methylated and ethylated derivatives, one of which, the iodide of methyl-leucolylammonium, I had discovered when studying the action of iodide of methyl upon ammonia and its analogues. It was in preparing this compound from the chinoline obtained by the distillation of cinchonine, and in separating the ammonium-base corresponding to the iodide by means of oxide of silver, that Mr. Williams first observed the splendid coloration which has led him to the discovery of the new dye now commercially known under the name of cyanine. Precisely similar phenomena were subsequently (in 1857) observed by M. von Babo, who produced them by treating chinoline with the sulphates of methyl and ethyl, and described the coloured substances thus obtained as methylirisine and ethylirisine. Mr. Williams was inclined to attribute the formation of the blue compounds, in which he recognized distinctly basic properties, to a process of oxidation ; M. von Babo represents his methyl- and ethylirisine, although with very great reserve, by the formulæ

$$C_{11} H_{14} N_2 O_2 \quad \text{and} \quad C_{13} H_{18} N_2 O_2.$$

No attempt has since been made to establish the composition of these singular compounds by a more minute examination. In fact several years elapsed without any further notice being taken of them, until the development of the aniline industry revived the memory of these remarkable colour phenomena, which have since attracted the general attention of dyers and printers. Mr. G. Williams showed that, among the several coloured compounds produced by the action of iodides of alcohol radicals upon chinoline bases, the one obtained by means of iodide of amyl is particularly rich in tinctorial power ; he has given a very interesting account of this new dye, and accurately described the mode of manufacture of this body, which, under the name of cyanine, soon became an article of commerce.

Unfortunately the tint produced by cyanine is less fast than beautiful, and the hopes entertained of the industrial future of the new

compound has not been realized; nevertheless the importance attached by dyers to Mr. Williams's discovery is well marked by the fact of a gold medal, together with a prize of 10,000 francs, having been proposed for the discovery of a means of rendering stable the beautiful colours dyed by cyanine.

The crystals submitted to me for examination by M. Menier were distinct prisms, sufficiently well formed for crystallographical determinations. They are at present in the hands of Quintino Sella. Their substance possesses a beautiful green metallic lustre with a golden tint, by which, as well as by crystalline form, they are readily distinguished from acetate of rosaniline, which they in other respects much resemble. The crystals are insoluble in anhydrous ether, difficultly soluble in water, but dissolve readily in alcohol. The solution has a magnificent blue colour, with a coppery iridescence on its surface. Addition of acids destroys this colour. Ammonia and the fixed caustic alkalies leave the colour apparently untouched; but it is now produced by a finely divided deep-blue precipitate suspended in the liquid, which may be filtered off, the filtrate separated from it being colourless.

The green crystals were found to be the iodide of a peculiar basic compound. The iodine is rather firmly held in this compound; but it may be precipitated from the alcoholic solution by oxide of silver, and exchanged for bromine or chlorine by treatment of this solution with bromide or chloride of silver, when the bromide or chloride corresponding to the iodide are produced. The analysis of the crystals gave results indicating unequivocally the formula

$$C_{30}H_{39}N_2I,$$

which received a close confirmation by the examination of a fine platinum-salt crystallizing in rhombic tablets, which is obtained by precipitating the chloride corresponding to the iodide, strongly acidulated with hydrochloric acid, by dichloride of platinum. Nevertheless slight discrepancies between the theoretical values of the formula and the results obtained led me to assume the existence in the crystals of a compound containing less carbon and hydrogen, indeed of a homologous iodide,

$$C_{28}H_{35}N_2I.$$

This hypothesis, not countenanced at first by the remarkable constancy which the composition of the iodide presented even after three

or four crystallizations, was fully confirmed when the chloride was submitted to a systematic partial precipitation by dichloride of platinum. After several repetitions of the process, the partially precipitated platinum-salt being decomposed by sulphuretted hydrogen and the chlorides again partially precipitated, two platinum-salts were obtained, one of which, the less soluble one, proved to be the pure platinum-salt corresponding to the iodide with 30 equivalents of carbon, whilst the other one was sufficiently pure to show that it belonged in reality to the homologous iodide with 2 equivalents of carbon less.

The amount of the iodide

$$C_{28}\,H_{35}\,N_2\,I,$$

which contaminated (if the term may be applied to so beautiful a substance) the iodide

$$C_{30}\,H_{39}\,N_2\,I,$$

is, however, so small that its presence did not materially influence the analytical results obtained in the further examination of the compound.

The explanation of the formation of the iodide presents no difficulty; this substance obviously derives from lepidine,

$$C_{10}\,H_9\,N,$$

whilst only the slight admixture is due to the presence, in the original bases submitted to the action of iodide of amyl, of a small quantity of chinoline,

$$C_9\,H_7\,N.$$

In fact Mr. Williams, in describing the preparation of his dye, distinctly states that the chinoline by no means requires to be pure for the purpose. M. Menier has moreover kindly furnished me with a considerable quantity of the crude material from which the green crystals are obtained. This proved to be a mixture of several bases, in which the presence of lepidine and chinoline was traced without the slightest difficulty, by the analyses of platinum-salts.

In the genesis of the new iodide two different phases have to be distinguished, viz., 1, the transformation of lepidine into iodide of amyllepidyl-ammonium,

$$\underbrace{C_{10}\,H_9\,N}_{\text{Lepidine.}} \quad + \quad \underbrace{C_5\,H_{11}\,I}_{\text{Iodide of amyl.}} \quad = \quad \underbrace{C_{15}\,H_{20}\,N\,I}_{\text{Iodide of amyl-lepidyl-ammonium.}};$$

2, the condensation under the influence of potash of two molecules of the compound into one molecule of a higher order,

$$\underbrace{2(C_{15}\,H_{20}\,N\,I)}_{\text{Iodide of amyl-lepidyl-ammonium.}} \quad + \quad K\,H\,O = \underbrace{C_{30}\,H_{39}\,N_2\,I}_{\text{New iodide.}} + K\,I + H_2\,O.$$

It became indispensable to verify these reactions by the analysis of additional compounds.

The green crystals dissolve with facility in boiling dilute hydriodic acid; the colourless solution deposits on cooling yellow needles of remarkable beauty, the analysis of which has furnished the values of the formula

$$C_{30}\,H_{40}\,N_2\,I_2 = C_{30}\,H_{39}\,N_2\,I,\,H\,I.$$

These crystals are isomeric with iodide of amyl-lepidyl-ammonium, from which, however, they are distinguished by all their properties They dissolve in cold water without decomposition, but on addition of alcohol they immediately assume a blue coloration, the original monacid compound being reproduced. The same change takes place at 100°; so that in preparing the compound for analysis it was necessary to dry it *in vacuo*. In the facility with which the diacid compounds are converted into the monacid salts, this substance resembles rosaniline, which, as I have pointed out in a recent paper, forms likewise colourless acid salts of little stability.

The green iodide dissolves with equal facility in hydrochloric and hydrobromic acid, yielding perfectly colourless solutions, and giving rise to the formation of well-crystallized compounds, which contain, in addition to iodine, respectively bromine and chlorine.

On submitting the green iodide in alcoholic solution to the action of chloride of silver, the whole of the iodine is separated in the form of iodide of silver, a blue solution being obtained from which the monacid chloride crystallizes, on slow evaporation, in green metal-lustrous sharply-defined prisms of surpassing beauty. This salt was found to contain

$$C_{30}\,H_{39}\,N\ Cl.$$

Dissolved in hydrochloric acid, this salt furnished a diacid compound which, on evaporation *in vacuo*, separates in long straw-coloured needles. The highly deliquescent character of this substance has

hitherto prevented me from analysing it; but if there was the slightest doubt of this compound having the composition

$$C_{30}\,H_{40}\,N_2\,Cl_2 = C_{30}\,H_{39}\,N_2\,Cl,\,H\,Cl,$$

it would be dispelled by the analysis of a fine-yellow difficultly soluble platinum-salt crystallizing in small well-defined rhombic plates, which falls directly on addition of dichloride of platinum to the alcoholic solution of the diacid chloride, containing a considerable amount of hydrochloric acid, and which, by analysis, was found to be represented by the formula

$$C_{30}\,H_{40}\,N_2\,Cl_2,\;2\,Pt\,Cl_2.$$

The gold-salt is obtained by precipitating the solution of the acid chloride with trichloride of gold, when a yellow, scarcely crystalline precipitate is formed, which, dried *in vacuo*, contains

$$C_{80}\,H_{40}\,N_2\,Cl_2,\;2Au\,Cl_3.$$

I have, moreover, prepared the monacid bromide, which forms beautiful metal-lustrous prisms easily crystallizable; the diacid nitrate as a crystalline network, on evaporating a solution of the base in nitric acid *in vacuo*; and, lastly, the acid sulphate, which crystallizes in white, well-formed rhombic tables, very soluble in water, but insoluble in alcohol, by which it is not decomposed like the other diacid compounds.

I have refrained from multiplying the analytical evidences by the minute examination of these salts, because I was happy enough to observe a reaction which supported the interpretation of the results of analysis in an unequivocal manner. Remembering the simple scission which I had formerly accomplished by exposing the iodide of tetrethylammonium to the action of heat, when the compound splits into iodide of ethyl and triethylamine, I was induced to submit the green iodide to distillation. The green crystals rapidly fuse into a blue liquid, the surface of which presents a peculiar coppery lustre. On raising the temperature, decomposition takes place, and in the receiver is condensed a mixture of lepidine and iodide of amyl, the reunion of which to iodide of amyl-lepidyl-ammonium may be prevented by collecting them in hydrochloric acid; at the same time a gas is evolved, burning with a brilliant flame and readily absorbed by bromine, and which could easily be condensed by passing it through a serpentine surrounded with ice. I was thus enabled to collect

enough of the volatile hydrocarbon to determine its boiling-point, which proved it to be pure amylene. If the heat be carefully regulated, the amount of charcoal remaining in the retort is comparatively small. The interpretation of the phenomena observed is given in the following equation :—

$$\underset{\text{Green iodide.}}{C_{30}H_{39}N_2I} = \underset{\text{Lepidine.}}{2C_{10}H_9N} + \underset{\text{Iodide of amyl.}}{C_5H_{11}I} + \underset{\text{Amylene.}}{C_5H_{10}}$$

Here, again, I have had an opportunity of proving the presence in the crystals of a small quantity of the homologous chinoline compound; for on submitting, after separating the iodide of amyl, the hydrochlorate of the volatilized base to distillation with potassa, and collecting apart the first quantity of the basic liquid which came over with the vapour of water, this substance proved by the platinum determination to be chiefly chinoline, while the portion of the base distilling last proved by the same mode of analysis to be pure lepidine.

The results obtained in these experiments furnish new illustrations of the tendency to molecular accumulation by which the ammonias and their derivatives are distinguished. Only a few weeks ago I had the honour of submitting to the Royal Society a short account of this class, which is obtained as a secondary product in the manufacture of aniline. The coloured derivatives of the bases of the chinoline series present in their composition considerable analogy with paraniline.

Aniline series.

$$\underset{\text{Aniline.}}{C_6H_7N} \qquad \underset{\text{Chloride.}}{C_6H_7N, HCl}$$

$$\underset{\text{Paraniline.}}{\begin{matrix} C_6H_7N \\ C_6H_7N \end{matrix}} \qquad \underset{\text{Monacid chloride.}}{\left.\begin{matrix} C_6H_7N \\ C_6H_7N \end{matrix}\right\} HCl} \qquad \underset{\text{Diacid chloride.}}{\left.\begin{matrix} C_6H_7N \\ C_6H_7N \end{matrix}\right\} \begin{matrix} HCl \\ HCl \end{matrix}}$$

Lepidine series.

$$\underset{\text{Hydrated oxide of amyl-lepidyl-ammonium.}}{C_{15}H_{19}NH_2O} \qquad \underset{\text{Chloride.}}{C_{15}H_{19}N, HCl}$$

$$\underset{\text{Free blue base.}}{\left.\begin{matrix} C_{15}H_{19}NH_2O \\ C_{15}H_{19}NH_2O \end{matrix}\right\} ?} \qquad \underset{\text{Green iodide, monacid.}}{\left.\begin{matrix} C_{15}H_{19}N \\ C_{15}H_{19}N \end{matrix}\right\} HCl} \qquad \underset{\text{Yellow iodide, diacid.}}{\left.\begin{matrix} C_{15}H_{19}N \\ C_{15}H_{19}N \end{matrix}\right\} \begin{matrix} HCl \\ HCl \end{matrix}}$$

I have written the formulæ of the coloured compounds so as to bring out their analogy with the paraniline salts—in fact, so as to

characterize them as para-compounds of the amyl-lepidyl-ammonium salts, but I am far from attributing to these formulæ any other value. In fact the molecular construction of this new class of compounds remains to be established by further experiments.

The theory which (in 1852) satisfactorily represented the constitution of the nitrogen bases then examined, requires an expansion to include the tinctorial ammonias added to our knowledge during the last decade. The time for the enunciation of this amplified theory has not yet arrived.

Here only a few experiments may still be mentioned, which were made with the oxide corresponding with the salts described.

The action of oxide of silver upon the iodide dissolved in alcohol liberates the base, which, on evaporation of the alcohol, separates as an indistinctly crystalline deep-blue mass, moderately soluble in water, less soluble in anhydrous ether, easily soluble in alcohol. Ether precipitates the base from its alcoholic solution; I have not examined it.

Submitted to distillation, the free oxide gives rise to an oily base, which I naturally expected to be lepidine; but the experiments which I have hitherto made with this substance appear to negative this assumption. I have undertaken a more minute examination of the compound, because, if it be different, its study will probably throw some light upon the still uncertain constitution of the tertiary bases of the chinoline series, which I have frequently attempted to decipher. It remained now only for me to examine the mode of formation of the remarkable compound the nature of which I have endeavoured to clear up. With this view I have studied the action of iodide of methyl and amyl upon chinoline and lepidine, large quantities of which were kindly placed at my disposal by my friend Mr. David Howard. The products obtained in this reaction I have not submitted to a minute examination, having satisfied myself that their principal phases are well illustrated by the equations which I have given for the formation of the substances produced by the action of iodide of amyl upon lepidine. Nor have I followed out in detail the complicated secondary changes, and more especially the generation of the red colouring matter which is abundantly formed in these reactions. I have nothing to add to the perfect description of these phenomena by the distinguished discoverer of this pigment.

In conclusion I may be allowed to express my best thanks to M. Menier: without the magnificent crystals furnished by his ateliers, I could not have even attempted to clear up this question.

Though proud of her office as guide of industry, science acknowledges without blushing that there are territories on which she cannot advance without leaning on the strong arm of her powerful companion. Joint labours of this kind cannot fail to seal the pledge of alliance between industry and science.

II. "On some new Compounds obtained by Nitrogen-substitution, and new Alcohols derived therefrom." By PETER GRIESS, Esq. Communicated by Dr. HOFMANN. Received December 18, 1862.

In the beginning of this year (1862) I pointed out* that diazo-amidobenzol, when submitted to the action of nitric acid containing nitrous acid in solution, is transformed into a new compound according to the equation

$$\underbrace{C_{12}H_{11}N_3}_{\text{Diazo-amidobenzol.}} + 2\,HNO_2 + HNO_3 = 2\underbrace{(C_6H_4N_2,\ HNO_3)}_{\text{New compound.}} + 2\,H_2O.$$

I have now found that this remarkable compound, the nitrate of diazobenzol, can be much more easily produced by the action of nitrous acid upon nitrate of aniline,

$$\underbrace{C_6H_7N,\ HNO_3}_{\text{Nitrate of aniline.}} + HNO_2 = \underbrace{C_6H_4N_2,\ HNO_3}_{\text{Nitrate of diazobenzol.}} + 2\,H_2O.$$

This process has furnished me a considerable number of similarly constituted nitrogen-substituted derivatives, not only of monacid monamines, but also of diamines; and it is to some of the bodies generated by means of the latter that I beg leave to call the attention of the Royal Society.

If a current of nitrous acid be passed into a cold solution of the nitrate of benzidine, a base which, by the researches of P. W. Hofmann, has been characterized as a well-defined diacid diamine, a new compound is produced, crystallizing from water in white needles,

* Ann. Chem. Pharm.

explosive like fulminate of mercury, the composition of which was established by the analysis of a platinum-salt containing

$$C_{12}H_6N_4, 2HCl, 2PtCl_2.$$

The formation of this new substance is illustrated by the following equation:—

$$\underbrace{C_{12}H_{12}N_2+2HNO_3}_{\text{Nitrate of benzidine.}}+2HNO_2=\underbrace{C_{12}H_6N_4, 2HNO_3}_{\text{New compound.}}+4H_2O.$$

Of subordinate interest themselves, these substances deserve to be noticed on account of the numerous and often peculiar bodies arising from their decomposition. Thus the tetrazo-compound just described, when boiled with water, splits according to the equation

$$\underbrace{C_{12}H_6N_4, 2HNO_3}_{\text{Nitrate of tetrazo-compound.}}+2H_2O=\underbrace{C_{12}H_{10}O_2}_{\text{New substance.}}+N_4+2HNO_3.$$

The new non-nitrogenous substance thus obtained crystallizes in small sublimable plates. Both formula and properties characterize it as a compound standing, like phenol, upon the boundary line between acids and alcohols: it furnishes a very extensive series of derivatives, which may be generally represented by the formula

$$2\,\text{Phenyl-body}-2H=\text{new compound}.$$

Here I will only mention the chloride corresponding to the new alcohol (acid). It crystallizes in white volatile plates, which may be readily prepared by heating the above-mentioned platinum-salt with carbonate of sodium. The reaction takes place at 100°.

$$C_{12}H_6N_4, 2HCl, 2PtCl_2=C_{12}H_8Cl_2+2PtCl_2+4N.$$

In conclusion, I may be allowed to state that nitrate of naphthylamin likewise yields an azo-compound. This compound,

$$C_{10}H_6N_2HNO_3$$

when submitted to the action of boiling water, undergoes a transformation analogous to that of nitrate of diazobenzol,

$$\underbrace{C_6H_4N_2HNO_3}_{\text{Nitrate of diazobenzol.}}+H_2O=\underbrace{C_6H_6O}_{\text{Phenol.}}+2N+HNO_3,$$

$$\underbrace{C_{10}H_6N_2HNO_3}_{\text{Nitrate of diazonaphthol.}}+H_2O=\underbrace{C_{10}H_8O}_{\text{New compound.}}+2N+HNO_3.$$

I have not yet analysed this new compound; but both mode of formation and properties (it crystallizes in white very fusible needles, possessing the odour of creosote) leave no doubt that it is the alcohol of the naphthaline series which has so long eluded the researches of chemists.

III. "On the Differential Equations of Dynamics. A sequel to a Paper on Simultaneous Differential Equations." By GEORGE BOOLE, F.R.S., Professor of Mathematics in Queen's College, Cork. Received December 22, 1862.

(Abstract.)

Jacobi in a posthumous memoir*, which has only this year appeared, has developed two remarkable methods (agreeing in their general character, but differing in details) of solving non-linear partial differential equations of the first order, and has applied them in connexion with that theory of the differential equations of dynamics which was established by Sir W. R. Hamilton in the 'Philosophical Transactions' for 1834–35. The knowledge, indeed, that the solution of the equation of a dynamical problem is involved in the discovery of a single central function, defined by a single partial differential equation of the first order, does not appear to have been hitherto (perhaps it will never be) very fruitful in practical results. But in the order of those speculative truths which enable us to perceive unity where it was unperceived before, its place is a high and enduring one.

Given a system of dynamical equations, it is possible, as Jacobi had shown, to construct a partial differential equation such that from any complete primitive of that equation, *i. e.* from any solution of it involving a number of constants equal to the number of the independent variables, all the integrals of the dynamical equation can be deduced by processes of differentiation. Hitherto, however, the discovery of the complete primitive of a partial differential equation has been supposed to require a previous knowledge of the integrals of a certain auxiliary system of ordinary differential equations; and

* Nova methodus æquationes differentiales partiales primi ordinis inter numerum variabilium quemcunque propositas integrandi. (Crelle's Journal, vol. lx. p. 1.)

in the case under consideration that auxiliary system consisted of the dynamical equations themselves. Jacobi's new methods do not require the preliminary integration of the auxiliary system. They require, instead of this, the solution of certain systems of simultaneous linear partial differential equations. To this object, therefore, the method developed in my recent paper on "Simultaneous Differential Equations" (Philosophical Transactions for 1862) might be applied. But the systems of equations in question are of a peculiar form. They admit, in consequence of this, of a peculiar analysis. And Jacobi's methods of solving them are in fact different from mine, though connected with it by remarkable relations. He does indeed refer to the general problem of the solution of simultaneous partial differential equations, and this in language which does not even suppose the condition of linearity. He says, "Non ego hic immorabor quæstioni generali quando et quomodo duabus compluribusve æquationibus differentialibus partialibus una eademque functione satisfieri possit, sed ad casum propositum investigationem restringam. Quippe quo præclaris uti licet artificiis ad integrationem expediendam commodis." But he does not, as far as I have been able to discover, discuss any systems of equations more general than those which arise in the immediate problem before him.

It is only very lately that I have come to understand the nature of the relation between the general method of solving simultaneous partial differential equations, published in my recent memoir, and the particular methods of Jacobi. But in arriving at this knowledge I have been led to perceive how, by a combination of my own method with one of those of Jacobi, the problem may be solved in a new and perhaps better, certainly a remarkable way. This new way forms the subject of the present paper*. Before proceeding to explain it, it will be necessary to describe Jacobi's methods, to refer to my own already published, and to point out the nature of the connexion between them.

The system of linear partial differential equations being given, and it being required to find a simultaneous solution of them, Jacobi, according to his first method, transforms these equations by a change of variables; he directs that an integral of the first equation be found;

* It was stated by me, but without demonstration, at the Meeting of the British Association in Cambridge in October of the present year (1862).

he shows that, in virtue of the form of the equations and the relation which connects the first and second of them, other integrals of the first equation may be derived by mere processes of differentiation from the integral already found; and he shows how, by means of such integrals of the first equation, a *common* integral of the first and second equations of the system may be found. This common integral is a function of the known integral and certain variables, and its form is obtained by the solution of a differential equation between two variables—a differential equation which is in general non-linear, and of an order equal to the total number of integrals previously found.

An integral of the first two equations of the given system having been obtained, Jacobi shows that by a second process of derivation, followed by the solution of a second differential equation, an integral which will satisfy simultaneously the first three equations of the system may be found; and thus he proceeds by alternate processes of derivation and integration till an integral satisfying all the equations of the given system together is obtained. In these alternations, it is the function of the processes of derivation to give new integrals of the equations already satisfied; it is the function of the processes of integration to determine the functional forms by which the remaining equations may in their turn be satisfied.

Jacobi's second method does not require a preliminary transformation of the equations; but the process of derivation, by which from an integral of the first equation other integrals are derived by virtue of the relation connecting the first and second equations, is carried further than in his first method. It is indeed carried on until no new integrals arise. The difference of result is, that the common integral of the first and second partial differential equations is determined as a function solely of the integrals known, and not as a mixed function of integrals and variables. But its form is determined, as before, by the solution of a differential equation. All the subsequent processes of derivation and integration are of a similar nature.

On the other hand, the method of my former paper applied to the same problem leads, by a certain process of derivation, to a system of ordinary differential equations equal in number to the number of possible integrals, and, without being individually exact, susceptible of combination into exact differential equations. The integration of these would give all the common integrals of the given system.

All these methods possess, with reference to the requirements of the actual case, a superfluous generality. A single common integral of the system is all that is required.

Now the chief result to be established in this paper is the following.

If, with Jacobi, according to his second method, we suppose one integral of the untransformed first partial differential equation to be found, if by means of this we construct according to a certain type a new partial differential equation, if to the system thus increased we apply the process of my former paper, continually deriving new partial differential equations until, no more arising, the system is *complete,* then, under a certain condition hereafter to be explained, a common integral of all the equations of the complete system, and therefore of the original system which is contained in it, may be found by the integration of a single differential equation susceptible of being made integrable by means of a factor.

But if the condition referred to is not satisfied, a new integral of the first partial differential equation must be found and the process repeated, with the certainty that sooner or later it will succeed.

As soon, then, as the condition is satisfied, a solution not, as by Jacobi's methods, first of two of the partial differential equations of the given system, then of three, and so on, but of all at once, is obtained; and this solution is obtained, not as in my former process, by the solution of a system of equations reducible to the exact differential form, but by that of a *single* differential equation reducible to that form.

The condition in question is grounded on the theoretical connexion which exists between the process of derivation of partial differential equations involved in my former method, and the process of derivation of integrals involved in Jacobi's methods. In the actual problem, and in virtue of the peculiar form of the partial differential equation given, these two processes are coordinate. If each be carried to its utmost extent, then to each new partial differential equation arising from the one will correspond a new integral (of the first partial differential equation) arising from the other. The theory now to be developed is founded upon the inquiry whether it is possible to satisfy the completed system of partial differential equations by a function of the *completed* system of the Jacobian integrals, *i. e.* to determine a common integral of the completed series of equations as a function of

the completed series of integrals of the first equation. The reader is reminded that by the completed series of integrals is meant, not all the integrals of the first partial differential equations that exist, but all that arise from a certain root integral by a certain process of derivation, together with the root integral itself. Now the answer here to be established to this inquiry is the following. The first of the partial differential equations necessarily *will*, and others *may*, be satisfied by the proposed function irrespectively of its *form*. If the number of equations of the completed system which is *not* thus satisfied is odd (this is the condition in question), the form of the function which will satisfy all is determinable by the solution of a single differential equation of the first order, capable of being made integrable by means of a factor.

Although the direct subject of this paper is the solution of partial differential equations of the first order, I wish it rather to be received as a slight contribution to that theory of the dynamical equations which was first published in the Philosophical Transactions, and which suggested to Jacobi the line of investigation which I here only seek to pursue a little further.

January 29, 1863.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

Mr. Henry John Carter was admitted into the Society.

The following communication was read:—

"On the Absorption of Gases by Charcoal.—No. I." By Dr. R. Angus Smith, F.R.S. Received December 27, 1862.

(Abstract.)

The following is a summary of the author's observations:—

1. Charcoal absorbs oxygen so as to separate it from common air, or from its mixtures with hydrogen and nitrogen, at common temperatures.

2. Charcoal continues the absorption of oxygen for at least a month, although the chief amount is absorbed in a few hours, sometimes in a few seconds, according to the quality of the charcoal.

3. It does not absorb hydrogen, nitrogen, or carbonic acid for the same period.

4. Although the amount absorbed is somewhat in the relation of the condensibility of the gases by pressure, this is not the only quality regulating the absorption, of oxygen at least.

5. When it is sought to remove the oxygen from charcoal by warmth, carbonic acid is formed, even at the temperature of boiling water, and slowly even at lower temperatures.

6. Charcoals differ extremely in absorbing power, and in the capacity of uniting with oxygen, animal charcoal possessing the latter property in a greater degree than wood-charcoal.

7. Nitrogen and hydrogen, when absorbed by charcoal, diffuse into the atmosphere of another gas with such force as to depress the mercury three-quarters of an inch.

8. Water expels mercury from the pores of charcoal by an instantaneous action.

9. The action of porous bodies is not indiscriminate but elective.

Theoretical Considerations.

1. The elective nature of porous bodies may be closely allied to three properties:—

a. The condensibility of the gases.

b. The attraction and perhaps inclination to combine.

c. The capacity of combination.

2. In either case the attraction which results in condensation of the gas is exercised at distances greater than the distances of atoms or molecules in combination.

3. The gases in porous bodies lie in strata, the outside and more distant being less attracted than the atoms nearer the solid body.

4. We cannot separate chemical from physical attraction; but attraction may exist without its ultimate result (combination), which is distinctly chemical.

5. It is exceedingly probable that as physical attraction moves onwards to chemical combination, it produces the phenomena which have been attributed to so-called masses.

Chemical affinity is supposed to involve an attraction which is purely chemical; we have no proof of any such attraction as a separate power, we have only a proof of the combination. Attraction

may exist without the capacity of combining chemically, or, in other words, without chemical affinity. Chemical affinity (a very inappropriate term) is only known by combination; the previous attraction has never yet been shown to be of two kinds; and it seems more in accordance with Nature to diminish than to increase the number of original powers.

February 5, 1863.

Major-General SABINE, President, in the Chair.

The Earl of Caithness was admitted into the Society.

The following communication was read:—

"On the Embryogeny of *Comatula rosacea* (Linck)." By Professor WYVILLE THOMSON, LL.D., F.R.S.E., M.R.I.A., F.G.S. &c. Communicated by Professor HUXLEY. Received December 29, 1862.

(Abstract.)

After briefly abstracting Dr. W. Busch's description of the early stages in the growth of the young of *Comatula*, the author details his own observations, carried on during the last four years, on the development and subsequent changes of the larva. After complete segmentation of the yelk, a more consistent nucleus appears within the mulberry mass still contained within the vitelline membrane. The external more transparent flocculent portion of the yelk liquefies and is absorbed into this nucleus, which gradually assumes the form of the embryo larva, a granular cylinder contracted at either end and girded with four transverse bands of cilia. This cylinder increases in size till it nearly fills the vitelline sac, gradually increasing in transparency, and ultimately consisting of delicately vacuolated sarcode, the external surface transparent and studded with pyriform oil-cells, the inner portion semifluid and slightly granular.

The vitelline membrane now gives way, and, usually shortly after the escape of the larva into the water, the third ciliated band from the anterior extremity arches forwards at one point; and in the space thus left between it and the fourth band, a large pyriform depression indi-

cates the position of the larval mouth. At the same time a small round aperture, merely separated from the posterior margin of the mouth by the last ciliated band, becomes connected with the mouth by a short loop-like canal passing under the band, and fulfils the function of an excreting-orifice. A tuft of long cilia, which have a peculiar undulatory motion, is developed at the posterior extremity of the body. The larva now increases rapidly in size, assuming somewhat the form of a kidney bean, the mouth answering in position to the *hilum*. It swims freely in the water, with a swinging semirotatory motion, by means of its ciliated bands and posterior tuft of cilia.

Shortly after the larva has attained its definite independent form, ten minute calcareous spicula make their appearance, imbedded within the external sarcode-layer of the expanded anterior portion of the larva. The ten spicula are arranged in two transverse rings of five, the spicula of the anterior row symmetrically superposed on those of the posterior. By the extension of calcareous network, these spicula rapidly expand into ten plates, which at length form a trellis enclosing a dodecahedral space, open above and below, within the anterior portion of the zooid. Simultaneously with the appearance of these plates, a series of from seven to ten calcareous rings form a chain passing from the base of the posterior row of plates backwards, curving slightly to the left of the larval mouth, and ending by abutting against the centre of a large cribriform plate, which is rapidly developed close to the posterior extremity of the larva. Delicate sheaves of anastomosing calcareous trabeculæ shortly arise within these rings, and the series declares itself as the jointed stem of the pentacrinoid stage, the basal and first interradial plates of the calyx being represented by the already formed casket of calcareous network. The skeleton of the Crinoid is thus completely mapped out within the body of the larva, while the latter still retains its independent form and special organs.

Within the plates of the calyx of the nascent Crinoid two hemispherical or reniform masses may now be detected,—one superior, of a yellowish, subsequently of a chocolate colour; the other inferior, colourless and transparent. The lower hemisphere indicates the permanent alimentary canal of the Crinoid, with its glandular follicle; the upper mass originates the central ring of the ambulacral system, with its cæca passing to the arms. The body of the Crinoid is, how-

ever, at this stage entirely closed in by a dome of sarcode, forming the anterior extremity of the larva. After swimming about freely for a time, averaging from eight hours to a week, and increasing rapidly in size till it has attained a lengh of from 1 to 2 millims., the larva becomes sluggish, and its form is distorted by the growing Crinoid. The mouth and alimentary canal of the larva disappear, and the external sarcode-layer subsides round the calcareous framework of the included embryo, forming for it a transparent perisom. The stem now lengthens by additions of trabeculæ to the ends of the joints. The posterior extremity dilates into a disk of attachment. The anterior extremity becomes expanded, then slightly cupped; the lip of the cup is divided into five crescentic lobes corresponding to the plates of the upper ring; and finally five delicate tubes, cæca from the ambulacral circular canal, are protruded from the centre of the cup, the rudiments of the arms of the Pentacrinoid. At some stage during the progress of these later changes the embryo adheres, and at length becomes firmly cemented to some permanent point of attachment.

The author states his views as to the morphological and physiological relations of the larval zooid. He believes that all the peculiar independently organized zooids developed from the whole or from a part of the segmented yelk in the Echinoderms, and which form no stage in the development of the perfect form of the species, must be regarded as assimilative extensions of sarcode, analogous in function to the embryonic absorbent appendages in the higher animals. For such an organism the term "pseudembryo" is proposed. In the Echinoderm subkingdom, although constructed apparently upon a common plan, these pseudembryos present considerable range of organization, from a somewhat complex zooid provided with elaborate natatory fringes, with a system of vessels which are ultimately connected with the ambulacral vascular system of the embryo, with a well-developed digestive tract, and in some instances with special nervous ganglia, to a simple layer of absorbent and irritable sarcode which invests the nascent embryo. The pseudembryo of *Comatula* holds an intermediate position. It resembles very closely in external form and in subsequent metamorphosis the "pupa stage" of the Holothuridæ, the great distinction between them being that in the Holothuridæ the pupa has already passed through the more active "Auricularian" stage, while the analogous form in *Comatula* has been developed directly from the egg.

February 12, 1863.

Major-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting of the Society, His Royal Highness the Prince of Wales was proposed by the President for election and immediate Ballot. The Ballot having been taken, His Royal Highness was declared duly elected a Fellow of the Royal Society.

The following communications were read:—

I. "On some Compounds and Derivatives of Glyoxylic Acid." By HENRY DEBUS, Ph.D., F.R.S. Received December 31, 1862.

(Abstract.)

Glyoxylic acid contains one atom of oxygen less than oxalic acid, and may be considered as glycolic acid minus two atoms of hydrogen. It therefore bears to these two acids the same relation that oil of bitter almonds does to benzoic acid and benzylic alcohol. On another occasion* it has been shown to possess other properties in common with hydride of benzoyl. Dilute nitric acid, for instance, oxidizes glyoxylic acid to oxalic acid, and hydrate of potash converts it into glycolic acid and oxalic acid. The same reagents produce with oil of bitter almonds benzoic acid, benzoate of potash, and benzylic alcohol.

$$\underbrace{C_2H_2O_3}_{\text{Glyoxylic acid.}} + HNO_3 = \underbrace{C_2H_2O_4}_{\text{Oxalic acid.}} + HNO_2,$$

$$\underbrace{C_7H_6O}_{\text{Hydride of benzoyl.}} + HNO_3 = \underbrace{C_7H_6O_2}_{\text{Benzoic acid.}} + HNO_2,$$

$$2C_2H_2O_3 + 3KHO = \underbrace{C_2H_3KO_3}_{\text{Glycolate of potash.}} + \underbrace{C_2K_2O_4}_{\text{Oxalate of potash.}} + 2H_2O,$$

$$\underbrace{2(C_7H_6O)}_{\text{Hydride of benzoyl.}} + KHO = \underbrace{C_7H_5KO_2}_{\text{Benzoate of potash.}} + \underbrace{C_7H_8O.}_{\text{Benzylic alcohol.}}$$

In the memoir of which this is an abstract certain properties of

* Quarterly Journal of the Chemical Society, xii. 234.

glyoxylic acid are described which still more intimately connect this body with the class of the aldehydes, so as to leave no doubt as to its position in the system of organic substances.

Glyoxylates and sulphites have a strong tendency to combine. Glyoxylic acid expels one-half only of the sulphurous acid from a given quantity of sulphite of soda, and forms the substance represented by the formula $NaHSO_3 + C_2HNaO_3$; an excess of sulphurous acid, on the other hand, expels one-half of the acid in glyoxylate of lime, producing $CaHSO_3 + C_2HCaO_3$. These salts crystallize well and are very stable.

Sulphuretted hydrogen and glyoxylate of lime exchange sulphur and oxygen, water and a new compound,

$$C_4H_2Ca\left.\begin{matrix}O_5\\S\end{matrix}\right\} + 3H_2O,$$

being the result. The sulphur acid in the latter salt seems to bear to glyoxylic acid a relation similar to that in which triacetic acid stands to acetic acid. Glyoxylic acid itself is decomposed by sulphuretted hydrogen, a new crystallizable acid being produced.

Ammonia forms definite compounds with glyoxylates. The following of them were investigated:—two bodies formed from NH_3 and glyoxylate of lime,

$$3(C_2HCaO_3), 2NH_3, \text{ and } 3(C_2HCaO_3), 2NH_3 + H_2O,$$

one silver and one lead compound. These derivatives are very unstable, but the products of their decomposition could not be obtained in a pure state.

Hydrogen *in statu nascendi* combines with glyoxylic acid and converts it into glycolic acid,

$$C_2H_2O_3 + H_2 = \underbrace{C_2H_4O_3}_{\text{Glycolic acid.}}$$

This transformation was brought about by dissolving zinc in dilute glyoxylic acid.

Glycolic acid, glyoxylic acid, and oxalic acid therefore possess, as regards composition and some other essential properties, the same connexion as ethylic alcohol, acetic aldehyde, and acetic acid. The differences between the two series arise from the greater number of oxygen-atoms in the molecules of the first three bodies.

II. "On the Telescopic Appearance of the Planet Mars." By JOHN PHILLIPS, M.A., LL.D., F.R.S, F.G.S., Professor of Geology in the University of Oxford. Received February 5, 1863.

Notwithstanding the descriptions and drawings of Mars, for which we are indebted to eminent observers*, there remains much uncertainty as to the permanent boundaries of the bright and shady parts of the planet, to which respectively, on a first view, we attach, perhaps too readily, the idea of land and seas. The extremely variable aspects under which this planet appears in its excentric orbit, the axis being inclined more than 30° to the ecliptic, the different regions very unequally presented to incident light, and very unequally influenced by vicissitudes of heat and cold, may account for much of the uncertainty. Other difficulties arise when the work of different instruments is compared; for it is established that reflectors will on the whole give the best results for colour, while achromatics of fine quality discover more of detail than instruments of less perfect definition.

The author having devoted some evenings between the 27th of September and 13th of December 1862 to the examination of Mars with a 6-inch achromatic by Cooke, equatorially mounted, and moved by clockwork, at Oxford, presented to the Society some results of these observations combined with others, also made with achromatics, by Mr. Grove, Mr. Main, and Mr. Lockyer.

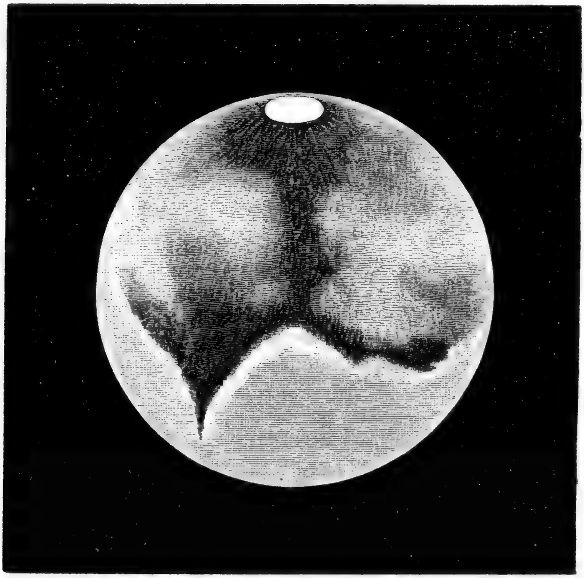
These various observations, made entirely without concert, were rendered comparable by a calculated reduction of each to the longitude on Mars corresponding to the epoch of each, according to one standard. [Tables of these reductions were given in the paper.] The sketches were then arranged on sheets in the order of the computed longitudes; and, in addition, two globes were exhibited, on one of which the main results of the author's observations were drawn, the data for the other being supplied by Mr. Lockyer's sketches.

He was also aided in the explanations by large drawings made with reflectors by Mr. De la Rue and Mr. Nasmyth.

* Herschel, Mädler, Jacob, De la Rue and Secchi have all published careful drawings of Mars.

From the author's sketches, three, representing opposite hemispheres, and one intermediate quadrature, have been selected for engraving,—one central to the assumed meridian of 0° or 360°, the others to the meridians of 90° and 180° nearly. See figs. 1, 2, 3.

Fig. 1.—Mars as seen on the 27th of September and on several other occasions till the 13th of December. (Longitude 0°.)



On considering the surface of the planet, either as seen in the telescope, or delineated on paper, we feel in some doubt as to the meaning of what we see. Are the bright parts (often seen of a red tint) land, the darker parts (often appearing of a greenish grey) water? or, as in the moon, are the reflecting powers of different parts of a dry surface very unequal? Is there any considerable change in the aspect of the masses or boundaries between one epoch and another, so as to indicate atmospheric vicissitudes like those on Jupiter and our own planet?

Taking the latter question first, the author found, on the experience of his observations during 74 days, that no material change took place in the main and prominent features about the longitude which he marks 0°. Not that after this considerable interval the appearances remained exactly as at first: that was not, and could not be

Fig. 2.—The appearance of Mars at longitude 90°, with long oblique ridges south of the great boundary, and nearly or quite running into the northern land, here less broad than in fig. 3: seen November 11th.

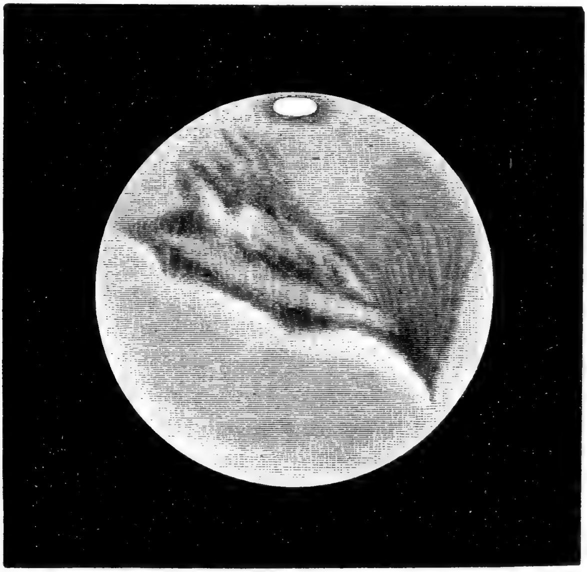
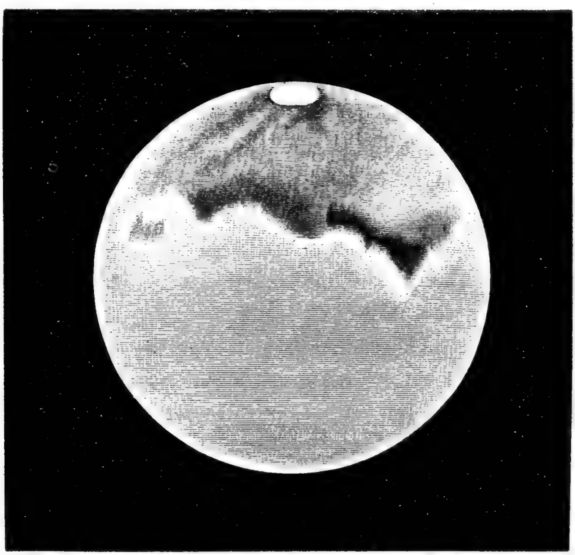


Fig. 3.—The hemisphere of Mars, opposite to fig. 1: seen October 15th and 16th. With a specially dark band



expected to be the case, after the planet had increased his distance from the earth to nearly double that when the observations began. Adding to his own the experience of Mr. Lockyer, whose observations began 35 days earlier, this inference, of permanence in the main boundaries of lights and shades, is extended to above 100 revolutions of Mars; and on comparison of these with the earlier sketches of Mädler, Herschel, Jacobs, and De la Rue, the conclusion appears to embrace the whole series of more than thirty years.

The author regards as one of the main features very firmly defined in the late opposition, the broad white or rather reddish band which from about 65° of north latitude (the north pole being invisible in these observations) spreads up into large bright cloud-like prominences toward and beyond the equator, and retires into one principal and several smaller bays toward the pole. From this bright space, which in many parts is sharply defined, a broad dusky tint spreads toward the south, partially relieved by half-lighted expansions with shades of various depths between. The south pole itself is surrounded (excentrically as it appears) by a bright white mass, obviously glittering in the telescope. This is believed to be snow; and the effect of its whiteness is increased in most parts of its circumference by the contrast of a dark ring round it, which expands here and there into broader spaces. Thus a great part of the northern area appeared in the late opposition bright, and often reddish, as if it were land, while a great part of the southern area was of the grey hue which is considered to indicate water, but relieved by various tracts of a tint more or less approaching to that of the brighter spaces of the northern hemisphere. The principal boundary of light and shade, for the most part very well defined, ran obliquely across the equator of Mars, so as to reach latitudes from 20° to 30° north and south of that line. This may perhaps be understood by the drawings selected for illustration, especially if compared with an orthographic projection of the latitudes *. (Still better by means of the globes which accompanied the communication, constructed by the author, one from his own sketches, the other from those of Mr. Lockyer.)

* The inclination of the axis of Mars to the observer was, on the 1st of October, 1862, 25½°, as Mr. Main has informed me.

Allowing the white spaces to be land, which reflects light as the moon in opposition, it seems a natural supposition that the shady spaces should be called sea; and this may be supported by the obvious requirement of water somewhere on Mars, to agree with the alternate gathering and melting of the snow round the poles. Still, every observer remarks no small resemblance of some of these shady tracts with particular parts of the unequally tinted grey surfaces of the moon. A positive proof of ocean on the disk of Mars would be afforded by the star-like image of the sun reflected from the quiet surface*, or the more diffused light thrown back from the waves; but nothing of this sort has been placed on record, nor is there such a variation in the appearance of these spaces from the centre toward the edges as to give any special reason for thinking them occupied by water.

Atmospheric vicissitudes, however, appear to be recognized in the somewhat variable aspect of many portions of the grey spaces; for these, though not much changed in the situation of the masses of light or shade, are sufficiently inconstant in their shapes and details to suggest the idea of a vaporous envelope, brooding over and about some parts more than others, and variable from one epoch to another. The drawings of Mr. Lockyer supply the best evidence of these variations; for Professor Phillips, except on a few occasions, confined his attention chiefly to the stronger and apparently more settled boundaries of light and shade.

The tints on the body of Mars were observed by each of the gentlemen named, but with different results. To Mr. Nasmyth, with a large reflector, the 'land' appeared of a decidedly red tint, the 'water' green. The 'land' appeared red in some parts, but

* The quiet image here alluded to would not exceed $\frac{1}{20}$th of a second of angle at the opposition, if no allowance be made for irradiation. But much allowance must be made for this. A thermometer-bulb, with diameter half an inch, reflects the sun as a star visible by the eye at 25 yards' distance, the reflecting surface in this case being about $\frac{1}{240}$th of an inch in diameter, and therefore (if no irradiation were allowed for) the angle subtended at the eye would be only about 1″. By employing on Mars a power of 300, the $\frac{1}{20}''$ becomes relatively magnified to 15″.

The reflective power of water at a nearly vertical incidence is, however, so much reduced, that Professor Phillips found it possible, under that condition, to observe the sun's image in water without a protecting dark glass. It seems probable, therefore, that even in very large reflectors the direct solar reflexion from water on Mars would be too faint for observation.—Feb. 17, 1863.

bright and almost silvery in other parts, to Professor Phillips, looking through his achromatic, which also showed the 'water' of a grey or greenish tint. No redness appeared in Mr. Lockyer's instrument, which, like many others of excellent quality for astronomical research, is intentionally 'over-corrected.'

Mr. Nasmyth saw the snow-patch on the south pole so distinctly bordered, as to give him the impression of its having a cliff-boundary. The south snow-patch did not appear to him to agree with the south pole of the planet, but, on the contrary, to be considerably excentric to it; and he supposed this to be due to the relative distribution of land and water, influencing the position of the centre of greatest cold. Only a faint glimmering of the snowy surfaces round the north pole was seen by any observer.

On the whole, the author of this paper concluded that, over a permanent basis of bright and dusky tracts on the surface of Mars, a variable envelope gathers and fluctuates, partially modifying the aspect of the fundamental features, and even in some cases disguising them under new lights and shades, which present no constancy,—a thin vaporous atmosphere probably resting on a surface of land, snow, and water.

Addendum.

Since the reading of the paper the author has been enabled, by the kindness of the Earl of Rosse, to examine a series of sketches of Mars during the late opposition, from the great telescopes at Birr. These drawings, six in number, were made on July 22, Sept. 14, Sept. 16, Oct. 6, Oct. 29, and Nov. 6. They confirm in a remarkable manner the conclusions already presented by the author, and suggest some interesting questions for further observation and study. On the 22nd of July the southern snow was a large patch, meeting the limb by its diametral line. It must then have had a radius of 500 miles at least: in the later observations it was reduced to less than half this measure.

One of the drawings nearly corresponds to longitude 180° on the author's scale, and represents the specially dark short band which distinguishes that aspect of the planet (fig. 3). Two correspond nearly to fig. 1, and contain the remarkable deep angular bay which extends so far towards the north pole. In these and the re-

maining three drawings, general resemblances and special differences appear on comparison with the sketches of Prof. Phillips and Mr. Lockyer. The differences affect principally the grey southern parts, and are remarkable enough to justify serious doubts whether any of our drawings of those parts are much to be trusted as representing permanent physical boundaries. Nor should this be thought surprising; owing to the high inclination of the axis of Mars to the plane of his orbit, the regions round each pole are presented alternately to the sun through periods somewhat less than our whole year. The effect is seen in the vast outspread of snows round the cold pole, and the contraction of those white sheets to a small glittering ellipse round the warm pole. The enormous transfer of moisture from one hemisphere to the other while the snows are melting round one pole and growing round the other must generate over a great part of the planet heavy storms and great breadths of fluctuating clouds, which would not, as on the quickly rotating mass of Jupiter, gather into equatorial bands, but be more under the influence of prominent land and irregular tracts of ocean.

February 19, 1863.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

Pursuant to notice given at the last Meeting of the Society, The Right Honourable Edward Pleydell Bouverie, and His Grace the Archbishop of York, were proposed for election and immediate Ballot.

The Ballot having been taken, Mr. Bouverie and the Archbishop of York were declared duly elected Fellows of the Society.

The following communication was read:—

"On Thallium." By WILLIAM CROOKES, Esq. Communicated by Professor STOKES, Sec. R.S. Received February 5, 1863.

(Abstract.)

After discussing the occurrence and distribution of the new metal in different parts of the globe, the author proceeds to describe the method adopted by him for extracting it from its ore. Thalliferous pyrites is distilled at a bright red heat, in quantities of about 1 cwt. at a time, in cast-iron retorts. The resulting sulphur, varying from 13 to 17 per cent. of the pyrites taken, is then dissolved in aqueous caustic soda, which leaves the sulphide of thallium as an insoluble black precipitate; this is filtered off, dissolved in acids, and the thallium precipitated in the form of iodide. This is then converted into sulphate, and the metal reduced from the solution by electrolysis. It is obtained in the coherent form by fusion under cyanide of potassium.

The physical characteristics of thallium are then described. In appearance it most resembles tin and cadmium, but has a distinct colour of its own; it has a brilliant metallic lustre, and is susceptible of taking a very high polish; it oxidizes in the air with almost the rapidity of an alkaline metal, but when coated with oxide, the metal may be freely handled and exposed to the air with scarcely any further change. An oxidized surface applied to the tongue is very biting and caustic, and has a sweetish metallic taste. It is the softest known metal admitting of free exposure to the atmosphere, being scratched by soft lead with the greatest ease. It makes a dark blue mark upon paper, rapidly turning yellow, which in the course of a few hours nearly fades out, but can be restored with sulphide of ammonium. It has little tenacity, is very malleable, and may be readily pressed into wire.

The specific gravity of thallium varies from 11·81 to 11·91, and it is probably capable of still greater condensation.

When freshly prepared, thallium wire is perfectly amorphous, but when kept in water it gradually assumes a superficial crystalline appearance: this effect is immediately produced when thallium in wire, ingot, or plate, tarnished or clean, is boiled in water.

Its melting-point is 550° F., being between bismuth and lead,

and the metal does not become pasty before undergoing complete fusion. Two pieces of clean metal weld together by pressure in the cold. It begins to volatilize at a red heat, and boils below a white heat; it may be distilled in a current of hydrogen.

It is a pretty good conductor of heat and electricity, and stands electro-chemically very near cadmium. It is strongly diamagnetic, ranking in this respect near bismuth. The alloys which thallium forms with different metals are next described.

Further details are given respecting the spectrum of thallium: the characteristic green line is perfectly single under a very high magnifying power and after refraction through nine heavy glass prisms; and no new lines make their appearance at the temperature of the oxyhydrogen blowpipe,—although, with the electric spark, Dr. Miller has shown that several new lines come into existence.

The delicacy of the optical test for thallium is roughly estimated, the $\frac{1}{5,000,000}$th of a grain being easily perceptible.

The atomic weight of thallium is given as 203, being the mean of five experiments. The author states, however, that this is not to be regarded as a final result.

The chemical properties of thallium are next described. It does not decompose water even at the boiling-point, but remains bright under this liquid. The superficial tarnish is a powerful base soluble in water, and reacting like an alkaline solution. Melted in the air, thallium forms a readily fusible oxide, its behaviour resembling that of lead.

The formation of thallic acid and the properties of some of the thallates are described. Sulphate, nitrate, the chlorides, sulphide, iodide, and other salts of thallium are described in detail. The metal may be quantitatively determined by precipitation, either as protochloride, iodide, or platinochloride.

The position of thallium amongst elementary bodies is then discussed. Although one or two of its properties show a resemblance to the alkaline metals, the author does not agree with continental chemists in classing it with this group,—numerous facts proving that its true position is by the side of mercury, lead, or silver. The ready dehydration of its basic oxide; the insolubility of its sulphide, iodide, chloride, bromide, chromate, phosphate, sulphocyanide, and ferrocyanide; its great atomic weight; its ready reduction by zinc

to the metallic state; its power of forming a strongly acid oxide; and, according to Dr. Miller, the complexity of its photographic spectrum,—all prove that thallium cannot consistently be classed anywhere but amongst the heavy metals, mercury, silver, lead, &c. No weight is attached to M. Dumas's argument in favour of thallium being related to potassium and sodium because its equivalent is rather near a figure obtained by adding twice the atomic weight of one metal to four times the atomic weight of the other. The author shows that, by similar processes of addition, multiplication, or subtraction, it is not difficult to prove that thallium is related to any desired group of elements.

The author gives full analytical notes on thallium, showing where it would occur in the ordinary course of analysis, and detailing accurate methods of separating it from every metal with which it can be accompanied.

February 26, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Effect of Temperature on the Secretion of Urea, as observed on a Voyage to China, and at Hong Kong." By EMIL BECHER, M.D., Assistant-Surgeon, Army Medical Staff. Communicated by Dr. EDMUND A. PARKES. Received January 20, 1863.

(Abstract.)

With a view to extend our knowledge of the physiological effects of temperature, with especial regard to the influence of tropical heat on the healthy system, Dr. Becher, with the liberal assistance of the Director-General, Army Medical Department, took advantage of a voyage to China (round the Cape of Good Hope) in 1857, and a short residence at Hong Kong, in order to *determine on himself the influence of the extreme variations of temperature* incidental to that voyage, on the quantity of urine, urea, and chloride of sodium excreted during each twenty-four hours.

During a period of 163 days (100 days at sea, 63 days at Hong Kong during the change of monsoon), Dr. Becher collected the daily quantity of urine, and determined the amount of urea and NaCl by the volumetric method (solutions of nitrate of mercury), and registered meteorological observations as accurately as circumstances would permit, observing all the time as constant a mode of living (with regard to food, exercise, &c.) as was practicable without undue restriction.

The whole of the observations, divided into the two periods indicated, are fully detailed in Tables, and graphically represented in Diagrams.

The results show a most remarkable relation between air-temperature and daily quantity of urea and NaCl, viz. a constant increase with the rising of temperature from 50°–70°, and an equally constant falling off with the further rise of temperature from 70°–90°.

The physiological limit of the tropical zone, as marked by the sudden decrease in the quantity of urinary water, is constantly fixed at 76°.

Appended is an extract from a manuscript of Dr. Forbes Watson, containing a series of observations on the daily quantity of urine and the amount of solids therein excreted by a number of healthy soldiers in various temperatures during a voyage to India in 1850.

These observations were made by Dr. Forbes Watson, who most kindly consented to their being added here, as far as they serve to illustrate the influence of temperature under otherwise constant conditions. They are tabulated and graphically represented as much as possible on the same plan as those of Dr. Becher, and in their results show the most satisfactory harmony with the latter.

II. "On Clinant Geometry, as a means of expressing the General Relations of Points in a Plane, realizing Imaginaries, reconciling Ordinary Algebra with Plane Geometry, and extending the Theories of Anharmonic Ratios." By ALEXANDER J. ELLIS, B.A., F.C.P.S. Communicated by ARTHUR CAYLEY, Esq. Received January 28, 1863.

(Abstract.)

The serious difficulties presented by "imaginaries" in plane geometry arise from treating the "principle of signs" as a matter of convention, and not as a particular case of a general operation, here termed a *clinant*, which consists in altering the length of a line in a given ratio, and rotating it through a given angle. As the calculus of clinants furnishes a geometrical representation for every algebraical result, imaginaries disappear, and there is no longer any apparent disagreement between analysis and geometry. Many theories, as, for example, those of anharmonic ratios, hitherto only established for points on a straight line, are also easily extended by means of clinants to embrace any points upon a plane. The object of the present paper is to establish and illustrate these facts. For this purpose it is divided into three distinct but closely connected parts.

Part I. shows that clinants obey the same laws of calculation as ordinary algebraical expressions, and explains their notation and geometrical construction. This is illustrated by the solution of the problem of the determinate section generalized, and by a geometrical explanation of "imaginary" trigonometrical functions, applied to the discovery of the "imaginary" double rays in an homography.

Part II. establishes the theory of *stigmatics*. An *index* point, supposed to move from any *origin* into every point on a plane, is accompanied by one or more satellite points, termed *stigmata*, the relative position of the stigmata and index at any time being dependent on the relative position of the index and origin, according to some assigned law. The locus of the stigmata, corresponding to each *path* of the index, forms a *stigmatic curve*. The aggregate of these curves constitutes a *stigmatic*, which therefore consists of points conjugated with each other according to a *characteristic* law, ulti-

mately expressible by an equation between the clinant of the line connecting the index with the origin and the clinant of the line connecting the stigma and the index. By elimination between two such equations, the common stigmata (*systigmata*) of two stigmatics, and by the condition of equal roots their coalescent systigmata, or *homostigmata,* may be determined. These systigmata and homostigmata include, as particular cases, the points of "real" and "imaginary" intersection and contact of algebraical curves.

These generalities are illustrated by a consideration of the general stigmatic straight line and the central stigmatic circle. The stigmatic straight line consists of stigmatic curves similar to the paths of the index, and their systigmata are the "double points" of similar figures. The stigmata of a stigmatic circle are always harmonically conjugated with the extremities of its axis (with which they always lie either on the same straight line, or the circumference of the same circle), and hence form an "involution" of points on a plane. The construction of the systigmata and homostigmata of a stigmatic straight line, and stigmatic circle, furnishes a complete geometrical explanation and realization of the "imaginary intersections" of straight lines, with "real" and "imaginary" circles, "imaginary tangents" to such circles, and their polars and radical axes and common chords.

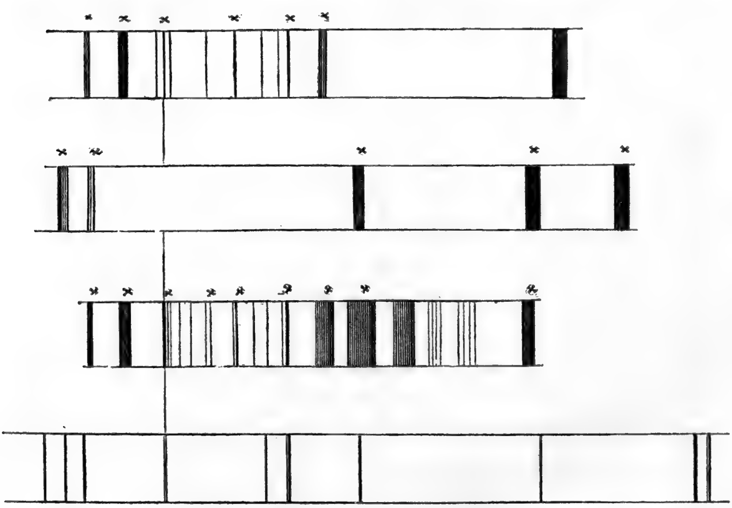
Part III. contains an extension of the theories of anharmonic ratios from points on straight lines to any points in a plane, and explains and constructs the homography and involution of such systems of points, with their double points, &c. Constant reference is made throughout this part to M. Chasles's 'Géométrie Supérieure,' to show how his fundamental theories may be interpreted as conclusions in clinant geometry, to explain all cases of "imaginaries," and to establish the fact that "real" and "imaginary" points are only two very particular cases of the general theory of conjugated points.

The whole memoir forms an introduction to a new and practical geometrical calculus, including and interpreting all analytical investigations on plane geometry.

III. "Note on the Lines in the Spectra of some of the Fixed Stars." By William Huggins, Esq., F.R.A.S., and William Allen Miller, M.D., LL.D., Treasurer and V.P.R.S. Received February 19, 1863.

The recent detailed examination of the solar spectrum, and the remarkable observations of Kirchhoff upon the connexion of the dark lines of Fraunhofer with the bright lines of artificial flames, having imparted new interest to the investigation of spectra, it has appeared to the authors of the present note that the Royal Society may not consider a brief account of their recent inquiry upon the spectra of some of the self-luminous bodies of the heavens unworthy of attention, although the investigation is as yet far from complete.

After devoting considerable time to the construction of apparatus suitable to this delicate branch of inquiry, they have at length succeeded in contriving an arrangement which has enabled them to view the lines in the stellar spectra in much greater detail than has been figured or described by any previous observer. The



apparatus also permits of the immediate comparison of the stellar spectra with those of terrestrial flames. The accompanying drawing

shows with considerable accuracy the principal lines which the authors have seen in Sirius, Betelgeux, and Aldebaran, and their position relatively to the chief solar lines.

Without at present describing in detail, as they propose to do when the experiments are completed, the arrangements of the special apparatus employed, it may be sufficient to state that it is attached to an achromatic telescope of 10 feet focal length, mounted in the observatory of Mr. Huggins at Upper Tulse Hill. The object-glass, which has an aperture of 8 inches, is a very fine one by Alvan Clark of Cambridge, U.S.; the equatorial mounting is by Cooke of York, and the telescope is carried very smoothly by a clock motion.

It may further be stated that the position in the stellar spectra corresponding to that of Fraunhofer's line D, from which the others are measured, has been obtained by coincidence with a sodium line, the position of which in the apparatus was compared directly with the line D in the solar spectrum.

The lines in the drawings against which a mark is placed have been measured.

Addendum.—Since the foregoing Note was presented to the Royal Society, the authors have learned that a paper on the same subject, accompanied by diagrams of the spectra of the Moon, Jupiter, Mars, and several of the fixed stars, by Mr. L. M. Rutherfurd, has appeared in the January Number of the 'American Journal of Science' for the current year. The method of observing finally employed by Mr. Rutherfurd much resembles that adopted by the authors of this Note.

They therefore desire to add that, during the past twelvemonth, they have examined the spectra of the Moon, Jupiter, and Mars, as well as of between thirty and forty stars, including those of Arcturus, Castor, α Lyræ, Capella, and Procyon, some of the principal lines of which they have measured approximatively. They have also observed β and γ Andromedæ, α, β, ϵ and η Pegasi, Rigel, η Orionis, β Aurigæ, Pollux, γ Geminorum, α, γ and ϵ Cygni, α Trianguli, ϵ, ζ and η Ursæ Majoris, α, β, γ, ϵ and η Cassiopeiæ, and some others.—[Feb. 21, 1863.]

March 5, 1863.

Major-General SABINE, President, in the Chair.

The Right Hon. Edward Pleydell Bouverie was admitted into the Society.

In accordance with the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Henry Foster Baxter, Esq.
William Bovill, Esq., Q.C.
Sir Charles Tilstone Bright.
William Brinton, M.D.
John Charles Bucknill, M.D.
Capt. Richard Burton.
Lieut.-Col. John Cameron, R.E.
Thomas Spencer Cobbold, M.D.
Edward William Cooke, Esq., A.R.A.
William Crookes, Esq.
Henry Dircks, Esq.
Alexander John Ellis, B.A.
Henry Fawcett, Esq.
James Fergusson, Esq.
Frederick Field, Esq.
Rev. Robert Harley.
John Russell Hind, Esq.
William Charles Hood, M.D.
William Jenner, M.D.
Edmund C. Johnson, Esq.
Henry Letheby, M.B.
Sir Charles Locock, M.D.
Robert McDonnell, M.D.
Charles Watkins Merrifield, Esq.
Capt. Andrew Noble, R.A.
Professor Daniel Oliver.
George Wareing Ormerod, M.A.
Frederick William Pavy, M.D.
William Pengelly, Esq.
John George Perry, Esq.
Thomas Lambe Phipson, Ph.D.
Charles Bland Radcliffe, M.D.
Thomas Richardson, M.A.
Henry E. Roscoe, Ph.D.
William Henry Leighton Russell, B.A.
Rev. George Salmon, D.D.
Samuel James Augustus Salter, M.B.
Rev. Arthur Penrhyn Stanley, D.D.
Lieut.-Col. Alexander Strange, M.C.
Thomas Tate, Esq.
Charles Tomlinson, Esq.
Charles Wye Williams, Esq.
Lieut.-Col. Frederick Marow Eardley Wilmot, R.A.
Nicholas Wood, Esq.
Henry Worms, Esq.

The following communications were read:—

I. "On Skew Surfaces, otherwise Scrolls." By A. Cayley, F.R.S. Received February 3, 1863.

(Abstract.)

It may be convenient to mention at the outset that in the paper "On the Theory of Skew Surfaces," Camb. and Dubl. Math. Journ. vol. vi. pp. 171–173 (1852), I pointed out that upon any skew surface of the order n there is a singular (or nodal) curve meeting each generating line in $(n-2)$ points, and that the class of the circumscribed cone, or what is the same thing, the class of the surface, is equal to the order n of the surface. In the paper "On a Class of Ruled Surfaces," Camb. and Dubl. Math. Journ. vol. viii. pp. 45, 46 (1853), Dr. Salmon considered the surface generated by a line which meets three curves of the orders m, n, p respectively: such surface is there shown to be of the order $=2mnp$; and it is noticed that there are upon it a certain number of double right lines (nodal generators); to determine the number of these, it is necessary to consider the skew surface generated by a line meeting a given right line and a given curve of the order m twice; and the order of such surface is found to be $=\frac{1}{2}m(m-1)+h$, where h is the number of apparent double points of the curve. The theory is somewhat further developed in Dr. Salmon's memoir "On the Degree of a Surface reciprocal to a given one," Trans. R. Irish Acad. vol. xxiii. pp. 461–488 (read 1855), where certain minor limits are given for the orders of the nodal curves on the skew surface generated by a line meeting a given right line and two curves of the orders m and n respectively, and on that generated by a line meeting a given right line and a curve of the order m twice. And in the same memoir the author considers the skew surface generated by a line, the equations whereof are $(a, \ldots \between t, 1)^m=0$ $(a^1, \ldots \between t, 1)^n=0$, where $a, \ldots a^1, \ldots$ are any linear functions of the coordinates, and t is an arbitrary parameter. And the same theories are reproduced in the "Treatise on the Analytic Geometry of three Dimensions," Dubl. 1862. I will also, though it is less closely connected with the subject of the present memoir, refer to a paper by M. Chasles, "Description des Courbes à double Courbure de tous les ordres sur les surfaces reglées du troisième et du quatrième ordre," Comptes Rendus, t. liii. (1861, 2^{e} Sem.), pp. 884–889.

The present memoir (in the composition of which I have been assisted by a correspondence with Dr. Salmon) contains a further development of the theory of the skew surfaces generated by a line which meets a given curve or curves: viz. I consider,—1st, the surface generated by a line which meets each of three given curves of the orders m, n, p respectively; 2nd, the surface generated by a line which meets a given curve of the order m twice, and a given curve of the order n once; 3rd, the surface which meets a given curve of the order m three times; or, as it is very convenient to express it, I consider the skew surfaces, or say the "scrolls," $S(m, n, p)$, $S(m^2, n)$, $S(m^3)$. The chief results are embodied in the Table given after this introduction, at the commencement of the memoir. It is to be noticed that I attend throughout to the general theory, not considering otherwise than incidentally the effect of any singularity in the system of the given curves, or in the given curves separately: the memoir contains, however, some remarks as to what are the singularities material to a complete theory; and in particular as regards the surface $S(m^3)$. I am thus led to mention an entirely new kind of singularity of a curve in space—viz., such a curve has in general a determinate number of "lines through four points" (lines which meet the curve in four points); it may happen that of the lines through three points, which can be drawn through any point whatever of the curve, a certain number will unite together and form a line through four (or more) points, the number of the lines through four points (or through a greater number of points) so becoming infinite.

II. "Researches on the Refraction, Dispersion, and Sensitiveness of Liquids." By J. H. Gladstone, Ph.D., F.R.S., and the Rev. T. P. Dale, M.A., F.R.A.S. Received February 5, 1863.

(Abstract.)

This communication contains the results of some inquiries which were started by the authors in a previous paper "On the Influence of Temperature on the Refraction of Light"*. The same apparatus

* Phil. Trans. 1858, p. 887.

had been employed, but some modifications were introduced in the method of observation, which are described; and the amount of probable error from different sources was determined. The liquids experimented on were either prepared or purified in Dr. Gladstone's laboratory, or were specimens reputed to be pure, and lent for the purpose of this inquiry by Prof. Hofmann, Prof. Williamson, Prof. Frankland, Drs. Warren De la Rue and Hugo Müller, Mr. Buckton, Dr. Odling, Mr. A. H. Church, Mr. C. Greville Williams, and Mr. Piesse. The data are collected in two long tables forming two appendices: the first containing the refractive indices of the lines A, D, and H, of 78 specimens at two or three different temperatures; the second, the refractive indices of all the more important lines for 61 of these liquids, and 10 others at the temperature of the room when the observations were made.

Five points were investigated, and the following are the results arrived at with respect to each point.

I. *The relation between the change of refraction (sensitiveness) and the change of volume by heat.*—The uniform testimony of about 90 different liquids examined was that both refraction and dispersion diminish as the temperature increases.

The following Table will suffice as an example, showing as it does that the different rays are more sensitive in the order of their refrangibility:—

Liquid.	Temp.	Refractive Indices.						
		A.	B.	D.	E.	F.	G.	H.
Bisulphide of Carbon......	11° C.	1·6142	1·6207	1·6333	1·6465	1·6584	1·6836	1·7090
	36°·5	1·5945	1·6004	1·6120	1·6248	1·6362	1·6600	1·6827
Difference	....	0·0197	0·0203	0·0213	0·0217	0·0222	0·0236	0·0263

This change of refraction by heat was compared with the known or ascertained change of volume in bisulphide of carbon, water, methylic, ethylic, and amylic alcohols, ether, acetone, acetic acid, formic, acetic, and butyric ethers, methylic and ethylic iodides, salicylate of methyl, bromoform, benzole, xylole, cumole, nitrobenzole, hydrate of phenyl, the rectified oils of turpentine and Portugal and eugenic acid, and in every case it was found that the refractive index minus unity, multiplied by the volume, gave very nearly a

constant at different temperatures. Now every refractive index contains at least two coefficients: the one of refraction, which is represented by the theoretical limit of the spectrum; the other of dispersion, for which the difference between the refractive indices of H and A may be taken as the exponent. The refractive index, minus unity ($\mu-1$), is termed by the authors the "refractive energy" of the substance, and this multiplied by the volume ($\mu-1$), or divided by the density, is termed the "specific refractive energy." It was not found as a rule that the theoretical limit of the spectrum gave more truly a constant than the line A; but the difference is within experimental errors. The empirical law was therefore expressed as follows:—The refractive energy of a liquid varies directly with its density under the influence of change of temperature, or, in other words, the specific refractive energy of a liquid is a constant not affected by temperature. Yet the influence of dispersion renders this not absolutely accurate in the observed numbers, for the change of dispersion does not follow the same law, the spectrum contracting in some cases much more, and in other cases much less rapidly than the volume increases; indeed no relation is as yet discoverable between the change of dispersion and that of density.

II. *The refraction and dispersion of mixtures of liquids.*—This question has engaged the attention of several experimenters, only one of whom, however, M. Hoek, has offered a solution. His formula depends on μ^2-1. Yet most of the results recorded were equally well explained on the supposition that the specific refractive energy of a mixture is the mean of the specific refractive energies of its components. It was clearly desirable to test this in some cases where the refractive indices of the liquids mixed were very wide apart. Fortunately, bisulphide of carbon and ether, substances almost at the opposite limits of the scale, were found to mix without condensation; and another good experiment was obtained with aniline and alcohol, on mixing which, however, some diminution of volume occurs. In both these cases the experimental numbers were slightly below those deduced from the mean of the specific refractive energies, the discrepancy being beyond the limits of probable error; yet no other formula could be devised which would give a nearer approximation to the indices actually observed.

III. *The refraction, dispersion, and sensitiveness of different*

members of homologous series.—Many such series were examined, and the results are tabulated, the refractive index of A and the length of the spectrum or dispersion being reduced, if necessary, to 20° C., and the sensitiveness being taken for the 10 degrees rising above 20° C.; the specific refractive energy, dispersion, and sensitiveness also form part of the Tables. Methylic, ethylic, amylic, and caprylic alcohols are the first series examined, and it is found that on ascending the series the refraction increases; the dispersion does so still more rapidly, while the sensitiveness remains nearly the same. Other homologous series of the same group, such as the iodides, compound ethers, or mercury compounds, were also examined, and they all agree in exhibiting a progressive change in refraction and dispersion with the advancing members of the series; but in which direction and to what extent depend on the other substances with which the compound radical is combined. Yet, if we regard not the actual indices, but these, minus unity, divided by the density, a pretty regular increase is found to take place as the series advance. The following Tables exhibit this:—

Specific Refractive Energy.

Radical.	Formula.	Alcohol.	Iodide.	Ether of Acid.	Formiate.	Acetate.	Butyrate.	Oxalate.	Mercury Compound.	Stannic Compound.	Hydride.
Methyl	$C_2\ H_3$	·4105	·2359	·3905	...	·389	...	...	·1707	·3727	
Ethyl	$C_4\ H_5$	·4482	·2614	·4127	·3905	·4127	·4402	·3502	·2112	·3876	
Propyl	$C_6\ H_7$	...	...	·4333							
Butyl	$C_8\ H_9$	...	...	·4402							
Amyl.........	$C_{10}\ H_{11}$	·4895	·3213	·4492	·4432	·4527	·4724	·4306			
Œnanthyl ...	$C_{14}\ H_{15}$	...	...	·4750	...	...	...	...	...	...	·5499
Capryl	$C_{16}\ H_{17}$	·5096	...	...	...	...	...	...	...	...	·5522
Laurostearyl	$C_{14}\ H_{25}$	...	...	·4890							

Specific Dispersion.

Radical.	Alcohol.	Iodide.	Ether of Acid.	Acetate.	Mercury Compd.	Stannic Compd.	Hydride.
Methyl	163	209	168	...	140	256	
Ethyl	190	218	174	174	170	268	
Propyl	...	...	191				
Butyl	...	...	191				
Amyl...............	212	224	198	198			
Œnanthyl.........	...	...	...	...	...	...	241
Capryl	237	...	...	...	...	...	237

Other groups of homologous bodies were also examined. Benzole, toluole, xylole, cumole, and cymole gave nearly the same numbers, and no regular progression. Pyridine, picoline, lutidine, and collidine showed an augmentation of the specific refractive energy, but a diminution of the specific dispersion with the advancing series. Chinoline and lepidine (which proved to be the most refractive organic liquid known) showed an increase of each of the optical properties by the addition of $C_2 H_2$. Thus the influence of the added increment on the rays of light differs in different groups, just as it does in respect to the boiling-point.

IV. *The refraction, dispersion, and sensitiveness of isomeric liquids.*—Several of the liquids, isomeric with the different members of the benzole series, were examined; some proved to be identical in all optical properties; others sensibly the same in actual refraction and dispersion, though slightly different in density; some again identical in density, but differing in optical properties; while other isomeric bodies differed slightly in each of these respects. Several hydrocarbons of the type $C_{20} H_{16}$, from essential oils, seemed to be identical in actual refraction, notwithstanding slight differences of their density. In dispersion, too, there were some variations; but not in sensitiveness. Other hydrocarbons, however, of the same ultimate composition, but differing considerably in physical properties, differed also optically. Compound ethers, as valerianic ether and acetate of amyl, which contain the same number of carbon, hydrogen, and oxygen elements, though differently arranged, are optically identical, as was partially shown by Delffs some years ago. Aniline and picoline, each empirically $C_{12} H_7 N$, are totally different. The conclusion arrived at is that isomeric bodies are sometimes widely different in these optical properties; but that in many cases, especially where there is close chemical relationship, there is identity also in this respect.

V. *The effect of chemical substitution.*—By observing the amount of change in the optical properties which results from a replacement of one element by another, the chemical type remaining the same, it seemed possible to arrive at a knowledge of the influence of the individual elements on the rays of light transmitted by them. Of the immense number of data required for the perfecting of such an inquiry, the following are afforded by the experiments already made.

The replacement of hydrogen by a compound radical, aniline—amyl-aniline; and water, alcohol, ether (according to Williamson's theory). Of hydrogen by oxygen—alcohol, acetic acid; ether, acetic ether; and carvene, carvole, eugenic acid. Of hydrogen by peroxide of nitrogen—benzole, nitrobenzole, dinitrobenzole (in solution); glycerine, nitroglycerine; and amylic alcohol, nitrate of amyl. Of hydrogen by chlorine—benzole, chlorobenzole, terchlorobenzole; and the substitution of chlorine by bromine—terchloride of phosphorus, terbromide of phosphorus; chloroform, bromoform; and bichloride of chlor-ethylene, bibromide of chlor-ethylene, bibromide of brom-ethylene. When hydrogen is replaced by some other body, there is generally an increase of the actual refraction and dispersion; but this is due to the increased weight, hydrogen having a very low actual, but a very high specific influence on the rays of light. In each of the five instances of two substitution-products, as, for instance, chlorobenzole and trichlorobenzole, the lower one always retains in its optical properties an intermediate position between the original substance and the higher product.

These experiments on substitution sufficed to show, as the examination of isomeric bodies had done, that the special influence exerted on the rays of light by the elements of a compound is greatly dependent on the manner of their combination.

The following is given as a generalization approximately, if not absolutely true:—Every liquid has a specific refractive energy composed of the specific refractive energies of its component elements, modified by the manner of combination, and which is unaffected by change of temperature, and this refractive energy accompanies it when mixed with other liquids.

III. "On the Change of Form assumed by Wrought Iron and other Metals when Heated and then Cooled by partial immersion in Water." By Lieut.-Col. H. Clerk, R.A., F.R.S. Received February 9, 1863.

Origin of the Experiments.—A short time ago, when about to shoe a wheel with a hoop-tire, to which it was necessary to give a bevel of about $\frac{3}{8}$ths of an inch, one of the workmen employed suggested that the bevel could be given by heating the tire red-hot and

then immersing it one-half its depth in cold water. This was tried, and found to answer perfectly, that portion of the tire which was out of the water being reduced in diameter. The tire was 3 inches wide, $\frac{1}{2}$ inch thick, and $4' \, 2''$ in diameter.

As this result was curious and not generally known, I considered it desirable to institute some further experiments in order to try how far, by successive heatings and coolings, this change of form could be augmented, and also whether the same effect could be produced on other metals than wrought iron.

Mode of carrying out the Experiments.—The experiments were made on cylinders of wrought iron of different dimensions, both hollow and solid; immersed, some to one-half of their depth, others to two-thirds; also on similar cylinders of cast iron, steel, zinc, tin, and gun-metal.

The specimens experimented on were all accurately turned in a lathe to the required dimensions, which were carefully noted; they were then heated to a red heat in a wood-furnace used for heating the tires of wheels. As soon as they had acquired the proper heat, they were taken out and immersed in water to one-half or two-thirds of their depth (as stated in the experiment). The temperature of the water ranged from 60° to 70° Fahr.

The specimens were allowed to remain in the water about two minutes, in which time the portion in the air had lost all redness, and that in the water had become sufficiently cool to handle. These alternate heatings and coolings were repeated till the metal showed signs of cracking or giving way.

The dimensions were noted after every five heatings. The circumferences were measured in preference to the diameters, as the true circular form was liable to alter.

General Results.—It will be seen by an inspection of the figures that the general effect is a maximum contraction of the metal about one inch above the water-line; and that this is the same whether the metal be immersed one-half or two-thirds of its depth, or whether it be nine, six, or three inches deep. With wrought iron the heatings and coolings could be repeated from fifteen to twenty times before the metal showed any signs of separation; but with cast iron after the fifth heating the metal was cracked, and the hollow cylinder separated all round just below the water-line after the second heating.

Cast steel stood twenty heatings, but was very much cracked all over its surface. As respects the change of form of cast iron and steel, the result was similar to that in wrought iron, but not nearly so large in amount. The cast iron did not return to its original dimensions, but the smallest diameter was about one inch above the water-line.

Tin showed no change of form, there being apparently no intermediate state between the melting-point and absolute solidity. Brass, gun-metal, and zinc showed the effect slightly; but instead of a contraction just above the water-line, there was an expansion or bulging.

The effect on wrought iron is best seen in the solid cylinder (figs. 9 and 10), where the displacement of particles just above the water-line appears to be compensated by the bulgings at the two extremities.

The specimens of wrought iron were submitted by Mr. Abel (Chemist to the War Department) to chemical analysis, and he informs me that he found nothing noteworthy in the composition of the metal; nor was there any appreciable difference in the specific gravity of the metal taken from different parts of the specimen. It appears therefore to be simply a movement of the particles whilst the metal is in a soft or semifluid state.

The following is an account of the experiments, which were carried out under the superintendence of Mr. Butter, Draughtsman of the Royal Carriage Department, to whom also I am indebted for the accompanying diagrams. The exact dimensions of each specimen before and after heating are given in a tabulated form at the end of the paper, to facilitate comparison.

In figs. 22 and 23 the changes in form of the 9″ cylinders (one immersed one-half, the other two-thirds its depth) are shown in section after every five heatings (half the full size).

Experiment. 1—A 4 ft. 2 in. hoop-tire of 3 inches breadth and $\frac{3}{8}$ths inch in thickness (fig. 1) was heated and cooled by being immersed to half its depth in cold water five times, by which the effect shown in fig. 2 was produced.

Fig. 1.

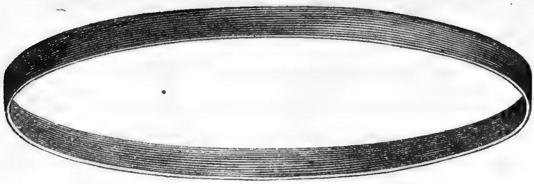
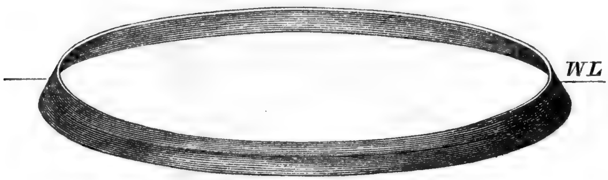


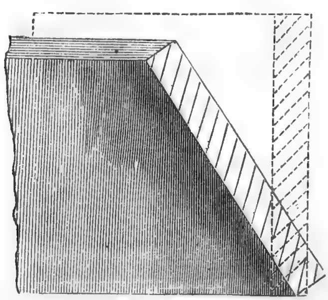
Fig. 2.



One-eighteenth of full size.

The upper edge, or that cooled in air, had contracted 8 inches, or $\frac{1}{20}$th its entire length, and slightly increased in thickness; while the lower edge, cooled in water, had expanded ·875 inch, making a difference between the two circumferences of 8·875 inches. The breadth remained unaltered (3 inches), and kept perfectly straight.

Fig. 3.



Section showing the amount of contraction. One-half the full size. The dotted lines show the original form.

The quality of the iron was afterwards tested by pieces taken from the upper and lower edges, and also from the centre; the fibrous

condition had remained unchanged, the specific gravity had not altered appreciably, and there appeared to be no deterioration in any part of it.

Experiment 2.—Two hollow cylinders of wrought iron, 12 inches diameter and $\frac{1}{2}$ inch thick each, and respectively 9 inches and 6 inches deep, were heated to redness, and cooled by half-immersion in cold water twenty times; for effects see figs. 4 and 5.

Fig. 4.

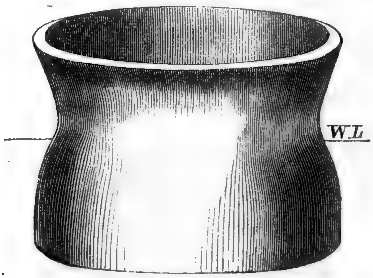
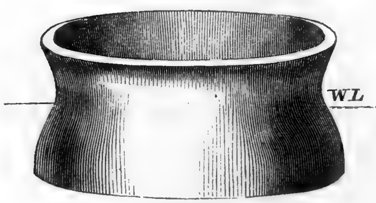


Fig. 5.



One-eighth of full size.

The 9-inch cylinder did not alter on the upper edge, cooled in air; but the lower edge, cooled in water, contracted ·6 inch, and the circumference, at about one inch above the water-line, was reduced 5·5 inches; the internal surface had increased in depth ·35 inch.

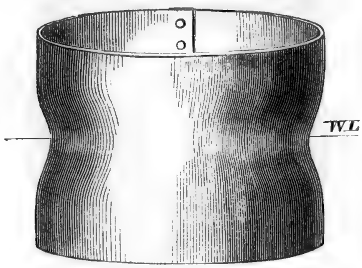
The small cylinder diminished ·7 inch on the upper edge, increased ·3 inch on the lower edge, and contracted 5·25 inches at about 1 inch above the water-line; the internal surface had increased in depth ·3 inch.

Experiment 3.—A cylinder of very thin wrought iron, so thin that

it could not be welded, and was therefore riveted, of the same external dimensions as the 9-inch one of the foregoing experiment, was heated to redness and cooled by half-immersion ten times, in order to test the effect when the thickness of the metal was reduced as much as possible.

The upper and lower edges were not altered materially, while the greatest contraction took place on the water-line, instead of 1 inch above it as in the last experiment, and amounted to 3·5 inches. The depth measured on the curve had increased ·15 inch (see fig. 6).

Fig. 6.



One-eighth of full size.

Experiment 4.—Two wrought-iron cylinders, exactly similar to those used in experiment 2, were heated and cooled by being immersed to two-thirds their depth in water twenty times.

The upper edge of the large cylinder was reduced 2·1 inches, and the lower edge ·9 inch; it contracted 5·9 inches at about an inch above

Fig. 7.

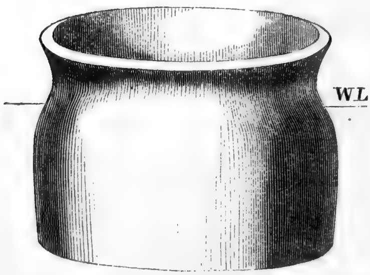
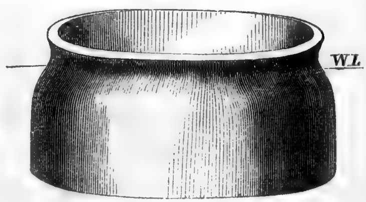


Fig. 8.



One-eighth of full size.

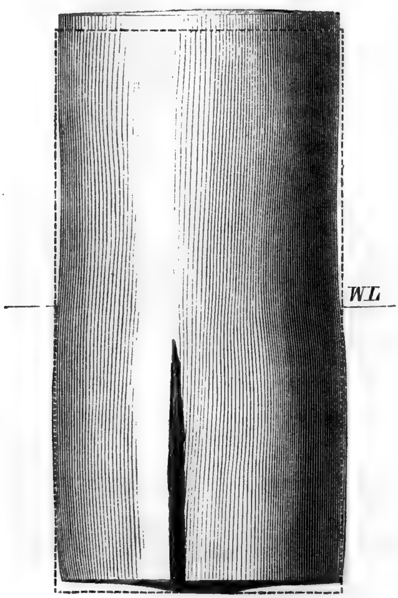
the water-line, and the inside surface had increased in depth ·35 inch (see fig. 7).

The upper edge of the small cylinder was reduced in circumference 3·6 inches and the lower edge ·65 inch, while the greatest contraction at about one inch above the water-line was 4·6 inches; and the internal surface had increased ·15 inch in height (see fig. 8).

Experiment 5.—A solid cylinder of wrought iron, 3 inches in diameter and 6 inches deep, was heated and cooled by being immersed half its depth in water fifteen times.

The greatest contraction took place a little above the water-line and on the lower edge, being in each case ·45 inch; the upper edge was reduced only ·1 inch.

Fig. 9.



One-half of full size. The dotted lines indicate the original form.

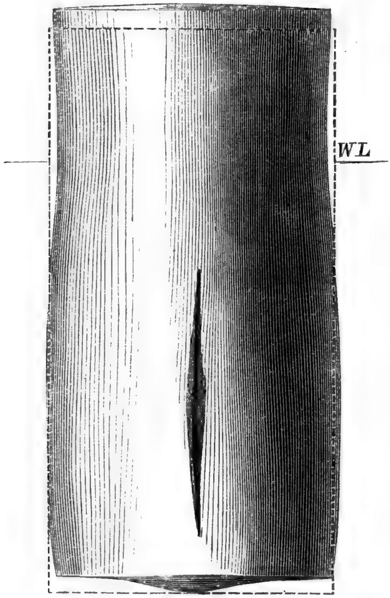
A swell of metal took place on the two ends, but was greatest on the bottom, or that cooled in water, being ·15 inch in height.

The fibre of the iron opened at the fifteenth cooling (see fig. 9).

Experiment 6.—A wrought-iron cylinder exactly similar to the last was cooled by being immersed to two-thirds its depth fifteen times.

The greatest contraction, amounting to ·4 inch, took place a little above the water-line; the upper edge was ·05 inch smaller, and the lower edge ·35 inch, while the swellings on the ends were nearly the same as in the last experiment (see fig. 10).

Fig. 10.



One-half of full size. The dotted lines indicate the original form.

The separation of the fibre took place at the fifteenth cooling.

Experiment 7.—Two flat pieces of wrought iron, each 12 inches long, 6 inches deep, and ·5 inch thick, were heated and cooled twenty times, one being immersed to half, and the other to two-thirds its depth in water.

That immersed one-half had contracted or become indented on the ends fully ·3 inch; the other had similar indentations, but to only

one-half the amount. They were both turned up into the form of an arc, had thickened on their upper edges, and increased ·1 inch in thickness where the contractions on the ends took place (see figs. 11 and 12).

Fig. 11.

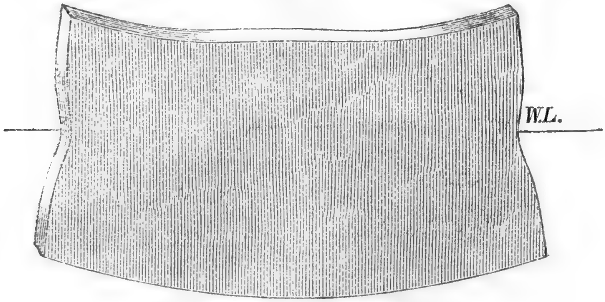
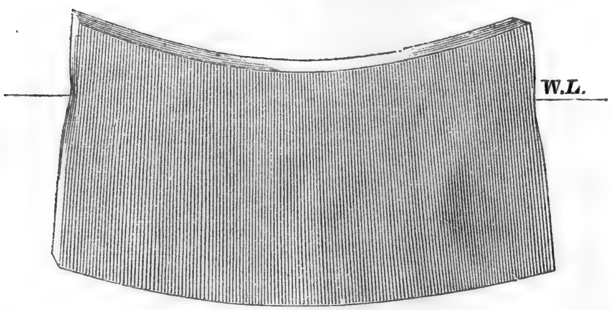


Fig. 12.



One-fourth of full size.

Experiment 8.—Two hollow wrought-iron cylinders, 9 inches deep and 12 inches in diameter, were heated and cooled, one by simple exposure to air (fifteen times), and the other by total immersion in water (ten times). No alteration occurred in the form of either*.

* The cylinder which was cooled in air weighed, before the experiment, 49 lbs. 14·5 ozs., and after the experiment 49 lbs. 11 ozs., showing a loss by scaling of 3·5 ozs.

During the progress of the experiment, however, it was frequently weighed, and was found each time to have increased in weight up to the tenth heating, at which

Experiment 9.—A solid cast-steel cylinder, of the same dimensions as that used in Experiment 5, was heated and cooled by half-immersion twenty times.

The effect obtained was similar to that produced upon the solid wrought-iron cylinders, but the breaking up of the structure was different (see fig. 13). The greatest contraction was slightly above the

Fig. 13.

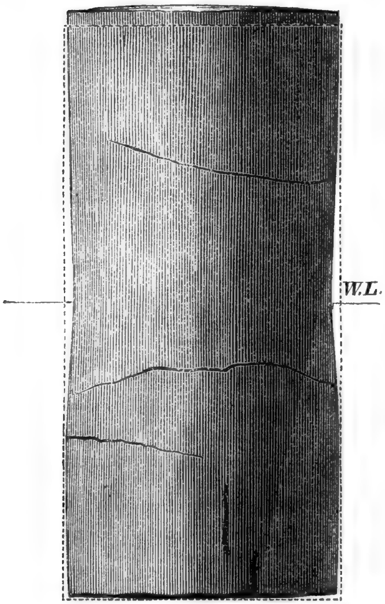


Fig. 14. (Top of fig. 13.)

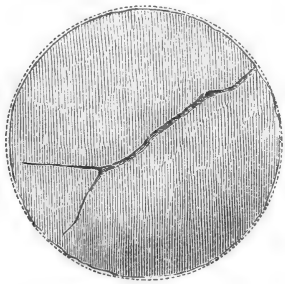
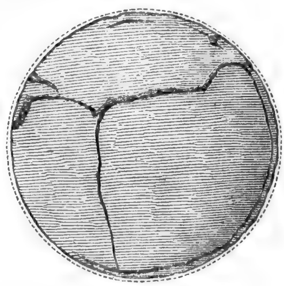


Fig. 15. (Bottom of fig. 13.)



One-half of full size. The dotted lines indicate the original figure.

water-line, and amounted to ·38 inch; the bulgings on the ends were ·075 inch, being much less than on the wrought-iron cylinders.

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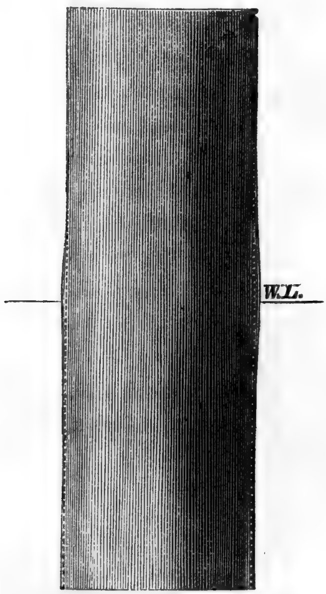
point it weighed 50 lbs. 1·125 oz., or 2·625 ozs. heavier than it was at the commencement; from the tenth to the fifteenth heating the accumulated scales peeled off, and the weight was gradually reduced to that stated above.

That which was cooled in water weighed 50 lbs. 12·5 ozs. before the experiment, and 48 lbs. 14·5 ozs. at its conclusion, giving a loss of 1 lb. 14 ozs., which was due to the action of the water peeling off the scale each time the cylinder was cooled.

Experiment 10.—A hollow brass cylinder, 6 inches long, 2 inches in diameter, and $\frac{1}{16}$th of an inch thick, was heated to redness and cooled by half-immersion thirty-four times.

The effect produced was the opposite to that which took place with the iron cylinders, being an expansion instead of a contraction at the water-line, the amount of which was ·175 inch, and it was also expanded on the lower edge ·1 inch (see fig. 16).

Fig. 16.



One-half of full size. The dotted line indicates the original figure.

Experiment 11.—A hollow gun-metal cylinder was heated to redness and cooled twenty times by half-immersion.

The thickness of metal being greater than in the last experiment, the effect at the water-line was much less, but the lower edge had expanded ·1 inch. It began to crack all over at the last cooling.

Experiment 12.—A hollow tin cylinder was heated in linseed-oil which was brought to a temperature of 400° Fahr.; it was cooled by half-immersion in water five times.

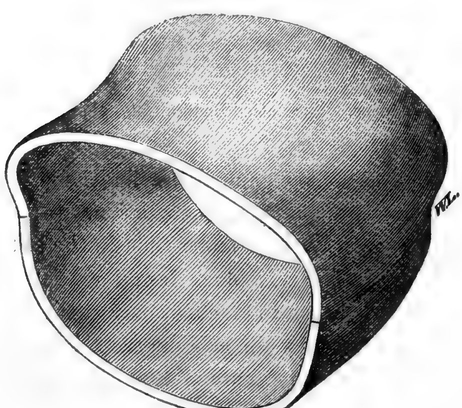
The form was not altered in the least, though the heat was raised in the last instance to the melting-point, as shown by the lower part of the cylinder beginning to melt.

Experiment 13.—A hollow zinc cylinder was heated and cooled by half-immersion fifty times.

It was heated in a wood furnace, the degree of heat to which it was brought being regulated by the melting of a piece of tin which was conveyed at the same time with it into the furnace. Several experiments with pieces of tin and zinc had been previously made, by means of which it was ascertained that in the same temperature tin melted in two-sevenths of the time requisite to melt zinc; hence when the zinc cylinder and piece of tin were placed in the furnace together, the time occupied by the tin in reaching its melting-point was carefully noted, and the cylinder was left in the furnace as long again as the time thus observed; by this means it was brought very nearly to its melting-point without incurring any danger of its actually melting. The last five times, however, it was allowed to remain a little longer in the flame; and the melting upon the top was retarded the last four times by placing a piece of iron upon it, which conducted heat from that part, allowing it to remain half a minute longer in the furnace.

The effect obtained was the same as that produced upon the brass cylinder (Exp. 10), or the opposite of what took place with iron; an

Fig. 17.



expansion of ·175 inch occurred upon the water-line, and of ·115 inch upon the lower edge.

Experiment 14.—The hollow wrought-iron cylinder was heated to redness and cooled by half-immersion on its *side*, instead of on its end as in other experiments, twenty times.

The effect was a very complicated one (see figs. 17, 18, and 19); the dotted lines show the original form.

Fig. 18. (Side view of fig. 17.)

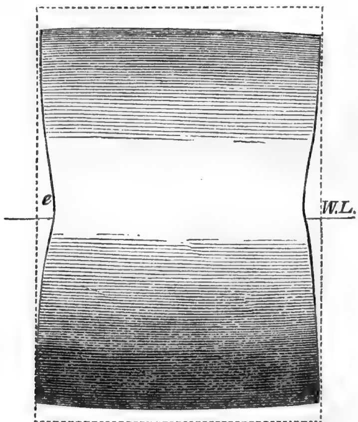
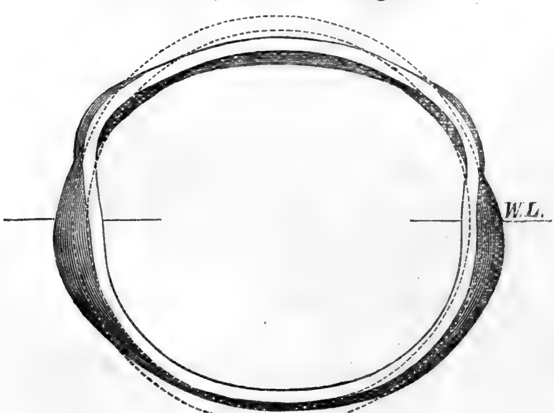


Fig. 19. (Front view of fig. 17.)



The three figures are one-sixth of full size.

Experiment 15.—A solid wrought-iron cylinder was heated to redness and cooled by half-immersion on its side twenty times.

The effect was of a similar nature to that of the last experiment (see figs. 20 and 21).

Fig. 20.

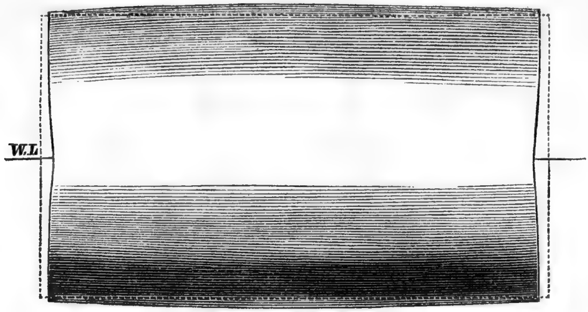
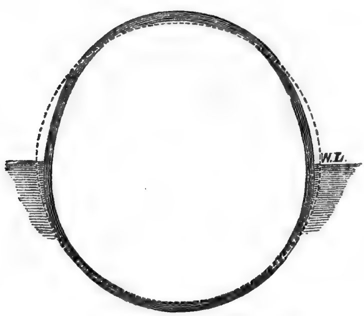


Fig. 21.



One-half of full size. The dotted line indicates original figure.

Experiment 16.—A hollow cast-iron cylinder, the dimensions of which were the same as those of the deep cylinder of Experiment 14, was heated to redness and cooled twice by half-immersion.

At the second cooling it fractured nearly all round, about an inch below the water-line. It expanded all over, but the expansion was least about an inch above the water-line, *i. e.* it did not contract to its original dimensions.

Experiment 17.—A solid cast-iron cylinder, 3 inches in diameter and 6 inches deep, was heated and cooled five times by half-immersion.

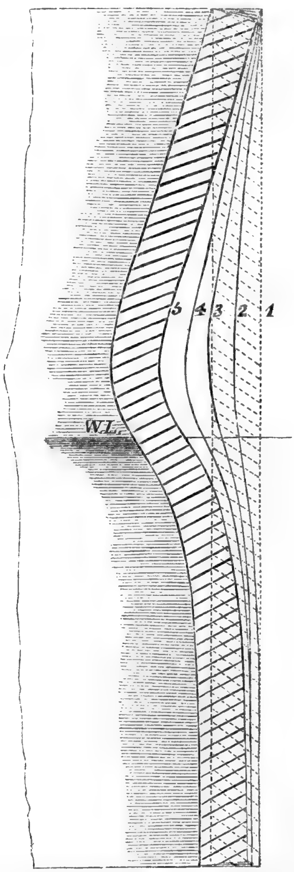
At the fifth cooling it cracked across the bottom; it also expanded

throughout, and the expansion was least a little above the water-line, *i. e.* it did not contract to its original dimensions.

The subjoined figures (half the full size) show the changes produced on the 9-inch cylinders after every five heatings. (Experiments 2 and 4.)

Fig. 22.

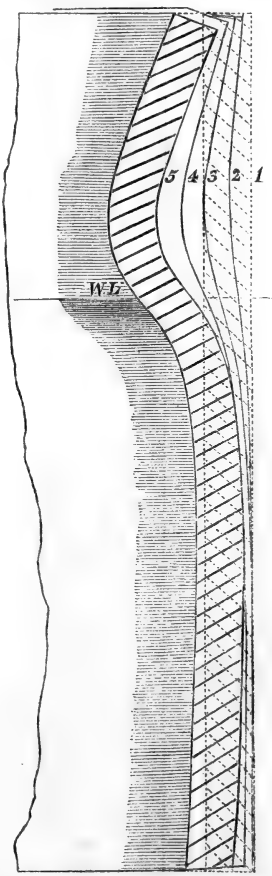
12″ Cylinder, 9″ high, ½″ thick.
Vide fig. 4. Cooled by ½-immersion.



No. 1. External surface, original form.
2. " " after 5 coolings.
3. " " " 10 "
4. " " " 15 "
5. " " " 20 "

Fig. 23.

12″ Cylinder, 9″ high, ½″ thick.
Vide fig. 7. Cooled by ¾-immersion.



No. 1. External surface, original form.
2. " " after 5 coolings.
3. " " " 10 "
4. " " " 15 "
5. " " " 20 "

Tabulated Statement of the Results of the Experiments.

Number of experiment.	Kind of metal.	Number of coolings.	Amount of immersion.	Form of article, &c.	Dimensions, in inches. Before experiment.	After experiment.	Difference.
1[a].	Wrought iron.	5	$\frac{1}{3}$	Hoop-tire for a 4′ 2″ wheel:—			
				External circumf. of upper edge...	155·5	147·5	−8·0
				do. do. lower edge...	155·5	156·375	+0·875
				Bevel of face	90°	69°	−21°
2[b].	Wrought iron.	20	$\frac{1}{3}$	12″ cylinder, 9″ deep and $\frac{1}{2}$″ thick:			
				Internal circumf. of upper edge...	37·6	37·6	0·0
				do. do. contraction ..	37·6	32·1	−5·5
				do. do. lower edge...	37·6	37·0	−0·6
				Depth, perpendicular	9·0	8·8	−0·2
				do. on curve, external	9·0	9·15	+0·15
				do. do. internal	9·0	9·35	+0·35
2[c].	Wrought iron.	20	$\frac{1}{2}$	12″ cylinder, 6″ deep and $\frac{1}{2}$″ thick:			
				Internal circumf. of upper edge...	37·6	36·9	−0·70
				do. do. contraction...	37·6	32·35	−5·25
				do. do. lower edge...	37·6	37·9	+0·30
				Depth, perpendicular	6·0	5·7	−0·30
				do. on curve, external	6·0	6·05	+0·05
				do do internal	6·0	6·30	+0·30
3[d].	Wrought iron.	10	$\frac{1}{2}$	12″ cylinder, 9″ deep, thin sheet:—			
				External circumf. of upper edge...	38·40	38·40	0·00
				do. do. contraction...	38·40	34·90	−3·50
				do. do. lower edge...	38·40	38·45	+0·05
				Depth, on curve	9·00	9·15	+0·15
4[e].	Wrought iron.	20	$\frac{2}{3}$	12″ cylinder, 9″ deep and $\frac{1}{2}$″ thick:—			
				External circumf. of upper edge...	40·90	38·80	−2·10
				do. do. contraction...	40·90	35·00	−5·90
				do. do. lower edge...	40·90	40·00	−0·90
				Depth, perpendicular	9·00	8·80	−0·20
				do. on curve, external	9·00	9·00	0·00
				do. do. internal	9·00	9·35	+0·35
4[f].	Wrought iron.	20	$\frac{2}{3}$	12″ cylinder, 6″ deep and $\frac{1}{2}$″ thick:—			
				External circumf. of upper edge...	40·8	37·2	−3·6
				do. do. contraction...	40·8	36·2	−4·6
				do. do. lower edge...	40·8	40·15	−0·65
				Depth, perpendicular	6·0	6·0	0·0
				do. on curve, external	6·0	6·05	+0·05
				do. do. internal	6·0	6·15	+0·15
5[g].	Wrought iron.	15	$\frac{1}{2}$	3″ cylinder, 6″ deep, solid:—			
				Circumference, upper edge	9·4	9·3	−0·1
				do. contraction	9·4	8·95	−0·45
				do. lower edge	9·4	8·95	−0·45
				Bulge on upper end	0·00	0·04	+0·04
				do. lower end	0·00	0·15	+0·15

[a] For remarks see end of Table, p. 470.

TABLE (*continued*).

Number of experiment.	Kind of metal.	Number of coolings.	Amount of immersion.	Form of article, &c.	Dimensions, in inches. Before experiment.	After experiment.	Difference.
6h.	Wrought iron.	15	⅔	3″ cylinder, 6″ deep, solid:—			
				Circumference, upper end	9·40	9·35	−0·05
				do. contraction.........	9·40	9·00	−0·40
				do. lower edge	9·40	9·05	−0·35
				Bulge on upper end	0·00	0·05	+0·05
				do. lower end	0·00	0·20	+0·20
7i.	Wrought iron.	20	½	Flat piece, 12″×6″×½″:—			
				Length on curve, upper edge ...	12·00	10·75	−1·25
				do do lower edge ...	12·00	12·10	+0·10
				Breadth, ends...	6·00	5·75	−0·25
				do. centre	6·00	6·00	0·00
				Upper edge, out of straight	0·00	0·60	+0·60
				Indentation on ends	0·00	0·30	+0·30
7k.	Wrought iron.	20	⅔	Flat piece, 12″×6″×½″:—			
				Length on curve, upper edge......	12·00	11·10	−0·90
				do. do. lower edge......	12·00	12·20	+0·20
				Breadth, ends	6·00	5·87	−0·13
				do. centre	6·00	5·95	−0·05
				Upper edge, out of straight	0·00	0·50	+0·50
				Indentation on ends	0·00	0·15	+0·15
8l.	Wrought iron.	15 10	0 total	12″ cylinder, 9″ deep, ½″ thick do. do. do.	No	effect.	
9m.	Cast steel.	20	½	3″ cylinder, 6″ deep, solid:—			
				Circumference, upper edge	9·03	8·93	−0·10
				do. contraction	9·03	8·65	−0·38
				do. lower edge	9·03	8·93	−0·10
				Depth, perpendicular	6·00	6·10	+0·10
10n.	Brass.	34	½	2″ cylinder, 6″ deep, $\frac{1}{16}$″ thick:—			
				External circumf. of upper edge...	6·175	6·175	0·000
				do. do expansion ...	6·175	6·350	+0·175
				do. do. lower edge...	6·175	6·270	+0·095
11o.	Gun-metal.	20	½	3″ cylinder, 6″ deep, ½″ thick:—			
				External circumf. of upper edge...	9·25	9·24	−0·01
				do. do. on water-line...	9·25	9·26	+0·01
				do. do. of lower edge..	9·25	9·38	+0·13
12.	Tin.	5	½	2″ cylinder, 5″ deep, ¼″ thick	No	effect.	
13.	Zinc.	50	½	3″ cylinder, 6″ deep, ½″ thick:—			
				External circumf. of upper edge...	9·525	9·575	+0·050
				do. do. expansion ...	9·525	9·700	+0·175
				do. do. lower edge...	9·525	9·630	+0·105

TABLE (*continued*).

Number of experiment.	Kind of metal.	Number of coolings.	Amount of immersion.	Form of article, &c.	Dimensions, in inches. Before experiment.	After experiment.	Difference.
14[p].	Wrought iron.	20	½ on its side.	12″ cylinder, 9″ deep, ½″ thick:—			
				External circumference of edges..	40·65	39·86	−0·79
				do. do. centre..	40·65	41·05	+0·40
				Depth on curve, part cooled in air.	9·00	9·00	0·00
				do. do. water-line.	9·00	8·25	−0·75
				do. do. in water...	9·00	8·80	−0·20
				Swell of side, 1″ below W.L. (at *a*, *b*)	0·00	1·00	+1·00
				Hollow of side, 4″ above do. (at *c*, *d*)	0·00	0·40	+0·40
				Longest ex. diam. 1″ below W. L.	12·94	14·275	+1·335
				Shortest do. at rt. angles to W.L.	12·94	12·00	−0·94
				Indentation of edges a little above water-line at *e*	0·00	0·45	+0·45
15[q].	Wrought iron.	20	½ on its side.	3″ cylinder, 5⅜″ deep, solid:—			
				External circumference of edges..	9·4	9·2	−0·2
				do do. centre.	9·4	9·475	+0·075
				Depth along part cooled in air ...	5·375	5·150	−0·225
				do. do. on W.L.	5·375	5·100	−0·275
				do. do. in water.	5·375	5·225	−0·150
				Longest diam. at rt. angles to W.L.	3·000	3·100	+0·100
				Shortest do. parallel with W.L. and a little below it	3·000	2·760	−0·240
16.	Cast iron.	2	½	12″ cylinder, 9″ deep, ½″ thick:—			
				External circumf. of upper edge...	40·90	41·05	+0·15
				do. do. least expansion	40·90	40·95	+0·05
				do. do. of lower edge—	40·90	41·15	+0·25
17.	Cast iron.	5	½	3″ solid cylinder, 6″ deep:—			
				External circumf. of upper edge...	9·4	9·55	+0·15
				do. do. least expansion	9·4	9·50	+0·10
				do. do. of lower edge...	9·4	9·55	+0·15

Remarks.

[a] The width was unaltered, and the thickness of the upper edge slightly increased. Figs. 1 and 2. [b] Fig. 4. [c] Fig. 5. [d] Fig. 6. [e] Fig. 7. [f] Fig. 8.

[g] The fibre opened at the fifteenth cooling. Fig. 9.

[h] The fibre opened at the fifteenth cooling after having exhibited a slight crack for two or three previous coolings. Fig. 10.

[i] The thickness of the metal at the indentation on ends increased ·1″. Fig. 11.

[k] The thickness of the metal at the indentation on ends increased similarly to the last. Fig. 12.

[l] Cooled in air 15 times. Cooled in water 10 times.

[m] The ends became slightly rounded. Fig. 13.

[n] At the last cooling the lower end of the cylinder began to crumble away in the water. Fig. 16.

[o] The expansion of the lower end may probably be due to the cracking of the metal, which was greatest at that part.

[p] Figs. 17, 18, 19. There was an increased thickness of metal at *e*.

[q] Figs. 20, 21.

[The cause of the curious phenomenon described by Colonel Clerk in the preceding paper seems to be indicated by some of the figures, especially those relating to hollow cylinders of wrought iron, which are very instructive.

Imagine such a cylinder divided into two parts by a horizontal plane at the water-line, and in this state immersed after heating. The under part, being in contact with water, would rapidly cool and contract, while the upper part would cool but slowly. Consequently by the time the under part had pretty well cooled, the upper part would be left jutting out; but when both parts had cooled, their diameters would again agree. Now in the actual experiment this independent motion of the two parts is impossible, on account of the continuity of the metal; the under part tends to pull in the upper, and the upper to pull out the under. In this contest the cooler metal, being the stronger, prevails, and so the upper part gets pulled in, a little above the water-line, while still hot. But it has still to contract on cooling; and this it will do to the full extent due to its temperature, except in so far as it may be prevented by its connexion with the rest. Hence, on the whole, the effect of this cause is to leave a permanent contraction a little above the water-line; and it is easy to see that the contraction must be so much nearer to the water-line as the thickness of the metal is less, the other dimensions of the hollow cylinder and the nature of the metal being given. When the hollow cylinder is very short, so as to be reduced to a mere hoop, the same cause operates; but there is not room for more than a general inclination of the surface, leaving the hoop bevelled.

But there is another cause of deformation at work, the operation of which is well seen in figs. 2 and 3. Imagine a mass of metal heated so as to be slightly plastic, and then rapidly cooled over a large part of its surface. In cooling, the skin at the same time contracts and becomes stronger, and thereby tends to squeeze out its contents. This accounts for the bulging of the ends of the solid cylinders of wrought iron and the rents seen in their cylindrical surface. The skin at the bottom is of course as strong as at the sides in the part below the water-line; but a surface which resists extension far more than bending has far less power to resist pressure of the nature of a fluid pressure when plane than when convex. The effect of the cause first explained is also manifest in these cylinders,

although it is less marked than in the case of the hollow cylinders, as might have been expected.

The tendency of the cooled skin of a heated metallic mass to squeeze out its contents appears to be what gives rise to the bulging seen near the water-line in the hollow cylinder of brass. Wrought iron, being highly tenacious even at a comparatively high temperature, resists with great force the sliding motion of the particles which must take place in order that the tendency of the cooled skin to squeeze out its contents may take effect; but brass, approaching in its hotter parts more nearly to the state of a molten mass, exhibits the effect more strongly. It seems probable that even in the case of brass a *very* thin hollow cylinder would exhibit a contraction just above the water-line. Should there be a metal or alloy which about the temperatures with which we have to deal was stronger hot than cold, the effect of the cause first referred to would be to produce an expansion a little below the water-line.—G. G. S.]

March 12, 1863.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

His Grace the Archbishop of York was admitted into the Society.

The following communications were read:—

I. "On the Influence of Temperature on the Electric Conducting Power of Thallium and Iron." By A. MATTHIESSEN, F.R.S., and C. VOGT, Ph.D. Received February 12, 1863.

(Abstract.)

Thallium.—The experiments detailed in this paper were made with specimens of thallium lent to us by Mr. Crookes and Professor Lamy of Lille. The values obtained for the conducting power, together with the formulæ for the correction of the conducting power for temperature of the different specimens, were:—

For Mr. Crookes's metal,

1st wire.	2nd wire at 0°.
$\lambda=9{\cdot}364-0{\cdot}037936t+0{\cdot}00008467t^2$;	9·169.

For M. Lamy's first specimen,

	2nd wire at 0°.	3rd wire.
$\lambda=9{\cdot}419-0{\cdot}039520t+0{\cdot}00009656t^2$;	9·082 ;	9·223.

Second specimen,

	2nd wire at 0°.
$\lambda=9{\cdot}054-0{\cdot}034697t+0{\cdot}00006554t^2$;	9·226 ;

or as mean of all the determinations, some of which are not given here,

$$\lambda=9{\cdot}163-0{\cdot}036894t+0{\cdot}00008104t^2.$$

The conducting power of thallium therefore decreases between 0° and 100° 31·420 per cent., which is a larger percentage decrement than that obtained for many other pure metals, namely 29·307 per cent.*

Iron.—The specimens of iron experimented were, with two exceptions, lent us by Dr. Percy. In the following Table we give the results obtained with them :—

(1.) Electrotype iron, deposited from solution of pure sulphate of iron. The strips were very thin and porous; we could not therefore obtain concordant values for the conducting power, but we were able to determine the percentage decrement in the conducting power between 0° and 100°. We have, for the above reason, taken the first observed conducting power equal 100.

$$\lambda=100-0{\cdot}51182t+0{\cdot}0012915t^2,$$

corresponding to a percentage decrement of 38·262 per cent.

(2.) No. 1, annealed and cooled in hydrogen.

$$\lambda=100-0{\cdot}51894t+0{\cdot}0013415t^2,$$

corresponding to a percentage decrement of 38·479 per cent.

(3.) Electrotype iron, a strip cut from the same foil as No. 1.

$$\lambda=100-0{\cdot}51355t+0{\cdot}0013221t^2,$$

corresponding to a percentage decrement of 38·134 per cent.

(4.) No. 3, annealed in air.

$$\lambda=100-0{\cdot}50895t+0{\cdot}0002735t^2,$$

corresponding to a percentage decrement of 38·160 per cent.

(5.) This, as well as Nos. 6, 7, 8, were specimens of iron which have been analysed. They were all hard drawn.

$$\lambda=15{\cdot}719-0{\cdot}074370t+0{\cdot}0001763t^2,$$

corresponding to a percentage decrement of 36·070 per cent.

* Phil. Trans. 1862, Part I.

(6.) $$\lambda=15{\cdot}672-0{\cdot}074045t+0{\cdot}0001761t^2,$$

corresponding to a percentage decrement of 36·010 per cent.

(7.) $$\lambda=14{\cdot}269-0{\cdot}064133t+0{\cdot}0001456t^2,$$

corresponding to a percentage decrement of 34·742 per cent.

(8.) $$\lambda=12{\cdot}342-0{\cdot}055894t+0{\cdot}0001379t^2,$$

corresponding to a percentage decrement of 34·117 per cent.

(9.) Strip of iron, heated in a current of hydrogen at a red heat for two hours. This, as well as Nos. 10, 11, 12, were hardened.

$$\lambda=14{\cdot}673-0{\cdot}067999t+0{\cdot}0001597t^2,$$

corresponding to a percentage decrement of 35·459 per cent.

(10.) As No. 9, heated for three hours under sugar charcoal in a current of hydrogen; the carbon taken up was 0·99 per cent.

$$\lambda=10{\cdot}654-0{\cdot}044560t+0{\cdot}00009789t^2,$$

corresponding to a percentage decrement of 32·637 per cent.

(11.) As No. 9, heated for four hours under sugar charcoal in a current of hydrogen; the carbon taken up was 0·933 per cent.

$$\lambda=9{\cdot}925-0{\cdot}040097t+0{\cdot}00009168t^2,$$

corresponding to a percentage decrement of 31·163 per cent.

(12.) As No. 9, heated for three hours under sugar charcoal in a current of hydrogen; the carbon taken up was 1·06 per cent.

$$\lambda=9{\cdot}457-0{\cdot}037573t+0{\cdot}00008642t^2,$$

corresponding to a percentage decrement of 30·592 per cent.

(13.) Thin music wire, melted with one quarter of its weight of peroxide of iron under a flux of plate glass.

$$\lambda=13{\cdot}381-0{\cdot}056829t+0{\cdot}0001230t^2,$$

corresponding to a percentage decrement of 33·278 per cent.

(14.) A piece of narrow watch-spring.

$$\lambda=8{\cdot}565-0{\cdot}029099t+0{\cdot}00005383t^2,$$

corresponding to a percentage decrement of 27·689 per cent.

(15.) Commercial iron wire.

$$\lambda=13{\cdot}772-0{\cdot}058970t+0{\cdot}0001242t^2,$$

corresponding to a percentage decrement of 33·801 per cent.

From the results obtained, it is obvious that the higher the conducting power the higher the percentage decrement in the conducting power between 0° and 100°. This has been proved to be the case with about 100 alloys with which we have experimented. We have also found that we may deduce the conducting power of a pure metal from an impure one when the impurity does not reduce the conducting

power more than, say, 10 to 20 per cent. According to our experiments, the percentage decrement in the conducting power of an impure metal between 0° and 100° varies in the same ratio as the conducting power of the impure metal at 100°, compared with that of the pure metal at 100°.

Thus, from specimens Nos. 5, 6, 7, 9, 13, and 15, the conducting power of pure iron was found to be at 0°=16·725.

In conclusion, we give the values found for specimens of cobalt and nickel wire lent to us by Professor Wöhler. They were as follows:—

Cobalt wire.

$$\lambda=12{\cdot}930-0{\cdot}035521t+0{\cdot}00004887t^2,$$

corresponding to a percentage decrement of 23·692 per cent.

Nickel wire.

$$\lambda=12{\cdot}222-0{\cdot}040787t+0{\cdot}000\ 7088t^2,$$

corresponding to a percentage decrement of 27·573 per cent.

Although these metals were said to be chemically pure, the results obtained seem to indicate that they are not so, having probably taken up some impurities in the process of fusion.

The following Table of the conducting powers of pure metals shows the place which the metals treated of in this paper take in the series.

	Conducting power at 0°.
Silver (hard drawn)	100·00
Copper (hard drawn)	99·95
Gold (hard drawn)	77·96
Zinc	29·02
Cadmium	23·72
Cobalt *	17·22
Iron * (hard drawn)	16·81
Nickel *	13·11
Tin	12·36
Thallium	9·16
Lead	8·32
Arsenic	4·76
Antimony	4·62
Bismuth	1·245

* Probable value for the pure metal deduced from the observations with the impure one.

II. "On the Amyloid Substance of the Liver, and its ultimate destination in the Animal Economy." By Robert McDonnell, M.D. Communicated by William Bowman, Esq. Received February 13, 1863.

(Abstract.)

After briefly referring to the discovery of the amyloid substance of the liver, and the earlier history of the subject, the author examines the facts which have induced Dr. Pavy to conclude that this substance is not normally transformed into sugar during life. The author being led, after a careful repetition of Dr. Pavy's experiments, to concur in his views, asks, If then the amyloid substance of the liver be not converted into sugar, what becomes of it? what is its normal destination in the animal economy? It is the object of the memoir to attempt to answer this question, which, it must be admitted, is one of the greatest delicacy; nevertheless there appears on the whole to be evidence that the amyloid substance met with in the liver is on its way upwards towards the more exalted or complex immediate animal principles; that, in fact, the process of healthy assimilation tends, if the expression may be used, to promote it from the rank of ternary (hydrocarbonous) to that of quaternary (azotized) compounds; and that its conversion into sugar is to be looked upon as a deviation from this progressive course—a dissimilative instead of an assimilative process. In order to establish this view it became necessary—

1st. To investigate the chemical and physiological relations of the amyloid substance, not only of the liver, but of other organs and tissues, and to test the very interesting results, which are for the most part due to M. Charles Rouget.

2ndly. To compare the portal and hepatic blood with each other, and with arterial and venous blood derived from other sources; and

3rdly. To consider the relations to each other of the different functions performed by the liver. For if it be true, as Lehmann, Brown-Séquard, and others have asserted, that the fibrine and much of the albumen of the portal blood vanishes in the liver, and that at the same time that it destroys these azotized compounds it forms its non-azotized amyloid substance, and excretes bile containing so little nitrogen that it need hardly be taken into account, are we

not, from the consideration of these functions, led to infer that the nitrogen which leaves the liver by no other outlet may go forth in the hepatic blood in union with the amyloid substance thus changed into a new azotized principle;—that thus the liver is a great blood-making organ, in which there is constantly going on a reconstruction of certain ingredients of the blood; that in it the fibrine, &c., which has done its work, is disintegrated, the hydrocarbons of the bile abstracted, and the nitrogen combined with the amyloid substance, which, instead of being normally changed into sugar, emerges from the liver a constituent principle of the protoplasma, from the bosom of which (to use the words of Bernard with reference to the fœtal tissues) organic evolution is to be accomplished?

Of the existence of the Amyloid Substance of Bernard in the Placenta and other Organs and Tissues.

The cells of the placenta contain, during the earlier stages of embryonic life, animal dextrine, having characters identical with those of the amyloid substance of the liver; its presence may be readily demonstrated under the microscope. Bernard has discovered it in the placentæ of rabbits, guinea-pigs, &c. He also made the very interesting observation that the multiple placentulæ of the ruminants do not contain any amyloid substance, but that in this class of animals this substance is found in certain cells of the amnion. The presence, however, in the amnion or the placenta, of epithelial cells containing amyloid substance, is a fact quite secondary to the general fact that this substance enters largely into the constitution of most of the tissues of the embryo. Its existence does not indicate a new function of an organ doing temporarily the duty of the liver, but it indicates a new fact with regard to the development of certain structures and a new property of tissue. During embryonic life a great part of the fœtal tissues are found to be so impregnated with amyloid substance, that it appears to be the formative material from which these tissues are evolved; and, in fact, it would seem to be related to their growth and development, as starch is to the growth and development of the tissues of vegetables. In the skin of the chick *in ovo*, and of the fœtuses of rabbits, cats, guinea-pigs, sheep, oxen, pigs, it is readily demonstrated; it is seen by the addition of acidulated tincture of iodine,

and is most abundant at the points where the aggregation of epithelial cells shows that the feathers and hairs are about being developed. The horny structures contain it plentifully; in the bill, the hoof, and the claws it exists in large proportion. From the hoof of a fœtal calf of about four months enough may be obtained, by the alcoholic solution of potash, for chemical examination and fermentation. The muscular tissues of the fœtus are full of it; from 20 to 50 per cent. can be extracted from the muscles of fœtal calves of from three to seven months by the aid of the alcoholic solution of potash.

Having arrived, by a repetition of Dr. Pavy's ingenious experiments, at the conclusion that the amyloid substance of the liver is not normally changed into glucose, and finding on examination the accuracy of the facts concerning the physiological relations of the amyloid substance to the fœtal and other tissues, discovered by M. Charles Rouget, and investigated by Bernard himself, the question presents itself, May it not be that the liver does for the adult what divers tissues do during the development of the fœtus? May not this great organ form, with the help of the amyloid substance secreted in its cells, a nitrogenous compound, just as the muscles of the fœtus convert the amyloid substance contained in them into the highly nitrogenous material of muscular tissue?

May not, in fact, the amyloid substance of the liver be the basis of an azotized protoplasma forming a constituent of the blood of the adult animal, as the amyloid substance of muscle is the basis of the material from which the evolution of muscular tissue is accomplished?

Even a superficial consideration of the functions performed by the liver leads one to answer these questions in the affirmative. For if it be true that the blood which enters the liver is rich in fibrine and albumen, and that these materials are so completely changed within this organ that little or none of them leave it by the hepatic vessels, what becomes of them? It is true their hydrocarbonous constituents may be thrown out as bile. But what of the nitrogen contained in them? If it does not escape by the bile-ducts, it has no other mode of exit save by the hepatic vessels. The author conceives it to be reunited with the hydrocarbonous amyloid substance, and to leave the liver as a newly-formed proteic compound, partly perhaps as

globuline, and partly as material, in its reactions resembling caseine in some respects, in others albuminose, and which is fully described in the memoir. These considerations lead to the necessity of investigating the several distinct functions of the liver :—

1st. As to its action on the fibrine and albumen of the blood.

2nd. As to the constitution of healthy bile (so far as its azotized elements are concerned).

3rd. As to the relative composition and characters of the blood which enters and of that which leaves the liver.

The author adds his testimony to that of Lehmann and Brown-Séquard as regards the fibrine-destroying function of the liver ; he attempts to show that, in proportion to the amount of fibrine which disappears in the liver, the quantity of nitrogen eliminated in the form of bile is very small indeed, but that the blood in passing through the liver becomes greatly enriched in colourless corpuscles, and that it contains more abundantly than other blood an azotized compound, resembling what has been described by some authors as blood-caseine. This material, although resembling, is not identical with caseine ; it can be obtained from the serum of blood abstracted by a peculiarly contrived instrument (a drawing of which accompanies the paper) from the vena cava, close to the mouths of the hepatic veins.

Whatever may be its precise chemical composition and characteristics, whether it is to be regarded as a form of albumen, or albumen-peptone (albuminose), or caseine, it is enough to state, that during active digestion the blood which leaves the liver contains a proteic compound, that it is richer in this compound than arterial blood, and that this latter is richer in it than ordinary venous blood, or than that of the portal vein. At the same time the blood of the hepatic veins contains a far larger quantity of colourless blood-corpuscles than the portal blood.

A microscopic examination of these kinds of blood shows that the colourless corpuscles are from five to ten times more numerous in the former than in the latter. Physiologists are so familiar with this fact, as well as with the chief peculiarities of the colourless corpuscles of hepatic blood, that it is unnecessary to dwell upon the circumstances which have induced some of the most distinguished among them to regard as the most important function of the liver, the

formation, or at least the rejuvenescence, of the blood-corpuscles. Dr. Carpenter conceives that the appearance of the colourless corpuscles of the blood may be regarded as a phenomenon analogous to the development of cells in the albumen of seeds in the vegetable kingdom. He also supposes that these cells aid in the conversion of crude alimentary matters into proximate principles. Additional support is given to each supposition by the notion that these colourless cells stand in close relationship to the material formed in the liver, so closely resembling dextrine of vegetable origin.

It is true that there is nothing novel in the view that the liver is a great blood-forming organ, or rather that it is an organ in which certain components of the blood are disintegrated, while from some of the matter so disintegrated a constant reconstruction of the blood is going forward; yet it is certain that, not long since, physiologists would have been unwilling to admit that materials constituted as the colourless blood-cells or caseine could be formed within the liver from a substance resembling starch taking to itself nitrogen derived, as one may say, from the retrogressive metamorphosis of tissue. It is very improbable that, looking to the liver alone, such a conclusion would have been arrived at. The consideration, however, of the physiological relations of the amyloid substance (of Bernard), as regards the development of the azotized tissues of the fœtus,—the fact that it is, so far as they are concerned, a protoplasma, which, by taking to itself nitrogen, terminates in the evolution of fully-formed nitrogenous tissues,—prepares one to consider the idea that the liver evolves its proteic compounds during adult life by a somewhat similar process.

To M. Charles Rouget we unquestionably owe the observation of the fundamental facts which lead to the foregoing conclusions; yet the author hopes that the recapitulation of facts in this communication will be found worthy of the consideration of physiologists; for he conceives that not only is the view of the subject which he has ventured to adopt in harmony with a great number of hitherto unexplained circumstances, but that it gives a solution more satisfactory than any yet given of certain pathological phenomena which it would be out of place to speak of here.

III. Supplement to a Paper "On the Differential Equations of Dynamics." By Professor GEORGE BOOLE, F.R.S. Received February 9, 1863.

(Abstract.)

It is shown in the general paper that if an integral of any one equation of the peculiar system of (partial differential) equations there discussed be found, then if a certain numerical result of subsequent and always possible operations prove *odd*, an integral of the entire system can be found by the solution of a single differential equation of the first order. It is shown in the paper now sent that, when the above numerical result is *even**, we can reduce the original system of partial differential equations into a new system, fewer in number by unity at least, and of the same general character, so as to admit of a repetition of the same procedure. Thus the common integral sought will finally be given either by the solution of a single differential equation of the first order, or by finding one integral of the single partial differential equation, which, in the most unfavourable case conceivable, will remain at last.

March 19, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read :—

I. "On Peculiar Appearances exhibited by Blood-corpuscles under the influence of Solutions of Magenta and Tannin." By WILLIAM ROBERTS, M.D., Physician to the Manchester Royal Infirmary. Communicated by Dr. SHARPEY, Sec. R.S. Received February 18, 1863.

The object of the following paper is to give an account of certain observations which seem to indicate that the cell-wall of the vertebrate blood-disk does not possess the simplicity of structure usually attributed to it.

It is well known that the blood-corpuscles, when floating in their own serum, or after having been treated with acetic acid or water,

* Also when *odd*, but then not required.

appear to be furnished with perfectly plain envelopes, composed of a simple homogeneous membrane, without distinction of parts. But, as will appear from the observations here to be related, when the blood is treated with a solution of magenta (nitrate of rosanilin) or with a dilute solution of tannin, the corpuscles present changes which seem irreconcileable with such a supposition.

Attention is first asked to the effects of magenta. When a speck of human blood was placed on a glass slide and mixed with a drop of a watery solution of magenta*, the following changes were observed. The blood-disks speedily lost their natural opacity and yellow colour; they became perfectly transparent, and assumed a faint rose colour; they also expanded sensibly, and lost their biconcave figure. In addition, a dark-red speck made its appearance on some portion of their periphery. The pale corpuscles took the colour much more strongly than the red; and their nuclei were displayed with great clearness, dyed of a magnificent carbuncle-red. Many of the nuclei were seen in the process of division, more or less advanced; and in some cells the partition had resulted in the production of two, three, or even four distinct secondary nuclei.

These appearances were first observed in freshly-drawn blood from the finger. Subsequently blood from the horse, pig, ox, sheep, deer, camel, cat, rabbit, and kangaroo was examined in like manner. The effect on the red corpuscles (to which all the observations hereinafter recorded are exclusively confined) was in each instance the same as n human blood.

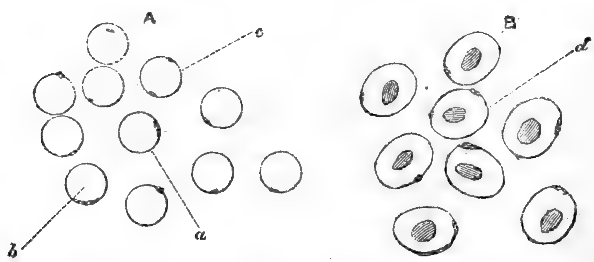
The nucleated blood-disks of the oviparous classes, when treated similarly, yielded analogous results. The coloured contents were forthwith discharged; the central nucleus came fully into view, and assumed a deep-red colour; the corpuscles expanded, they lost something of their oval form, and approached nearly, or sometimes quite, to a circular outline. Lastly, there appeared on the periphery a dark-red macula, of a character and position resembling that seen on the mammalian blood-disk. Such a macula was detected in the fowl, in the frog, and in the dace and minnow.

* The solution I found to answer best in these experiments was a nearly saturated solution of nitrate of rosanilin, made by boiling the salt in water, and filtering after it had stood twenty-four hours, then diluting slightly with water to prevent precipitation.

Owing, however, to the large quantity of molecular matter floating in the serum, and which was coloured by the magenta, difficulties were found in preparing specimens which carried conviction that the macula in question was not an adhering granule. It was also found that it required a nice adjustment of the relative quantities of the solution and of the blood to bring it out. It was only when the right proportions were hit, and especially when the disks were made to roll over in the field of the microscope, that the existence of a coloured particle organically connected with the cell-wall could be satisfactorily made out. The best specimens were prepared from human blood drawn in the fasting condition, and from the blood of a kitten two days old.

From well-prepared specimens of human blood the following particulars were gathered (see fig. 1) :—Nearly every disk possessed the parietal macula ; it could be distinctly recognized in nine-tenths of them ; and in several of those in which it was not at first visible, it came into view as the corpuscles revolved in the field.

Fig. 1.



A. Human blood. B. Fowl's blood treated with magenta.

The macula was clearly situated in the cell-wall, and not in the interior of the corpuscle. Usually it appeared as if imbedded or set in the rim of the disk, like the jewel in a diamond ring ; but sometimes it occupied various positions on the flat surfaces, and when so placed, the spot was difficult or impossible to detect.

It commonly presented a thickly lenticular shape ; sometimes it was square, and occasionally in appearance vesicular (fig. 1, A, *a*). In some instances, and especially in long-kept specimens, the particle was seen to stand out on the outline of the disk like an excrescence.

Still more rarely, instead of a spot, a thick red line ran round the circumference for a quarter or a third of its extent (fig. 1, A, *b*).

As a rule it was extremely minute, covering generally not more than a twentieth or thirtieth of the circumference; but there was a considerable variation in its magnitude and distinctness. Very rarely two specks could be seen; but the occurrence of adhering granules rendered the verification of this point extremely difficult.

This description applies, so far as the inquiry has yet been prosecuted, to the mammalian blood-disk generally, making allowances for differences in size. In the camel the macula occupied indifferently any part of the oval outline.

Among the oviparous classes, the blood of the fowl, frog, dace, and minnow has been most fully examined (see fig. 1, B); but the blood of the sparrow, duck, goose, and turkey was also searched, as well as that of the newt and carp.

In all of these a tinted particle appeared, more or less constantly, in the cell-wall, when the corpuscles were treated with magenta*. The presence of a central nucleus in these classes caused the macula to be invisible more frequently than in mammalia, inasmuch as it suffered eclipse when situated over or under the central nucleus.

In the fowl, dace, and minnow it was found easy to bring out the parietal macula; in the fish two spots were not unfrequently seen. The macula was situated indifferently on any part of the periphery; and sometimes it projected from the surface. When happily prepared the specimens were even beautiful. The central nucleus was dyed of the finest red; and on the delicate outline of the cell-wall hung the red parietal macula, offering a not altogether fanciful resemblance to the astronomical figures representing the moon coursing in its orbit round the earth.

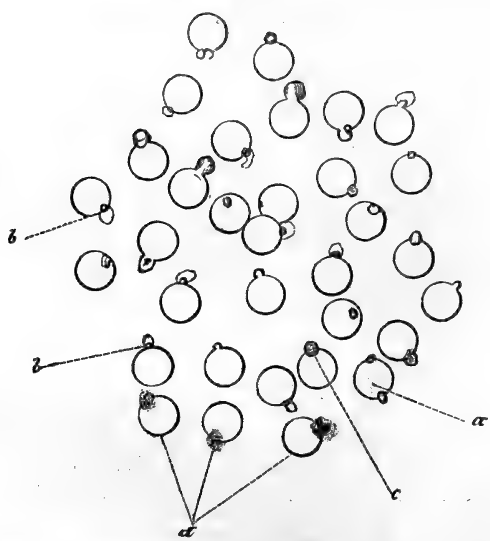
At this stage of the inquiry it was conceived that an improved demonstration might be obtained by fixing the dye with a mordant, and then subjecting the corpuscles to a lavatory process, so as to get rid of the floating granules which so much interfered with the view. For this purpose a solution of tannin (which is one of the mordants

* In order to bring out the best results, it was found requisite to modify the strength and quantity of the solution for the different kinds of blood. This doubtless depended upon the varying densities of the liquor sanguinis and cell-contents in different animals.

for magenta used in the arts) was employed; and some advantage was found therein. When a solution of tannin, of 3 grains to the ounce of water, was added to blood that had already been dyed with magenta, it was found that the parietal maculæ had their colour intensified, and that they became more conspicuous objects. The investigation was, however, not pushed any further in this direction, for it was found that tannin alone produced an even more remarkable effect than magenta. To this effect I now desire to draw particular attention.

When a solution of tannin, of the strength of 3 grains to the ounce, was applied to human blood, or to that of the horse, ox, sheep, pig, or cat, the blood immediately became turbid; and when a drop was placed under the microscope the corpuscles were found greatly changed, as represented in fig. 2.

Fig. 2.



Human blood after the action of tannin.
a. Double pullulation.
b, b. Hooded modification.
c. Outline of the cell seen continuously through the pullulation.
d. Bursting of the pullulations independently of destruction of the cell.

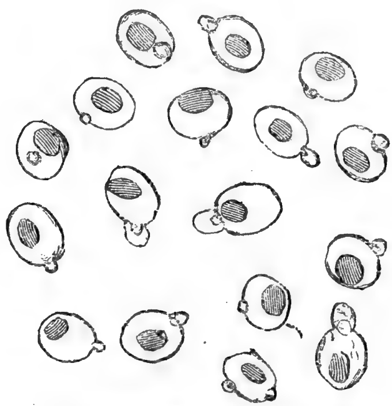
Each corpuscle appeared to have thrown out a bright, highly refractive bud or projection on its surface. The projections were

usually about a fourth part of the size of the corpuscle on which they were fixed; but they varied considerably. Some were only minute bright specks in the cell-wall; others were half or even two-thirds as large as the corpuscle itself. Very rarely (in mammalian blood) two such projections were seen; and as rarely a corpuscle was devoid of any.

The projections were commonly round or dome-shaped, bordered with a deeply refractive outline. Frequently a minute, apparently vesicular body could be seen within this outline, and then the projection presented a curiously hooded aspect (fig. 2, *b*, *b*). In a urinary deposit from a lad twelve years of age, containing pus and blood, nearly every blood-disk presented the hooded appearance after the addition of tannin.

The blood of the fowl, turkey, duck, and goose showed exactly analogous phenomena with the same reagent (see fig. 3).

Fig. 3.



Blood of fowl after the action of tannin.

The projection had sometimes the hooded character with a vesicular body within; sometimes the projection offered no such distinction of parts. It was situated indifferently on any part of the periphery. In all the birds examined a second projection was as rare as in mammalia.

Of fish, the dace, minnow, and carp were examined. The tannin-solution produced a similar effect to that seen in the fowl—with this difference, that a large number of corpuscles had two projections

instead of one. In the carp double and single projections occurred in about equal proportions; in the minnow double projections were all but universal. The second projection was situated sometimes at the opposite pole of the disk, sometimes in near proximity to its fellow, or at any point between. Very rarely, a third projection was seen in the dace.

In the blood of the frog there was a strong tendency to the indefinite multiplication of the projections; two, three, four, and even five would rise in succession on the surface of the disk. It appeared, too, not unfrequently as if the entire outer membrane of the cell was detached from the parts beneath and raised into eight or ten unequal elevations, giving the outline of the disk an irregularly crenate appearance*.

The formation of these singular projections, or *pullulations*, on the blood-disks could be watched without difficulty by placing a drop of the tannin-solution beneath the covering glass, and permitting a little blood to insinuate itself into the solution under the microscope. As the blood flowed in and mingled with the tannin, the corpuscles were observed gradually to enlarge, and then *suddenly*, without previous warning, to shoot out the projection. As a rule, it does not appear to grow afterwards. The phenomenon was finely seen in the defibrinated blood of the fowl after it had been allowed to sink through a column of syrup (sp. gr. 1025) in a test-tube. Fowl's blood washed in this way was mixed, in a little glass, with about five times its volume of the tannin-solution, and a drop immediately put under the microscope. The disks first enlarge and become rounded, and the central nucleus comes into view. In thirty or forty seconds the pullulation begins; and each corpuscle, with instantaneous rapidity and without previous sign, throws out its bud. The disk itself suffers not the least disturbance during this act; it preserves its symmetry unchanged, as if it had no concern, beyond that of proximity, with the sudden apparition on its surface.

No visible rupture of the cell-wall took place. The circular out-

* There is a certain adjustment of the proportions between the tannin-solution and blood required to bring out the effects described in this paper; but the proper proportions are, practically, very easily found after a few trials for each kind of blood. In mammalian blood, one drop of blood mixed in a conical glass with four or five of the solution generally answered perfectly. Any considerable excess of blood or solution above these proportions caused destruction of the corpuscles.

line of the latter could sometimes be distinctly followed through the projection (fig. 2, *c*); and as the altered corpuscles revolved in the field of the microscope, the projection appeared to be organically connected with it, but to form no part of its cavity. In the human blood-disks the application of acetic acid, soon after the tannin, caused, on two occasions, the pullulations gradually to subside, and finally to disappear, and then the disk resumed its original circular outline. I failed to produce this "redux" effect in the fowl; and did not always succeed with human blood, probably because the change produced by the tannin had gone too far.

The modification noted under the term "hooded" appearance depends, I believe, upon secondary conditions of concentration and quantity of the tannin-solution in comparison to the blood. When the hooded condition has been watched in the act of occurrence, it was noticed that the outer hood was shot out *first*, and instantly after this the highly refractive vesicular body made its appearance within. The contents of the hood (excluding the vesicular body) appeared usually to refract the light like the body of the cell, or even less strongly; sometimes, however, more strongly.

The effect of tannin did not cease with the production of the elevations just described. At first the cells and their projections preserved their elasticity; but after a while (a few minutes, or several hours, according to the proportions used) the corpuscles and their projections became solid, and they could be cracked by pressure under the microscope like starch-granules. More slowly the same destruction overtook the corpuscles spontaneously; and this significant fact was observed in the course of it:—sometimes the cell ruptured before the projection, the latter persisting as a bright granule amid or near the débris; sometimes, on the other hand (in the horse), the projection broke up before the disk to which it was attached. In this latter case, the hood (if there were any) broke up first into a scattered nebula of granular appearance, and then the nucleolus-like body within burst into three or four bright fragments (fig. 2, *d*). This train of events seemed to remove all doubt as to the complete isolation of the projection from the cavity of the disk. Last of all, the disk itself began to crack; in a few days all my specimens were thus destroyed.

In addition to magenta and tannin the following substances were

tried, but they did not produce phenomena in the least analogous with the foregoing:—gallic acid, ferrocyanide of potassium, santonine, sulphate of magnesia, alcohol and water, solutions of carbolic acid, of atropine, morphia, iodine, sugar, gum, glycerine, and infusion of coffee.

A solution of picric acid produced the appearance of a parietal particle like that brought out by magenta, except that it was not coloured. An exactly similar appearance was on one occasion observed in blood-corpuscles in the urine of a patient with acute Bright's disease.

When magenta was applied after the process of pullulation had taken place, the projections were found to take the dye strongly, and especially the vesicular body within the hood. By this proceeding beautiful and remarkable objects for microscopical examination were obtained. In the fowl, dace, and minnow the projection was tinted earlier than the central nucleus—probably from its more ready access to the pigment. The explanation of these appearances presents great difficulties, and in the present state of the inquiry can only be offered provisionally.

The effect of the magenta-solution is not merely to tint, and so render visible a very minute body. In watching the effect of magenta, the first thing observed is that the natural yellowish colour of the disk is discharged, and that a faint rose tint is assumed in its stead. The disks at the same time lose their biconcave shape. The parietal macula is rather "brought out" than revealed, and the action of the solution is, to a very great extent, of a simply osmotic character.

The action of the tannin-solution is likewise in the main of a similar nature, but modified in some very peculiar manner. Its first operation is to cause the corpuscle to enlarge by imbibition, and this goes on progressively until at length the cell is destroyed. If the solution be strong, this destruction supervenes at once. The tannin also unites with the cell-contents and coagulates them, imparting to the corpuscle, finally, a solid consistence. The conditions of the imbibition are disturbed by the previous application of magenta; for no pullulation, or at most only traces, occurs when the corpuscles are treated *first* with magenta and *then* with tannin.

The bearing of these observations on the current views respecting

the structure of the vertebrate blood-disk is important. They seem to warrant the inferences drawn in the two following paragraphs:—

1. The exact identity of the appearances produced in the blood-disks of the ovipara with those observed in the mammalian corpuscles lends strong support to the view that these corpuscles are homologous as wholes; and that the mammalian blood-disk is not the homologue of the nucleus of the coloured corpuscle of the ovipara, as was conceived by Mr. Wharton Jones.

2. The observations likewise lead to the belief that the envelope of the vertebrate blood-disk is a duplicate membrane; in other words, that within the outer covering there exists an interior vesicle which encloses the coloured contents, and, in the ovipara, the nucleus.

Dr. Hensen* of Kiel had already in 1861 convinced himself, from wholly different observations, that the blood-corpuscles of the frog possess such a structure. On this view the blood-corpuscle is anatomically analogous to a vegetable cell, and the inner vesicle corresponds to the primordial utricle.

The present observations indicate, by direct proof, a duplication at only one or, at most, two points in the blood-disks of mammals and birds. Nevertheless certain appearances, occasionally observed, favour the notion of a complete duplication (fig. 1, *b*).

The admission of this hypothesis, however, scarcely removes the difficulties sufficiently to permit a tenable explanation to be offered of the appearances described in this paper. Yet, as it may prove suggestive to some other inquirer, I will not suppress what appears to me the explanation least open to objections. It might be conceived that the cells enlarged by imbibition, until at length the less distensible inner membrane gave way, and permitted an extravasation of a portion of the cell-contents between it and the outer membrane, its own continuity being in the meanwhile instantaneously restored by cohesion of the ruptured borders†. In this way a microscopic

* Zeitschrift für Wissensch., Zoologie, Band xi. p. 263.

† In the same manner as a soap-bubble when bisected, instead of collapsing, forms, in virtue of the adhesiveness and fluidity of its envelope, two new and perfect bubbles. That the cell-wall of the blood-disk possesses some such endowment seems highly probable. I have on several occasions witnessed, after adding magenta, the total extrusion of the nucleus, both in the frog and in the newt, without the least collapse of the corpuscles.

drop of the cell-contents would be lodged between the outer and inner membrane, and completely severed from the general cell-cavity. The peculiar modification spoken of as the "hooded" appearance might be due to imbibition of fluid between this microscopic drop and the outer envelope.

The chief difficulties in the way of this explanation arise out of the differences of nature which appear to exist between the projection and the general cell-contents of which it is supposed to be a detached portion. The projection refracts light much more highly than the cell-contents; it also is deeply dyed by magenta, whereas the cell-contents are only very feebly so.

In conclusion, it may be added that important advantages may be expected from the use of magenta in histological researches. Its inert chemical character, its prodigious tinting power, and its solubility in water eminently fit it for such a purpose. It will probably prove of especial use in bringing into sight objects which otherwise evade the visual organs from their absolute colourlessness and transparency, and from the equality of their refraction with the medium in which they exist.

II. "On Quinidine, and some Double Tartrates of the Organic Bases." By JOHN STENHOUSE, LL.D., F.R.S. Received February 23, 1863.

Quinidine, as is well known, was first observed by Henry and Delondre, and likewise by Sertuerner, in what is called the "quinidine" of commerce, which consists chiefly of a mixture of quinidine, quinine, and resinous matters obtained from the mother-liquors of the sulphate-of-quinine manufacture. Van Heijningen, however, was the first person who succeeded in separating quinidine, which he called β-quinine, from this mixture and in obtaining it in a pure and crystalline state. He likewise ascertained that quinidine was isomeric with quinine. Its action on polarized light was studied by Pasteur, who observed that its solution in absolute alcohol produces deviation to the right, while a similar solution of quinine produces rotation to the left (Buchardat).

As comparatively few of the salts of quinidine have hitherto been analysed, and those chiefly by Van Heijningen, I was induced to

prepare a few more of them, and likewise to examine the action of iodide of ethyl upon the alkaloid itself. I am indebted for the quinidine which I employed for this purpose to Mr. John Elliott Howard of Stratford. It was perfectly white, and consisted of large, well-defined crystals. Before using it, however, I recrystallized it out of alcohol. The quinidine gave a green colour with chlorine-water and ammonia, and the solutions of its salts all exhibited a fluorescent power almost equal to that of quinine. A solution of its sulphate likewise yielded Herapath's so-called sulphate of iodo-quinidine, crystallizing in long quadrilateral prisms possessing a deep garnet-red colour and the other well-known characters of that salt. ·343 gramme of pure quinidine, dried at 100°C., gave ·9315 gramme carbonic acid and ·238 gramme water.

This corresponds to 74·04 per cent. of carbon and 7·71 per cent. of hydrogen.

The formula $C_{40}\,H_{24}\,N_2\,O_4$ requires 74·08 and 7·4 per cent.

Quinidine Platinum-salt, $C_{40}\,H_{24}\,N_2\,O_4$, 2H Cl + Pt Cl_2.—On the addition of bichloride of platinum to a solution of hydrochlorate of quinidine, an immediate precipitate takes place if the solution be cold and concentrated; but if dilute or hot solutions be employed, it only crystallizes out after some time. It is very insoluble both in cold and in hot water, but crystallizes from boiling dilute hydrochloric acid in brilliant but irregularly-formed crystals, which decomposed when heated to about 200° C., evolving a peculiar aromatic odour somewhat resembling that of hawthorn. ·7485 gramme of this salt, when dried at 150° C., on ignition gave ·1995 gramme of metallic platinum. This corresponds to 26·66 per cent.

The formula $C_{40}\,H_{24}\,N_2\,O_4$, 2H Cl + 2Pt Cl_2 requires 26·81 per cent.

Both quinidine and its platinum-salt were first analysed many years ago by Baron Liebig; but as he employed amorphous quinoidine for this purpose, Gerhardt supposes that he operated on a mixture of the three isomeric alkaloids, quinidine, quinicine, and quinine.

Quinidine Gold-salt, $C_{40}\,H_{24}\,N_2\,O_4$, 2H Cl + 2Au Cl_3.—The gold-salt is prepared by dissolving quinidine in hydrochloric acid and adding excess of terchloride of gold, when it falls as a bright yellow powder.

This salt appears to be decomposed by boiling with water, turning

brown and apparently fusing. It is therefore necessary to precipitate it in the cold. Well washed and dried *in vacuo*, on heating to 100° it lost about ·5 per cent., probably hygroscopic water; when further heated to 115°, it fused, turned brown, and began to decompose. ·3575 gramme of gold-salt dried at 100° gave ·14 gramme of metallic gold. This is equivalent to 39·15 per cent. The formula $C_{40} H_{24} N_2 O_4$, $2H Cl + 2Au Cl_3$ requires 39·2 per cent.

Nitrate-of-Silver Salt of Quinidine, $C_{40} H_{24} N_2 O_4$, $Ag NO_6$.—This was prepared by adding nitrate-of-silver solution to an alcoholic solution of quinidine; the mixture became semisolid from precipitation of the above salt in the form of minute needles. These were thrown on a filter and thoroughly washed with cold water, in which they are scarcely at all soluble. The compound was then recrystallized from hot water slightly acidulated with nitric acid; after filtration it separates, on cooling, in beautiful silky needles, which, on being freed from the mother-liquor and dried between bibulous paper, have almost the lustre of metallic silver, such as is produced on igniting its organic salts. It partially decomposes every time it is recrystallized, but much more so when spirit is substituted for water, the solution becoming black from reduced silver.

·401 gramme of the salt, dried at 100°, gave ·115 chloride of silver, corresponding to ·08656 gramme of metallic silver. This gives 21·59 per cent. According to the formula $C_{40} H_{24} N_2 O_4$, $Ag NO_6$, theory requires 21·86 per cent.

Mercuro-hydrochlorate of Quinidine, $C_{40} H_{24} N_2 O_4$, $2H Cl + Hg Cl$.—On mixing solutions of hydrochlorate of quinidine and chloride of mercury, the above salt is precipitated as a white powder. It is slightly soluble in cold water, much more so in hot, especially when acidulated with hydrochloric acid; but it cannot be conveniently crystallized from this menstruum, as it sometimes separates in resinous masses.

By far the best solvent from which to crystallize is boiling alcohol, in which it is readily soluble, separating in nacreous scales as the solution cools. It fuses at 100° under water, but not when dry.

·626 gramme of the above salt, dried at 100°, yields ·507 chloride of silver, which gives of chlorine 20·04 per cent. The formula $C_{40} H_{24} N_2 O_4$, $2H Cl + Hg Cl$ requires 20·00 per cent.

Zinco-hydrochlorate of Quinidine, $C_{40} H_{24} N_2 O_4$, $2H Cl + 2 Zn Cl$.—

A moderately strong solution of chloride of zinc, containing a slight excess of hydrochloric acid, is poured into an alcoholic solution of quinidine, when the double salt precipitates as a granular powder. It is very slightly soluble either in hot or in cold water, but dissolves readily in dilute hydrochloric acid and in spirit of 50 per cent., from which latter it crystallizes in a form very similar to that variety of carbonate of lime known as "dog-tooth" spar.

The specimen analysed was crystallized from dilute hydrochloric acid and dried at 100°. ·447 gramme gave ·4825 gramme chloride of silver, equal to ·1193 of chlorine. This corresponds to 26·7 per cent. of chlorine. The formula $C_{40} H_{24} N_2 O_4$, $2H Cl + 2 Zn Cl$ requires 26·65 per cent.

Basic Chloride-of-Zinc Salt, $C_{40} H_{24} N_2 O_4$, $H Cl + Zn Cl$.—The neutral salt described above appears to lose a portion of its hydrochloric acid and chloride of zinc after repeated crystallizations. A solution of the salt so treated deposits, upon slow evaporation, crystals of considerable size, consisting of hexagonal plates and prisms.

After drying at 100°, these were submitted to analysis; the zinc and quinidine were precipitated together by carbonate of soda, and after washing well with water, the quinidine was removed by boiling alcohol; the residuary oxide of zinc was then again washed with water, and ignited in the usual way.

·7335 gramme of the salt yielded ·0680 gramme of oxide of zinc, equivalent to ·05456 of metallic zinc, or 7·44 per cent.

The formula $C_{40} H_{24} N_2 O_4$, $2 H Cl + 2 Zn Cl$ requires 12·2 per cent.

The formula $C_{40} H_{24} N_2 O_4$, $H Cl + Zn Cl$ requires 7·58 per cent.

Oxalate of Quinidine, $C_{40} H_{24} N_2 O_4$, $H C_2 O_4$, HO.—The oxalate of quinidine is formed by exactly neutralizing oxalic acid by quinidine. It consists of very small brittle crystals, which are almost insoluble in cold, but comparatively soluble in hot water, from which it crystallizes again on cooling. After being purified by two recrystallizations and dried at 100°, it was submitted to analysis.

·206 gramme gave ·502 carbonic acid and ·133 water; this corresponds to 66·45 per cent. of carbon and 7·17 of hydrogen.

The formula $C_{40} H_{24} N_2 O_4$, $HC_2 O_4 + HO$ requires 66·67 per cent. carbon and 6·88 per cent. hydrogen.

This is evidently, therefore, the neutral oxalate, and differs wholly

in appearance and properties from the acid oxalate, $C_{40}H_{24}N_2O_4$, $H_2C_4O_8+2Aq$, obtained by Van Heijningen.

Picrate of Quinidine.—Quinidine dissolves in a boiling and not too concentrated solution of picric acid; but it separates on cooling, not in crystals, but as a resinous mass. When dissolved in hot alcohol, it is deposited by evaporation of the solution in resinous nodules, thus very closely resembling the corresponding salt of quinine.

Action of Iodide of Ethyl on Quinidine.—Iodide of ethyl attacks quinidine very readily, so that if the two substances (the former being in excess) be heated together in a flask furnished with a perforated cork and long tube to condense the iodide of ethyl, combination quickly ensues, and in half an hour the reaction is complete. Water is then added and the excess of iodide distilled off, the residue dissolved in dilute alcohol, filtered, and allowed to crystallize. On cooling, the solution becomes semisolid, from separation of the iodide of ethyl-quinidine in the form of long silky needles. These are well washed with cold water, in which they are almost insoluble, and then recrystallized from boiling dilute spirit, washed, dried at 100°, and submitted to analysis.

·5600 gramme yielded ·2735 gramme iodide of silver, equivalent to ·1478 of iodide, or 26·39 per cent.

The formula $\left.\begin{matrix} C_{40}H_{24}N_2O_4 \\ C_4H_5 \end{matrix}\right\} I$ requires 26·46 per cent. of iodine.

Platinum-salt of Ethyl-quinidine,

$$C_{40}H_{24}N_2O_4\left\{\begin{matrix} C_4H_5Cl \\ HCl \end{matrix}\right\}2PtCl_2.$$

On treating the foregoing iodide with chloride of silver the chlorine replaces the iodine, and we obtain chloride of ethyl-quinidine in solution and iodide of silver. The latter is separated by filtration, and the solution, now free from iodine, is precipitated by the addition of bichloride of platinum. The platinum-salt separates as a pale-yellow powder, almost insoluble either in hot or in cold water, and but sparingly soluble in boiling dilute hydrochloric acid, from which it separates almost completely on cooling.

The specimen analysed was simply washed with water and dried at 100°. ·537 gramme gave ·137 metallic platinum, corresponding to 25·61 per cent.

The formula $C_{40} H_{24} N_2 O_4 {\genfrac{}{}{0pt}{}{C_4 H_5 Cl}{H Cl}}$ 2 Pt Cl_2 requires 25·86 per cent.

Hydrated Oxide of Ethyl-quinidine.—On treating the iodide of ethyl-quinidine with oxide of silver in slight excess, a solution of the hydrated oxide is obtained. It is very bitter to the taste, readily attracting carbonic acid from the air, and is of course highly alkaline to test-paper. On evaporation it does not crystallize.

Action of Iodide of Ethyl on Hydrated Oxide of Ethyl-quinidine.—When iodide of ethyl and a strong solution of hydrated oxide of ethyl-quinidine are made to react upon each other in a sealed tube at the temperature of 100°, a mass of crystals are produced after digestion for about an hour. These were extracted from the tube, washed with a little water, dried between folds of filtering-paper, and recrystallized from dilute alcohol. On analysis, they proved to be nothing more than reproduced iodide of ethyl-quinidine.

·4435 gramme gives ·2165 of iodide of silver, corresponding to ·117 of iodine, or 26·38 per cent.

The formula $\left.\begin{matrix} C_{40} H_{24} N_2 O_4 \\ C_4 H_5 \end{matrix}\right\}$ I requires 26·46 per cent.

From these experiments it appears that quinidine contains no replaceable hydrogen, and in this respect it agrees with quinine and cinchonine, the alkaloids with which it is associated.

Double Tartrate of Quinidine and Antimony,

$$\left.\begin{matrix} C_{40} H_{24} N_2 O_4 H \\ Sb O_2 \end{matrix}\right\} C_8 H_4 O_{12}.$$

When an excess of quinidine in powder is added to a cold saturated solution of tartar-emetic, and heat applied so soon as the mixture begins to boil, the quinidine pretty rapidly dissolves, whilst at the same time a quantity of oxide of antimony is precipitated. The solution is filtered whilst boiling, and on cooling the double tartrate of quinidine and antimony is deposited in very long fine silky needles, often more than an inch in length. Any excess of quinidine which has been employed, together with the oxide of antimony which has been precipitated, remains upon the filter. The double tartrate is but slightly soluble in cold water, but dissolves easily in hot, from which it may readily be crystallized. It is also very soluble in boiling spirit of wine, from which it is deposited, almost completely on cooling, in tufts of slender needles.

The double tartrate of quinidine and antimony was very carefully examined for potassa by precipitating the antimony by means of sulphuretted hydrogen; it then left no perceptible residue on ignition, and a drop of bichloride of platinum gave no precipitate. It therefore could not contain potassa.

Analysis.—Dried *in vacuo* over sulphuric acid, it retains, apparently uncombined, from ·5 to 1 per cent. of water, which it loses at the temperature of 100°.

The salt dried *in vacuo* gave the following results:—

I. 0·3850 gramme gave ·6750 gramme carbonic acid and ·1780 gramme water.

II. ·2375 gramme gave ·4175 gramme carbonic acid and ·1270 gramme water.

III. ·5380 gramme gave ·1398 gramme of antimonious acid, $Sb\,O_4$.

The compounds, after two recrystallizations and drying at 100°, gave the following results:—

IV. ·5815 gramme gave 1·0100 gramme carbonic acid and ·2550 gramme water.

V. ·5110 gramme gave ·893 gramme carbonic acid and ·224 gramme water.

VI. ·7195 gramme substance gave ·1949 gramme tersulphide of antimony. This was dried *in vacuo* and contained 1·3 per cent. of water; therefore ·7195 gramme is equal to ·7101 dried at 100°.

	Theory.		I.	II.	III.	IV.	V.
C_{48} =	288 ..	47·28	47·8	47·94	——	47·36	47·66
H_{29} =	29 ..	4·76	5·13	5·94	——	4·87	4·87
N_2 =	28 ..	4·596	——	——	——	——	——
O_{18} =	144 ..	23·63	——	——	——	——	——
Sb =	120·3 ..	19·74	——	——	20·53	——	19·61
	609·3	100·006					

In order to ascertain that the residue upon the filter, in addition to undissolved quinidine, contained precipitated teroxide of antimony, it was washed with hot alcohol and dissolved in strong hydrochloric acid. This solution gave the characteristic reactions of antimony with water and sulphuretted hydrogen. The mother-liquor, which was quite neutral to test-paper, and from which as much as

possible of the quinidine-salt had been separated by concentration, was treated by sulphuretted hydrogen to separate the antimony, and was tested for potassa and for tartaric acid, both of which it was found to contain.

The reaction may therefore be represented by the following equation:—

$$2\left.\begin{matrix}K\\ Sb\,O_2\end{matrix}\right\}C_8\,H_4\,O_{12}+C_{40}\,H_{24}\,N_2\,O_4+HO=$$

$$\left.\begin{matrix}C_{40}\,H_{24}\,N_2\,O_4\,H\\ Sb\,O_2\end{matrix}\right\}C_8\,H_4\,O_{12}+\left.\begin{matrix}K\\ K\end{matrix}\right\}C_8\,H_4\,O_{12}+Sb\,O_3.$$

In order to confirm this view of the composition of the salt, some acid tartrate of quinidine was prepared by taking a solution of tartaric acid, dividing it equally into two portions, neutralizing the one with quinidine, and then adding the other. This solution was then boiled for some hours with excess of freshly-precipitated oxide of antimony, filtered, and allowed to cool, when the same beautiful salt made its appearance, identical in all its properties with the compound formed by boiling tartar-emetic with quinidine.

It will be obvious from these results that tartar-emetic, when boiled with quinidine, undergoes a somewhat singular and unexpected decomposition, neutral tartrate of potassa and the double tartrate of quinidine and antimony being formed, while one-half of the antimony of the original salt is precipitated as oxide. From this and the subsequently to be described double tartrates, the tersulphide of antimony which is precipitated has a light yellow or pale orange colour, and, after being long washed with boiling water, still contains some of the base, probably in the state of a salt. This portion of the alkaloid may, however, be abstracted from the tersulphide of antimony by digesting it with hot alcohol.

In order to confirm the fact that the compound contained quinidine and not an altered base, as it may be observed from the analyses given above that the carbon in the double tartrates is somewhat too high, the antimony was removed from a portion of the salt by means of sulphuretted hydrogen, the base precipitated by ammonia, well washed with water, and crystallized out of spirit; it was then obtained with the usual crystalline form assumed by quinidine, viz. 4-sided prisms, and gave the characteristic reaction with chlorine-water and ammonia. A platinum-salt also was prepared in the usual way,

which, recrystallized from dilute hydrochloric acid, gave the following analytical result:—

·411 gramme gave ·1075 gramme metallic platinum; this corresponds to 26·16 per cent. $C_{40} H_{24} N_2 O_4, 2 H Cl + 2 Pt Cl_2$ requires 26·81 per cent.

The *Tartrate of Strychnine and Antimony*,

$$C_{42} H_{22} N_2 O_4 \left.\begin{matrix} H \\ Sb\,O_2 \end{matrix}\right\} C_8 H_4 O_{12},$$

was likewise prepared by adding strychnine to a boiling solution of tartar-emetic. The same solution of the alkaloid and precipitation of oxide of antimony were observed as in the case of quinidine. On cooling, the double tartrate was deposited in very brittle needles, much less soluble in water than the corresponding quinidine-salt. It occasionally crystallizes in leafy plates.

The following results were obtained in the analysis of the salt dried at 100°:—

I. ·3365 gramme gave ·6005 gramme carbonic acid and ·133 gramme water.

II. ·6000 gramme gave ·148 gramme $Sb O_4$.

III. ·5495 gramme gave ·1375 gramme $Sb O_4$.

	Theory.		I.	II.	III.
C_{50} =	300	.. 48·44	48·7	——	——
H_{27} =	27	.. 4·36	4·39	——	——
N_2 =	28	.. 4·52	——	——	——
O_{18} =	144	.. 23·26	——	——	——
Sb =	120·3	.. 19·42	——	19·47	19·76
		100·00			

This corresponds with the formula

$$\left.\begin{matrix} C_{42} H_{22} N_2 O_4\, H \\ Sb\,O_2 \end{matrix}\right\} C_8 H_4 O_{12}.$$

Some of this salt was also decomposed by sulphuretted hydrogen and treated in the manner described in the case of the quinidine. It was then obtained in quadrilateral prisms with pyramidal summits, and gave the usual reactions with sulphuric acid and bichromate of potassa.

A platinum-salt of the recovered alkaloid was also prepared and

analysed. Dried at 100°, ·630 gramme gave ·1150 gramme of metallic platinum. This is equivalent to 18·26 per cent.

The formula $C_{42} H_{22} N_2 O_4$, H Cl, Pt Cl_2 requires 18·27 per cent.

Tartrate of Brucine and Antimony,

$$\left.\begin{matrix} C_{46} H_{26} N_2 O_8 H \\ Sb O_2 \end{matrix}\right\} C_8 H_4 O_{12}.$$

The salt was prepared in a precisely similar manner. It formed short and excessively brittle crystals. Analysis seems to assign to this salt the same constitution as those containing quinidine and strychnine, as may be seen by the following results:—

I. ·284 gramme brucine compound gave ·5000 gramme carbonic acid and ·1185 gramme water.

II. ·4845 gramme yielded ·1102 gramme of Sb O_4.

III. ·4800 gramme yielded ·1078 gramme of Sb O_4.

IV. 3120 gramme yielded ·07074 gramme of Sb. O_4.

	Theory.		I.	II.	III.	IV.
C_{54} =	324	.. 47·68	48·03	—	—	—
H_{31} =	31	.. 4·563	4·64	—	—	—
N_2 =	28	.. 4·122	—	—	—	—
O_{22} =	176	.. 25·90	—	—	—	—
Sb =	120·3	.. 17·7	—	17·97	17·75	17·91
	679·3	99·965				

This agrees with the formula $\left.\begin{matrix} C_{46} H_{26} N_2 O_8 H \\ Sb O_2 \end{matrix}\right\} C_8 H_4 O_{12}$.

Tartrate of Berberine and Antimony,

$$\left.C_{40} H_{17} NO_8 \begin{matrix} H \\ Sb O_2 \end{matrix}\right\} C_8 H_4 O_{12}.$$

This salt was prepared by a process analogous to that employed in the cases already described. It forms fibrous aggregations resembling Wavellite. It can be recrystallized from spirit, and, with less security from decomposition, out of water. As first formed, however, it may readily be obtained perfectly pure by copious washing with cold distilled water, in which it dissolves very sparingly.

Dried *in vacuo* over sulphuric acid,

I. ·7200 gramme gave 1·2265 gramme carbonic acid and ·2420 gramme water.

II. ·3480 gramme substance gave ·0873 gramme antimonious acid.

These numbers correspond to the formula

$$C_{48}H_{22}NSbO_{22}=\left.\begin{matrix}C_{40}H_{17}NO_8H\\SbO_2\end{matrix}\right\}C_8H_4O_{12}.$$

		Theory.		I.	II.
C_{48}	=	288	.. 46·43	46·46	——
H_{22}	=	22	.. 3·54	3·76	——
N	=	14	.. 2·25	——	——
Sb	=	120·3	.. 19·39	——	19·85
O_{22}	=	176	.. 28·37	——	——
		620·3	99·98		

It is very remarkable that corresponding crystalline double tartrates are not obtained by boiling tartar-emetic with many of the other organic bases.

The following have been tried, but unsuccessfully:—

Quinine. Aniline.
Cinchonine. Theine.
Cinchonidine. Piperine.
Furfurine.

March 26, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "Researches into the Chemical Constitution of Narcotine, and of its Products of Decomposition."—Part I. By A. MATTHIESSEN, F.R.S., and GEORGE CAREY FOSTER, B.A. Received February 26, 1863.

(Abstract.)

An abstract of a considerable portion of this paper has already appeared in the form of a preliminary notice* of the first results obtained. The most important additional facts now communicated are the following:—

The analysis of five additional specimens of narcotine has confirmed the accuracy of the formula $\mathrm{C}^{22}H^{23}N\mathrm{O}^7$, which the authors

* Proc. Roy. Soc. vol. xi. p. 55.

adopted for this base in their previous notice. The mean results of all their analyses (ten determinations of carbon and hydrogen and four determinations of nitrogen) give for the composition of narcotine,—

Carbon	63·78
Hydrogen	5·76
Nitrogen	3·32
Oxygen	27·14
	100·00

The formula $Є^{22} H^{23} NΘ^{7}$ corresponds to

Carbon	63·92
Hydrogen	5·57
Nitrogen	3·39
Oxygen	27·12
	100·00

The authors had previously stated that meconin, opianic acid, and hemipinic acid were all decomposed by boiling concentrated hydriodic acid, with evolution of iodide of methyl. They now find that hydrochloric acid acts similarly on these bodies, evolving with each of them chloride of methyl. They suggest the name *hypogallic acid* for the acid $Є^{7} H^{6} Θ^{4}$, formed by the action of hydriodic acid on hemipinic acid*, in order to recall the fact of its containing one atom of oxygen less than gallic acid, $Є^{7} H^{6} Θ^{5}$. They obtained the same product by the prolonged action of hydrochloric acid on hemipinic acid; but by a shorter action an intermediate product was obtained, which, according to a single analysis, appears to contain $Є^{8} H^{8} Θ^{4}$.

	Calculated.		Found.
$Є^{8}$	96	57·14	56·65
H^{8}	8	4·76	5·17
$Θ^{4}$	64	38·10	—
$Є^{8} H^{8} Θ^{4}$	168	100·00	

This substance is an acid of much greater stability than hypogallic acid; it crystallizes in long, thin, transparent prisms, which are anhydrous, and are almost insoluble in cold water, slightly soluble in boiling water, but dissolve more readily in alcohol and ether. At

* Proc. Roy. Soc. vol. xi. p. 58.

about 200° C. this acid sublimes unchanged, and it may be heated to 245° without melting or becoming coloured; it dissolves in strong sulphuric acid, and is precipitated unaltered on addition of water. It gives no coloration with sesquichloride of iron; with nitrate of silver it gives a white precipitate which slowly blackens on boiling. Its formation from hemipinic acid may be expressed by the equation

$$\underset{\text{Hemipinic acid.}}{C^{10}\,H^{10}\,O^{6}} + H\,Cl = C\,O^{2} + C\,H^{3}\,Cl + C^{8}\,H^{8}\,O^{4}.$$

This substance has the composition of methyl-hypogallic acid; but its great stability, as compared with hypogallic acid, makes it appear improbable that such is its true constitution.

Nascent hydrogen evolved from sodium-amalgam and water, or from zinc and dilute sulphuric acid, converts opianic acid into meconin,

$$\underset{\text{Opianic acid.}}{C^{10}\,H^{10}\,O^{5}} + H^{2} = \underset{\text{Meconin.}}{C^{10}\,H^{10}\,O^{4}} + H^{2}\,O.$$

Hemipinic acid is unaltered by nascent hydrogen.

Aqueous hydrochloric acid decomposes cotarnine at 140° C. into chloride of methyl, and a substance which the authors call *hydrochlorate of cotarnamic acid* and represent by the formula $C^{11}\,H^{13}\,NO^{4}$, HCl, supposing it to be formed according to the following equation:—

$$\underset{\text{Cotarnine.}}{C^{12}\,H^{13}\,NO^{3}} + H^{2}\,O + 2HCl = \underset{\text{Hydrochlorate of cotarnamic acid.}}{C^{11}\,H^{13}\,NO^{4}\,HCl} + CH^{3}\,Cl.$$

The formula proposed does not agree perfectly with the results of several analyses, which are accordant among themselves, and is given as provisional only.

	Calculated.		Found (mean).
C^{11}	132	50·87	49·98
H^{14}	14	5·40	5·70
N	14	5·40	5·71
O^{4}	64	24·65	24·61
Cl	35·5	13·68	14·00
$C^{11}\,H^{13}\,NO^{4}$, HCl	259·5	100·00	100·00

This body crystallizes in small silky needles of a pale yellow colour; it is partially decomposed, losing hydrochloric acid, when dissolved in pure water, but dissolves unchanged in water containing a trace

of free hydrochloric acid. Alkalies, or alkaline carbonates or sulphites, throw down from the aqueous or acid solution an orange-coloured granular precipitate (cotarnamic acid?) free from hydrochloric acid; this precipitate dissolves in excess of alkali, giving a solution which quickly becomes dark brown by exposure to air; with hydrochloric acid, it regenerates the original crystalline compound; it dissolves slightly in water, with a bright orange colour that is visible even in very dilute solutions. The slightly acidulated solution of the hydrochlorate is pale yellow when pure, but acquires a deep-green colour in the air. A few drops of nitric acid added to a hot solution cause it to appear of an opake crimson colour by reflected light, though the liquid is still transparent when examined by transmitted light. Nitrate of silver added in quantity more than sufficient to precipitate the chlorine is quickly reduced. The compound acquires a fine crimson colour when evaporated nearly to dryness on the water-bath, with a slight excess of dilute sulphuric acid.

Hydriodic and diluted sulphuric acids decompose cotarnine at a high temperature in the same way as hydrochloric acid. The products so formed have not been isolated, but were converted into hydrochlorate of cotarnamic acid.

The authors conclude by offering some suggestions towards the interpretation of the results they have obtained. Only the most important points discussed by them will be briefly indicated here.

They suppose that when opianic acid is converted into meconin by the action of nascent hydrogen, or into meconin and hemipinic acid by the action of potash*, a substance containing $C^{10}H^{12}O^5$ may possibly be the immediate product of the reaction, and that this body—bearing the same relation to meconin that glycol, $C^2H^6O^2$, does to oxide of ethylene, C^2H^4O, or mannite, $C^6H^{14}O^6$, to mannitan, $C^6H^{12}O^5$—may give rise to meconin by subsequent loss of the elements of water. Making this supposition, the two reactions in question may be represented by the equations

$$(1) \ldots \quad \underset{\text{Opianic acid.}}{C^{10}H^{10}O^5} + H^2 = \underset{\text{Hypothetical hydrate of meconin.}}{C^{10}H^{12}O^5}$$

$$(2) \ldots \quad \underset{\text{Opianic acid.}}{2(C^{10}H^{10}O^5)} + H^2O = \underset{\text{Hypothetical hydrate of meconin.}}{C^{10}H^{12}O^5} + \underset{\text{Hemipinic acid.}}{C^{10}H^{10}O^6}$$

* Proc. Roy. Soc. vol. xi. p. 57.

Thus viewed, both transformations appear strictly analogous to the known transformations of oil of bitter almonds and many other substances; *e.g.*,

(1) . . .	$C^7H^6O+H^2$	=	C^7H^8O	(Friedel)
	Oil of bitter almonds.		Benzylic alcohol.	

(2) . . .	$2(C^7H^6O)+H^2O$	=	C^7H^8O +	$C^7H^6O^2$	(Cannizzaro)
	Oil of bitter almonds.		Benzylic alcohol.	Benzoic acid.	

and may be regarded as supporting the views of Berthelot, who has suggested that opianic acid ought to be classed as an aldehyde rather than as a true acid.

With regard to the constitution of hemipinic acid, the authors suppose that it may be a dimethylized derivative of a bibasic but tetratomic acid, $C^8H^6O^6$ (analogous to tartaric acid, $C^4H^6O^6$), the two atoms of methyl occupying the places of the two atoms of hydrogen, which, though outside the radical, are incapable of being replaced by metals: hemipinic acid would thus be comparable to Wurtz's ethyllactic acid, the body at one time described by Butlerow as valerolactic acid.

An analogy is further pointed out between the derivatives of malic acid and the substances obtained from cotarnine. This analogy becomes apparent on comparing the two following series of formulæ:—

Cotarnic acid	$C^{11}H^{12}O^5$	$C^4H^6O^5$	Malic acid.
(?) Cotarnamic acid	$C^{11}H^{13}NO^4$	$C^4H^7NO^4$	Aspartic acid.
Hydrochlorate of cotarnamic acid	$C^{11}H^{13}NO^4 . HCl$	$C^4H^7NO^4 . HCl$	Hydrochlorate of aspartic acid.
Cotarnine, or methyl-cotarnimide	$C^{11}H^{10}(CH^3)NO^3$	$C^4H^4(C^6H^5)NO^3$	Malanile, or phenyl-malimide.

The authors intend to continue their experiments.

II. Postscript to a Paper read January 15, 1863, "On the Formation of Fibrin from Albumen." By Alfred Hutchison Smee, Jun. Communicated by Alfred Smee, Esq., F.R.S. Received March 2, 1863.

Since the paper was read before the Royal Society the following additional facts have been elicited. Fibrin was obtained from

serum when subjected to oxygen gas, when acetic acid was added to it, although another portion of the same serum had refused to yield it without that addition. In this experiment the acetic acid should be added until the serum is either neutral, or produces a slightly acid reaction on test-paper. Care must be taken in these experiments to prevent the temperature rising too high, for a coagulation then takes place. If blood-cells be present in the serum, the addition of acetic acid attacks the cells in preference to the alkalies of the serum; and on subsequent exposure to a temperature of 100° F. during the period it is under the influence of oxygen, the whole is transformed into a semisolid mass.

It is a curious fact that serum which has been placed on a dialyser for the removal of the salts by Graham's method was not improved in its power of producing fibrin, over serum which had not been submitted to that treatment previous to its oxidation. On the other hand, albumen purified from salts by Graham's method, and then subjected to the influence of oxygen, yielded the largest amount of fibrin. By this method it is most probable that I should have been able to have transformed the whole of the albumen into fibrin, had not an accident unfortunately brought the experiment to a termination. Nevertheless, although the experiment was not continued long, half the albumen was changed into fibrin.

When experimenting upon albumen nearly free from alkalies and alkaline salts, great care must be taken to keep the temperature as low as possible. I found that a temperature between 80° and 90° F. was the best, for above 98° the albumen had a very great tendency to coagulate.

When albumen was placed in a tube which contained about an equal bulk of oxygen, and in which a platinized platinum wire had been inserted extending the whole length of the tube, to facilitate the action of the oxygen on the albumen, and which tube was subsequently sealed and placed in a water-bath of 98° F., no fibrin made its appearance even after the lapse of 36 hours, but in its place a small quantity of an amorphous material subsided to the bottom of the tube. When, however, a tube of similar size was filled with albumen having free access to the air, and then placed on the same water-bath for an equal length of time, on the surface of the albumen which this tube contained small masses of

fibrin were formed, which had an appearance identical with that of blood-fibrin under the microscope, giving a conclusive proof to my mind that, during the formation of fibrin by the action of oxygen on albumen, a volatile constituent is formed and carried off by the excess of oxygen which passes into the albumen in solution.

The following are the chief physical and chemical properties of the fibrin artificially formed by the action of oxygen on albumen:—

It has a lighter specific gravity than albumen, being always found floating on the surface of the albumen, provided it is free and not entangled or attached to the side of the vessel or platinized platinum wire that has been inserted in the albuminous solution.

It has a fibrinated appearance under the microscope, and is capable of being teased out into filaments in the same manner as blood-fibrin.

Acetic acid completely dissolves it after some time.

Soda and potash cause it to swell up and dissolve. Concentrated solution of ammonia, after the lapse of some hours, causes the fibrin to swell up in a gelatinous mass, similar to that which occurs when blood-fibrin is submitted to the same reagent.

A hot or cold solution of nitrate of potash does not dissolve it when it is digested in that menstruum for some hours.

With Millon's test it becomes of a brick-red colour.

With nitric acid a bright yellow colour became visible.

Fibrin heated with hydrochloric acid gave a blue colour, and subsequently dissolved, giving a blue tint to the liquid.

An acid solution of acetate of lead caused both blood-fibrin and fibrin artificially prepared to swell up and become translucent after digestion for a certain period.

III. "On Diffusion of Vapours: a means of distinguishing between apparent and real Vapour-densities of Chemical Compounds." By J. A. Wanklyn and J. Robinson, Esq. Communicated by Dr. Frankland. Received March 10, 1863.

The density of the vapour given off when a chemical compound is heated is not necessarily the *vapour-density* of that chemical com-

pound; sometimes it is only the mean density of the products of decomposition. Some of the best-known substances, such as hydrated sulphurous acid, ammoniacal salts, and pentachloride of phosphorus, suffer decomposition when they are vaporized, and thus have an apparent vapour-density, which is in reality nothing more than the mean density of the products of their decomposition.

We recognize such cases—in which the apparent is not the real vapour-density—by making a diffusion-analysis of the vapours. This method of solving questions of the kind was proposed by one of us two years ago*. In carrying it out practically, it was resolved from the first not to diffuse through a porous diaphragm, but to recur to Graham's original method, namely, to let our vapours diffuse through a simple aperture or through a short tube.

Independently of the experimental difficulties attending the use of a porous diaphragm at high temperatures, there is a fatal objection to it, founded upon the inconclusiveness of the results obtained in such a way.

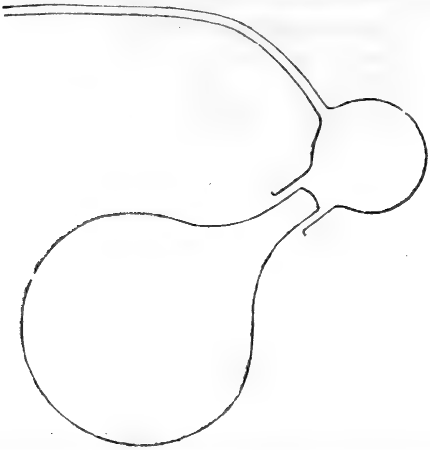
Our resolve to avoid porous substances was not by any means shaken by Pebal's memoir† on the diffusion of chloride-of-ammonium vapour through asbestos; for what is more likely than that a finely-divided silicate (a salt of an acid of indefinite capacity of saturation) should decompose ammoniacal salts at elevated temperatures?

The apparatus used in our experiments is of the simplest kind. It is represented in the drawing, and consists of two glass flasks, the necks of which do not fit air-tight: the narrow tube proceeding from the upper one is fused to the flask. The lower flask is for the reception of the vapour to be operated upon; the upper flask is for the atmosphere into which the vapour is to be diffused. The atmo-

* Playfair and Wanklyn on Vapour-densities, Transactions of Roy. Soc. of Edinburgh, 1861, vol. xxii. part 3. p. 458. In this paper it was proposed to extend to vapours what had previously been applied to gases. One of the earliest, perhaps the earliest example of a precise diffusion-analysis of a gas was given by Frankland in his research upon the isolation of ethyl (see Quart. Journ. Chem. Soc. vol. ii. p. 285, 1850). After describing his diffusion-apparatus and its use in the case of ethyl, Frankland proceeds, "This method might in almost every case be employed with advantage to determine whether or not any specimen of gas be simple or mixed."

† Ann. de Chim. et de Phys. January 1863.

sphere of dry air, or other suitable gas, is kept constantly renewed by the transmission of a slow stream of gas, which enters the upper



flask by the narrow tube above, and passes out by the space between the two necks, which, as we have said, do not fit air-tight. When in use, the whole apparatus is kept at a temperature above the condensing-point of the vapour by means of an air-bath.

After a diffusion has gone on for a sufficient length of time the apparatus is allowed to cool, and the contents of the lower flask are analysed, by which means it is seen whether diffusion has effected any alteration in the composition of the vapour.

We have used a lower flask of about 500 cubic centimetres capacity, with a mouth 10 millimetres in diameter; the capacity of the upper flask was 100 cubic centimetres.

The first substance taken for experiment was sulphuric acid, which is converted at high temperatures into vapour of sulphuric anhydride and vapour of water. Inasmuch as vapour of water is lighter than vapour of sulphuric anhydride, the former should diffuse more rapidly than the latter. Accordingly, the residue after diffusion should be richer in sulphuric anhydride than the acid before diffusion.

In one experiment we took an acid composed of

95	Mono-hydrated sulphuric acid.
5	Water.
100	

After diffusion for an hour at about 520° C., the residue was composed of

60 Mono-hydrated sulphuric acid.
40 Sulphuric anhydride.
——
100

In another experiment we took an acid containing

99 Mono-hydrate.
1 Water.
——
100

and after diffusion for a shorter time at 445° C. found the residue to consist of

75 Mono-hydrate.
25 Anhydride.
——
100

In both cases the residues after diffusion fumed strongly on exposure to the air, and consisted partly of crystals and partly of liquid.

The substance next submitted to diffusion was pentachloride of phosphorus, which is decomposed by heat into terchloride and free chlorine.

The pentachloride which we used gave no reaction with iodide of potassium and starch, and therefore contained no free chlorine; it gave no precipitate with corrosive sublimate, and therefore contained no terchloride of phosphorus. An analysis of it gave

Percentage of chlorine=84·67
The formula requires... 85·13

In one experiment we diffused into carbonic acid gas* for three-quarters of an hour at about 300° C., and afterwards dissolved the contents of the lower flask in water, and precipitated with corrosive sublimate, with the addition of a little hydrochloric acid. ·0175 gramme of calomel was obtained. In another experiment (also into carbonic acid) the time of diffusion was two hours, temperature 300° C., quantity of calomel obtained ·0285 gramme.

These two results leave no doubt as to the existence of terchloride

* If pentachloride of phosphorus be diffused into air, the residual terchloride combines with oxygen to form oxychloride of phosphorus, which does not reduce corrosive sublimate.

of phosphorus in the residue after diffusion; for the reduction of corrosive sublimate to calomel cannot be otherwise explained. Moreover, the presence of free chlorine in the diffused gases was shown by the reaction with iodide of potassium and starch.

We are continuing this research, and hope to lay before the Society the results of an examination of the most prominent cases of so-called abnormal vapour-density.

IV. "On a Simple Formula and Practical Rule for calculating Heights barometrically without Logarithms." By Alexander J. Ellis, B.A., F.C.P.S. Communicated by Dr. Neil Arnott, F.R.S. Received February 23, 1863.

The following formula and table for calculating heights barometrically without logarithms will be found to give the same results as Laplace's formula up to 30,000 feet, and the table can be readily extended if required. Let

L degrees be the mean latitude of the two stations,
$l = 2{\cdot}6257 \cos 2\text{L}$, $\text{G} = 1 + 0{\cdot}0026257 \cos 2\text{L}$,
$\text{R} = 20888629$, the number of feet in the earth's radius.

At the lower station.

H feet, its height above the sea, $\text{H}'' = \text{H}^2 \div \text{R}$,
B units of any kind, height of barometer, uncorrected,
B′ „ „ „ „ „ , corrected,
A deg. Fahr., A′ deg. Cent., A″ deg. Reaum., temperature of air,
M „ „ , M′ „ „ , M″ „ „ , „ of mercury.

At the upper station.

h, h'', b, b', a, a', a'', m, m', m'' in the same sense.

Then

$$h-\text{H} = \left[52400\frac{\text{B}-b}{\text{B}+b} + c - 2{\cdot}35\,.\,(\text{M}-m)\right].\frac{836+\text{A}+a}{900}$$
$$+{\cdot}001\,.\,(h-\text{H})\,l + h'' - \text{H}'', \quad \ldots \ldots \quad (a)$$

where $\text{M}-m=0$, when B, $b=\text{B}'$, b', and

$$2{\cdot}35\,(\text{M}-m) = 4{\cdot}23\,.\,(\text{M}'-m') = 5{\cdot}29\,.\,(\text{M}''-m'')$$
$$\frac{836+\text{A}+a}{900} = \frac{500+\text{A}'+a'}{500} = \frac{400+\text{A}''+a''}{400}.$$

The numbers c, l, h'', H″ are to be taken from the table on the next page, as will appear by the following examples.

Ex. 1. Height of Mont Blanc above Geneva from the observations of MM. Bravais and Martins, August 29th, 1844.

A′	19·3	B	729·65 mm.	M′	18·6	H	1335·33
a'	− 7·6	b	424·05	m'	− 4·2	L	46
	500·0	B+b	1153·70		22·8	l	0·09
	511·7	B−b	305·60		× 4·23		× $14\frac{1}{2}$
				p	96·4	q	1·3

$305{\cdot}6 \times 52400 \div 1153{\cdot}7$
$= 13880{\cdot}0$
272·9 c for 1300
61·6 diff. for 880
$\bar{1}03{\cdot}6$ $-p$
14118·1

$14118{\cdot}1 \times 511{\cdot}7 \div 500$
$= 14448{\cdot}5$
10·8 h'' for 15000 } h,
1·2 diff. for 800 }
$\bar{1}{\cdot}9$ $-$H″ for 1500, H
$\bar{1}8{\cdot}7$ $-q$

$h-$H$=14459{\cdot}0$ difference of level.

Result by Laplace's formula 14459·4.

Table of Corrections.

Feet.	c	Diff. for 100 feet.	$h''+$ H″−	Diff. for 100 feet.	L $l+$	L $l-$	l
1000	− 0·3	0·06	0·05	0·01	0°	90°	2·65
2000	+ 0·3	0·20	0·20	0·02	5	85	2·61
3000	2·3	0·41	0·43	0·03	10	80	2·49
4000	6·4	0·72	0·77	0·04	15	75	2·29
5000	13·6	1·08	1·20	0·05	20	70	2·03
6000	24·4	1·54	1·72	0·06	21	69	1·97
7000	39·8	2·07	2·35	0·07	22	68	1·91
8000	60·5	2·68	3·06	0·08	23	67	1·84
9000	87·3	3·35	3·88	0·09	24	66	1·77
10000	120·8	4·16	4·79	0·10	25	65	1·70
11000	162·4	5·04	5·79	0·11	26	64	1·63
12000	212·8	6·01	6·89	0·12	27	63	1·56
13000	272·9	7·07	8·09	0·13	28	62	1·48
14000	343·6	8·27	9·38	0·14	29	61	1·40
15000	426·3	9·55	10·77	0·15	30	60	1·33
16000	521·8	10·98	12·26	0·16	31	59	1·24
17000	631·6	12·40	13·84	0·17	32	58	1·16
18000	755·6	14·35	15·51	0·18	33	57	1·08
19000	899·1	16·05	17·28	0·19	34	56	0·99
20000	1059·6	18·03	19·15	0·20	35	55	0·91
21000	1239·9	20·23	21·11	0·21	36	54	0·82
22000	1442·2	22·56	23·17	0·22	37	53	0·73
23000	1667·8	25·14	25·33	0·23	38	52	0·64
24000	1919·2	27·96	27·58	0·24	39	51	0·55
25000	2198·8	31·00	29·92	0·24	40	50	0·46
26000	2508·8	34·65	32·36	0·25	41	49	0·37
27000	2852·3	37·96	34·90	0·26	42	48	0·27
28000	3231·9	41·99	37·53	0·27	43	47	0·18
29000	3651·8	46·36	40·26	0·28	44	46	0·09
30000	4115·4		43·09		45	45	0·00

Ex. 2. Rush's balloon ascent, September 10th, 1838 (see Meteorological Papers by Admiral FitzRoy, No. 9, p. 19).

A	60	B'	30·496 in.	H	0
a	5	b'	10·830	L	52
	836	$B'+b'$	41·326	l	0·64
	901	$B'-b'$	19·666	×	27
				q	17·3

19·666 × 52400 ÷ 41·326		27116 × 901 ÷ 900	
= 24935·8		= 27146·1	
2198·8	c for 25000	34·9	h'' for 27000
$\bar{1}81$·4	diff. for −65	0·3	diff. for 100
27116·0		$\bar{1}82$·7	$-q$
		$h-\mathrm{H}=27164\cdot0$	

Laplace's formula gives the same result.

As the British highlands do not exceed 5000 feet in altitude, and lie near the parallel of 56° north latitude, the corrections will nearly destroy each other. The following simple rule will therefore suffice for calculating all British heights :—

"Multiply the difference of the barometers by 524, and divide the product by the sum of the barometers, retaining three decimal places. Multiply this quotient by the sum of the temperatures of the air increased by 836, and divide the product by 9, keeping one decimal place. For aneroid and corrected mercurial barometers, the quotient is the height in English feet. For uncorrected barometers, subtract $2\frac{1}{2}$ times the difference of the temperatures of the mercury."

Ex. 3. Height of Ben Lomond (see Col. Sir H. James's Instructions for taking Meteorological Observations, App.).

A	59·0	B	29·890 in.	M	60·8
a	47·8	b	26·656	m	49·3
	836·0	$B+b$	56·546	$M-m$	11·5
	942·8	$B-b$	3·234		$\times 2\frac{1}{2}$
					28·7

$$3\cdot234 \times 524 \div 56\cdot546 \times 942\cdot8 \div 9 - 28\cdot7 = 3110\cdot5 = h - \mathrm{H}.$$

The height by Laplace's formula is 3110·8, by levelling 3115·8. The accuracy of the present formula is only intended to be tested by Laplace's, and it will be wrong to at least the same extent.

Very good results may also be obtained by neglecting H″, which is always very small, and transposing the terms h'' and $-2\cdot35\,(M-m)$; thus

$$h-H=\left(52400\frac{B-b}{B+b}+c+h''\right).\frac{836+A+a}{900}+\cdot001.(h-H)\,l-2\tfrac{1}{2}(M-m),$$

where $2\frac{1}{2}$ is written for 2·35 to compensate for omitting to multiply the latter by $(836+A+a)\div900$. This approximate form gives rise to the following practical rule for determining heights under 10,000 feet, embodying so much of the Table of corrections as is necessary for that purpose.

"Multiply the difference of the barometers by 52400, and divide by the sum of the barometers. If the number of clear thousands in the quotient be

	1,	2,	3,	4,	5,	6,	7,	8,	9,	10,
add	0,	0·5,	2·7,	7·2,	14·8,	26·1,	42·2,	63·6,	91·2,	125·6
and		0·2,	0·5,	0·8,	1·1,	1·6,	2·1,	2·9,	3·1	

for every additional hundred. Then multiply the result by the sum of the temperatures of the air increased by 836, and divide the product by 900. To this quotient

add for lat.	0,	10,	20,	30,	32,	34,	36,	38,	40,	42,	44,
subtract for lat.	90,	80,	70,	60,	58,	56,	54,	52,	50,	48,	46
the numbers....	2·6,	2·5,	2·0,	1·3,	1·2,	1·0,	0·8,	0·6,	0·5,	0·3,	0·1

for every clear thousand it contains. For aneroid and corrected mercurial barometers this result is the height in English feet. For uncorrected mercurial barometers, subtract $2\frac{1}{2}$ times the difference of the temperatures of the mercury.

"The barometers may be expressed in any units. If the temperatures are expressed in

degrees Centigrade, use........	500,	500,	$4\frac{1}{2}$,
degrees Reaumur, use	400,	400,	$5\frac{1}{2}$,
in place of..................	836,	900,	$2\frac{1}{2}$,

which are only suited for degrees Fahrenheit. The rule and the other numbers remain unaltered, and the result is in English feet."

Ex. 4. Height of Guanaxuato in Mexico.

A	77·5	B	30·046	M	77·5	L	21
a	70·3	b	23·660	m	70·3	l	2·0
	836·0	B+b	53·706	M$-m$	7·2		× 6·8
	983·8	B$-b$	6·386		$2\frac{1}{2}$	q	13·6
				p	18·0		

$6{\cdot}386 \times 52400 \div 53{\cdot}706$	$6260{\cdot}5 \times 983{\cdot}8 \div 900$
$= 6230{\cdot}7$	$= 6842{\cdot}5$
$26{\cdot}1$ for 6000	$13{\cdot}6 \quad q$
$3{\cdot}7$ diff. for 230	$\overline{1}81{\cdot}0 - p$
$6260{\cdot}5$	$h - \mathrm{H} = 6838{\cdot}1$

Result by Laplace's formula 6838·2.

These results are obtained by transforming Laplace's formula as follows. The original expression in the Méc. Cél. vol. iv. p. 293, reduced to English measures and the present notation, is

$$\left.\begin{aligned} h-\mathrm{H} &= 60158{\cdot}71\,.\,(1+0{\cdot}002845\cos 2\mathrm{L})\,.\,\frac{836+\mathrm{A}+a}{900} \\ &\times\left[\left(1+\frac{h-\mathrm{H}}{\mathrm{R}+\mathrm{H}}\right)\right](\log \mathrm{B}'-\log b')+\frac{h-\mathrm{H}}{\mathrm{R}+\mathrm{H}}\,.\,0{\cdot}868589\Big] \end{aligned}\right\}\,..(b)$$

which Delcros has transformed (in 'Annuaire Météorologique de la France' for 1849) to the equivalent of

$$\left.\begin{aligned} h-\mathrm{H} &= 60158{\cdot}71 \times [\log \mathrm{B}-\log b-0{\cdot}0000389278\,.\,(\mathrm{M}-m)] \\ &\times \frac{836+\mathrm{A}+a}{900}\times \mathrm{G}\times\left[1+\frac{h-\mathrm{H}+52251}{\mathrm{R}}+\frac{\mathrm{H}}{\frac{1}{2}\mathrm{R}}\right] \end{aligned}\right\}\,.\,(c)$$

The last factor may be split into the two

$$\left(1+\frac{52251}{\mathrm{R}}\right).\left(1+\frac{h+\mathrm{H}}{\mathrm{R}}\right)$$

without sensible error. Then, since

$$60158{\cdot}71 \times\left(1+\frac{52251}{\mathrm{R}}\right)=60309{\cdot}19$$

and $\qquad 60309{\cdot}19 \times 0{\cdot}0000389278 = 2{\cdot}34770,$

if we put $h-\mathrm{H}$ for the product of the three first factors on the right-hand side in (c), we find

$$\left.\begin{aligned} h-\mathrm{H} &= [60309{\cdot}19\,.(\log \mathrm{B}-\log b)-2{\cdot}34770\,.(\mathrm{M}-m)]\,.\,\frac{836+\mathrm{A}+a}{900} \\ &+\frac{h-\mathrm{H}}{1000}\times 2{\cdot}6257\cos 2\mathrm{L}+\frac{h^2-\mathrm{H}^2}{\mathrm{R}} \end{aligned}\right\}\,(d)$$

Putting 2·35 for 2·34770, and l, h'', H'' for their values, this form (d) will be identical with (a), provided that

$$60309{\cdot}19\,.\,(\log \mathrm{B}-\log b)=52400\,.\,\frac{\mathrm{B}-b}{\mathrm{B}+b}+c \quad . \quad . \quad (e)$$

Now putting $B-b=yB$, we have

$$\frac{B-b}{B+b}=\frac{y}{2-y}=\frac{1}{2}\cdot\left(y+\frac{1}{2}y^2+\frac{1}{4}y^3+\frac{1}{8}y^4+\ldots\right)=\frac{1}{2}z,$$

$$\log B-\log b=\log\frac{1}{1-y}=\mu.\left(y+\frac{1}{2}y^2+\frac{1}{3}y^3+\frac{1}{4}y^4+\ldots\right)=\mu(z+d),$$

where μ is the modulus of the tabular logarithms, and

$$d=\frac{1}{12}y^3+\frac{1}{8}y^4\ldots,$$

always a convergent series as y is always a proper fraction, and small when y is small, as it is for moderate heights.

Hence

$$60309{\cdot}19\,.\,(\log B-\log b)=60309{\cdot}19\times 2\mu\frac{B-b}{B+b}+60309{\cdot}19\,.\,\mu d$$

$$=52384\frac{B-b}{B+b}+c'.$$

The constant 52384 has been changed to 52400 to facilitate calculation and to divide the correction for the first two thousand feet, and c' has consequently been altered to c, the tabular values of which were calculated as follows.

Put $$x=52400\frac{B-b}{B+b}=52400\,.\,\frac{y}{2-y},$$

whence

$$y=\frac{2x}{52400+x}. \qquad (f)$$

Then (e) becomes

$$x+c=60309{\cdot}19\,.\,(\log B-\log b)=-60309{\cdot}19\log(1-y) \quad . \quad (g)$$

Make x successively $=1000$, 2000, &c. up to 30,000, and find the corresponding values of y from (f) and c from (g).

As the differences in the values of c are not uniform, slight errors may arise from neglecting second differences in interpolation, but they can scarcely even affect the result by a single unit, and may therefore be safely disregarded. Laplace's formula itself cannot be depended on within much larger limits.

The Table of corrections and transformation of Laplace's formula here given allow of the following simplification in the logarithmic calculation of $h-H$.

Let $\log n = \log[\log B - \log b - 0{\cdot}00004\,.\,(M-m)]$
$+1{\cdot}8261420 + \log(836+A+a)$
$= \log[\log B - \log b - 0{\cdot}00007\,.\,(M'-m')]$
$+2{\cdot}0814145 + \log(500+A'+a')$
$= \log[\log B - \log b - 0{\cdot}00009\,.\,(M''-m'')]$
$+2{\cdot}1783245 + \log(400+A''+a'')$,

then $h-H = n + {\cdot}001\,.\,nl + h'' - H''$,

where $1{\cdot}8261420 + \log 900 = 2{\cdot}0814145 + \log 500$
$= 2{\cdot}1783245 + \log 400 = 4{\cdot}7803845 = \log 60309{\cdot}19$.

This form requires less previous preparation, avoids the logarithms of numbers near to unity as $\left(1+\frac{A+a-64}{900}\right)$, and allows of the use of foreign data to obtain the result in English feet, so that it only becomes necessary to reduce the height of the lower station to English measures.

V. "Bessel's Hypsometric Tables, as corrected by Plantamour, reduced to English Measures and recalculated." By ALEXANDER J. ELLIS, Esq. Communicated by Dr. NEIL ARNOTT. Received February 23, 1863.

These Tables, with the preliminary explanations respecting their correction and reduction, have been, by direction of the Council, communicated to Admiral FitzRoy for insertion in the "Meteorological Papers published by Authority of the Board of Trade," and will appear in the twelfth Number of the series.

Let $\log n = \log [\log B - \log b - 0{\cdot}00004 \,.\, (M - m)]$

$$+ 1{\cdot}8261420 + \log(836 + A + a)$$

$$= \log [\log B - \log b - 0{\cdot}00007 \,.\, (M' - m')]$$

$$+ 2{\cdot}0814145 + \log(500 + A' + a')$$

$$= \log [\log B - \log b - 0{\cdot}00009 \,.\, (M'' - m'')]$$

$$+ 2{\cdot}1783245 + \log(400 + A'' + a''),$$

then $h - H = n + {\cdot}001 \,.\, nl + h'' - H''$,

where $1{\cdot}8261420 + \log 900 = 2{\cdot}0814145 + \log 500$

$$= 2{\cdot}1783245 + \log 400 = 4{\cdot}7803845 = \log 60309{\cdot}19.$$

This form requires less previous preparation, avoids the logarithms of numbers near to unity as $\left(1 + \frac{A + a - 64}{900}\right)$, and allows of the use of foreign data to obtain the result in English feet, so that it only becomes necessary to reduce the height of the lower station to English measures.

V. "Bessel's Hypsometric Tables, as corrected by Plantamour, reduced to English Measures and recalculated." By Alexander J. Ellis, Esq. Communicated by Dr. Neil Arnott. Received February 23, 1863.

These Tables, with the preliminary explanations respecting their correction and reduction, have been, by direction of the Council, communicated to Admiral FitzRoy for insertion in the "Meteorological Papers published by Authority of the Board of Trade," and will appear in the twelfth Number of the series.

April 16, 1863.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

Pursuant to notice given at the last Meeting, the Right Honourable Sir Edmund Walker Head, Baronet, was proposed for election and immediate ballot.

The ballot having been taken, Sir Edmund W. Head was declared duly elected a Fellow of the Society.

The following communications were read :—

1. "On Ozone." By E. J. Lowe, Esq., F.R.A.S., F.L.S. Communicated by Sir J. F. W. Herschel, Bart. Received March 16, 1863.

(Abstract.)

This paper consists of two parts, viz. :—

1. On the precautions necessary in ozone observations, and on certain corrections requisite before the actual amount can be determined.

2. The discovery of dry ozone powders as a substitute for the ordinary tests; an investigation into the ozone paper tests of M. Schönbein and Dr. Moffat, the determination of a proper formula for the tests, with an account of various observations and experiments made on the subject.

Part I.

At the last Meeting of the British Association I read a paper on the precautions and corrections requisite in order that a more perfect knowledge of ozone might be obtained. These precautions comprise uniformity of observation, each observer using the same box and the same tests, suspended at the same height, and as nearly as possible placed under the same circumstances. The corrections necessary are :—1st, For the velocity of the air; 2nd, for the height of the barometer; 3rd, for temperature; 4th, for the hygrometrical state of the air; 5th, for elevation above the ground.

1. *Velocity of the Air.*—The greater the speed the more ozone will be apparent, and this seems to be owing more to the increased velocity of the air than to a greater proportion of ozone.

2. *Height of the Barometer.*—It is found that during the last four years,

With the barometer at		$28\frac{1}{2}$	inches the amount of ozone was			5·7
"	"	29	"	"	"	3·5
"	"	$29\frac{1}{2}$	"	"	"	2·0
"	"	30	"	"	"	1·3
"	"	$30\frac{1}{2}$	"	"	"	0·4

A law as regards ozone and pressure is clearly apparent; but as the barometer falls for wind, the excess at low pressures is no doubt partly due to the increased velocity of the air. There is more ozone with the wind between W.S.W. and S.S.E. than when between

N.N.W. and E.N.E., and the barometer is half an inch lower with S.W. winds than with N.E. winds.

3. *Temperature.*—Temperatures between 30° and 40° will give less ozone than when between 40° and 50°, and the latter less than when between 50° and 60°. The same holds good when the ozone box is artificially warmed. This does not extend to very high temperatures, because the great dryness of hot weather is against the action of ozone on the tests.

4. *Moisture.*—Increase of moisture up to a certain point is favourable to the colouring of the tests, beyond which it operates unfavourably; for when the air is completely saturated with moisture, the effect of ozone is at its minimum.

5. *Altitude.*—The higher the test is hung the darker will be the colour obtained. The difference is as 4 to 6 between 4 feet and 35 feet above the ground.

There are yet several other circumstances to be mentioned:—

1. *Hour of the Day.*—The difference between the ozone readings at night and in the daytime are—

In June and July an excess at night of		0·1
In August and September	,,	0·4
In October and November	,,	0·5
In December and January	,,	0·8
In February and March	,,	0·7
In April and May	,,	0·7

the average excess of the summer months being only one-half of that which occurs in winter,

2. *Direction of the Wind.*—There is most ozone with the wind between S. and S.W., and least when between N. and N.E.

3. *Protection of the test from light.*—It is absolutely requisite that the test should be in a dark box; and no box has been found to answer so well as that constructed by myself, and known as "Lowe's Ozone box," an account of which has been furnished to the Royal Society* and to the British Association. This box, if freely exposed, and made to veer with the wind, so as always to present the opening to the direct current, is everything that could be desired.

The foregoing observations will be sufficient to show that precautions are requisite in these records, and that certain corrections are

* Proceedings, vol. x. p. 531.

necessary before we can declare whether ozone is present in a certain fixed amount, or whether it changes from hour to hour. These corrections have yet to be found out; those for the height of the barometer and the force and direction of the wind will be considerable.

Part II.

The ozone tests heretofore used have appeared to me to be unsatisfactory, and, on close examination, I found them to be faulty in many respects. The paper used had a glaze upon it, which prevented the solution from penetrating it; substances, moreover, had been used in its manufacture which acted injuriously on the tests. Again, the starch of commerce was found to be impure; it is manufactured with lime, sulphuric acid, and chlorine, substances fatal to these tests. The iodide of potassium was also impure; and there has been a want of uniformity in the proportions of starch and iodide of potassium employed by different observers.

Having found out that the starch of commerce was impure, I procured a jar of wheat-starch in the wet state before any chemicals had been used. This was steeped in distilled water, which was changed every two days until quite sweet to the taste, and, although by a long process, a chemically pure starch was thus obtained.

Sir John Herschel suggested trying other vegetable starches; I therefore made starch from rice, potato, sago, and wheat.

I obtained chemically pure iodide of potassium from Mr. Squire of Oxford Street, who forwarded me two samples made expressly for these experiments, the one prepared with water, the other crystallized several times from alcohol.

On the recommendation of Dr. R. D. Thomson, 15 grains of prepared chalk have been added to each ounce of air-dried starch to prevent it from becoming sour from any moisture that might be contained in it; subsequent observations have proved that this is absolutely requisite for uniformity of effect, as the intensity of action depends upon the amount of water contained in the starch, which is apparent from the following experiment:—

Tests made with air-dried starch—

α. Without further drying became coloured in 5 minutes.

β. After further drying by fire-heat for 1 minute became coloured in 7 minutes.

γ. After further drying by fire-heat for 3 minutes became coloured in 9 minutes.

δ. After further drying by fire-heat for 10 minutes became coloured in 13 minutes.

ϵ. After further drying by fire-heat for 30 minutes became coloured in 20 minutes.

η. With chalk added became coloured in 20 minutes.

With regard to the calico or paper used for the tests, both stained when impure. However, Mr. Joseph Sidebotham of the Strine Works prepared for me some chemically pure calico, and I was also enabled to procure a very porous chemically pure paper, both of which answer perfectly.

Having succeeded with the ozone slip tests, I tried as a first experiment a mixture of 10 parts of starch to 1 of iodide of potassium as a "dry-powder test;" this, when well mixed in a mortar, was bottled ready for use. A small portion was placed in the open air, and ten minutes' exposure showed that powder tests were an undoubted success, being more sensitive than the test slips. My next determination was what strength would colour quickest, and accordingly a number of strengths were prepared, varying in the proportions from 1 of iodide of potassium and 1 of starch up to 1 of iodide of potassium and 30 of starch, the starch used being made from wheat. From these experiments it was found that the proportion of 1 of iodide of potassium to 5 of starch was invariably the darkest, the degree of darkness diminishing in either direction when other strengths were used; thus 1 of iodide of potassium to $4\frac{1}{2}$ of starch, or 1 to $5\frac{1}{2}$, were neither so dark as with a strength of 1 to 5.

On repeating these experiments with potato-starch, the proportion that coloured soonest was 1 to $2\frac{1}{2}$; and this second series of experiments proved that with each starch a special formula is requisite.

My next experiments were with the view of ascertaining the effect of various acids and chemical substances on the ozone powder tests. For this purpose I procured a number of cups for solutions, and small pill-boxes to hold the powder tests, and these were placed together under separate bell-glasses. The result was that the following coloured the powder tests very rapidly:—Hydrochloric acid, nitric acid, nitrous acid, chloride of lime, phosphorus, iodine (in scales),

iodine (dissolved in alcohol), carbonate of iron on which sulphuric acid was poured, carbonate of iron on which glacial acetic acid was poured, limestone on which sulphuric acid was poured, limestone on which glacial acetic acid was poured, matches lighted under the bell-glasses. The following did not colour the tests :—Sulphuric acid, glacial acetic acid, carbonate of lime, carbonate of iron, ammonia, matches not lighted.

The substances used in the manufacture of ordinary starch of commerce gave the following : —

Chloride of lime coloured the tests instantaneously.

Sulphuric acid did not colour the tests.

Lime did not colour the tests.

Lime and sulphuric acid mixed coloured the tests rapidly.

There are advantages in the powders over the ordinary tests. They are more sensitive, and therefore more rapidly acted upon ; they retain their maximum colour, not afterwards fading, as with the tests of Schönbein and Moffat. (However, my calico and porous-paper tests are not nearly so liable to fade, owing to the solution penetrating into the fabric used, instead of being merely a surface-covering.) There is also a more important advantage still to be mentioned from the use of powders. By the aid of powder tests we shall ascertain what colours the tests ; in the experiments it was found that a different colour was imparted to the powder, and that the colour penetrated deeper with some substances and acids than with others, so that differences of effect took place, from which the different materials used might be recognized. Thus :—

1. Iodine, although coloured a brown-black, was merely a surface colouring, below the powder remained colourless.

2. Phosphorus, bluish black on the surface only, below almost colourless.

3. Chloride of lime, deep brown on the surface only, the powder below slightly yellow.

4. Hydrochloric acid, grey-pink on the surface only, the powder beneath orange.

5. Nitric acid, dark-red brown extending slightly into the powder, beneath that colourless.

6. Carbonate of iron with glacial acetic acid, yellowish brown to the thickness of cardboard, below that buff.

7. Limestone with sulphuric acid, pale brown to the thickness of cardboard, beneath slightly coloured.

8. Carbonate of iron with sulphuric acid, black to the depth of a quarter of an inch.

9. Nitrous acid, dark brown more than the eighth of an inch deep, beneath yellowish brown.

10. Nitric acid mixed with exposed ozone powder, blue-black to the sixth of an inch deep, below that reddish brown.

11. Nitric acid mixed with unexposed ozone powder, blue-black to the sixth of an inch deep, below that reddish brown.

These experiments may require some modification, yet they point out the fact that striking differences are apparent, differences which must open up a new method of investigating ozone.

Not only have the tests hitherto used been made without due regard to the pureness of the chemicals and fitness of the material used, but the paper box in which they have been kept is not sufficient for their perfect preservation; a dark, dry, air-tight box is essential; and this should not be opened in a room where there is iodine, chlorine, nitric acid, phosphorus, hydrochloric acid, or other chemicals likely to be injurious to the tests. I am now manufacturing the tests, which will be distributed by Messrs. Negretti and Zambra, and I have constructed a proper box in which in future they will be sent.

II. "On the Equations of Rotation of a Solid Body about a fixed Point." By William Spottiswoode, M.A., F.R.S. Received March 21, 1863.

(Abstract.)

In treating the equations of rotation of a solid body about a fixed point, it is usual to employ the principal axes of the body as the moving system of coordinates. Cases, however, occur in which it is advisable to employ other systems; and the object of the present paper is to develope the fundamental formulæ of transformation and integration for any system.

The integrals found are—

$$p_1=\sqrt{\frac{k^2-(\mathrm{S}+\theta_2)h}{\theta-\theta_2}}\cos am\left(\sqrt{\frac{\theta_2-\theta_1}{\nabla}}\sqrt{k^2-(\mathrm{S}+\theta)h}\,t+f\right);$$

$$q_1=\sqrt{\frac{k^2-(\mathrm{S}+\theta_2)h}{\theta_1-\theta_2}}\sin am\left(\sqrt{\frac{\theta_2-\theta_1}{\nabla}}\sqrt{k^2-(\mathrm{S}+\theta)h}\,t+f\right);$$

$$r_1=\sqrt{\frac{k^2-(\mathrm{S}+\theta)h}{\theta_2-\theta}}\,\Delta\, am\left(\sqrt{\frac{\theta_2-\theta_1}{\nabla}}\sqrt{k^2-(\mathrm{S}+\theta)h}\,t+f\right);$$

where θ, θ_1, θ_2 are the roots of the cubic

$$(\mathrm{S}+\theta)^3-\mathrm{S}(\mathrm{S}+\theta)^2+S(\mathrm{S}+\theta)-\nabla=0,$$

∇ being the determinant of the system A, B, C, $-$F, $-$G, $-$H, S the sum of A, B, C, and S that of the corresponding inverse quantities. Moreover p_1, q_1, r_1 are linear functions of p, q, r (the components of rotation about the axes for which A, B, C, &c. are calculated), the coefficients of which are determined in the paper itself.

III. "On the Fossil Human Jawbone recently discovered in the Gravel near Abbeville," in a Letter to the President, by W. B. Carpenter, M.D., V.P.R.S. Received April 16, 1863.

University of London, Burlington House, W.
April 16, 1863.

Dear Mr. President,—I esteem it a privilege to have it in my power to communicate, through you, to the Royal Society some particulars of the important discovery just made by M. Boucher de Perthes, of a *human maxilla* in one of the gravel-beds near Abbeville also yielding the now well-known flint implements. Having been informed of this discovery a few days since, whilst staying in Paris, I became additionally desirous of carrying out my previous intention of stopping at Abbeville on my way homewards; and accordingly, after a short visit to Amiens,—which gave me the opportunity of disinterring for myself a small but well-characterized flint implement from the gravel-pit of St. Acheul,—I proceeded on the afternoon of Monday last to Abbeville, where I was received with the greatest kindness and attention by M. Boucher de Perthes.

The history of his discovery is given in the following extract from the local journal 'L'Abbevillois,' by which it will be seen that the specimen in question was removed by M. Boucher de Perthes himself from the bed in which the first indications of it had been found by the workman employed in that part of the excavation:—

"A la fin de mars dernier, le terrassier Halatre, travaillant à cette carrière, vint lui apporter avec un silex taillé un petit fragment d'os qu'il y avait également recueilli. Ayant débarrassé ce fragment du sable qui le couvrait, M. de Perthes aperçut une dent fort endommagée, mais qu'il n'en reconnut pas moins pour une molaire humaine.

"Il suivit immédiatement Halatre à Moulin-Quignon, vérifia la place d'où venait la hachette et la dent, s'assura que cette place était nette de toute infiltration ou introduction secondaire et fit continuer la fouille.

"Elle ne produisit ce jour-là aucun résultat nouveau.

"Convaincu que quelqu'autre débris du corps d'où provenait cette molaire devait se trouver là, M. Boucher de Perthes recommanda aux terrassiers de ne rien déranger de ce qu'ils pourraient remarquer pendant son absence, mais de le prévenir sans retard. En effet, le 28 mars le terrassier Vasseur vint lui dire que quelque chose ressemblant à un os paraissait dans le banc.

"Rendu sur les lieux, M. de Perthes trouve le terrain comme l'avait dit Vasseur. L'extrémité de l'os enfermé dans sa gangue se montrait d'environ deux centimètres.

"Voulant l'avoir entier, M. de Perthes fit, à l'aide d'une pioche, dégager les alentours et, à sa grande satisfaction, il put le retirer du banc sans le rompre.

"Il ne s'était pas trompé dans ses prévisions. La dent avait annoncé la tête, et dans le morceau qu'il venait d'extraire, il reconnut une machoire humaine.—Un grand problème était résolu.

"A quelques centimètres de ce fossile humain, le premier peut-être dont la position géologique eût été aussi nettement constatée, car, par une autre circonstance heureuse, les témoins ici ne manquaient pas, était une hache en silex également engagée dans le banc, d'où, sur l'invitation de M. Boucher de Perthes, M. Oswald Dimpre, jeune archéologue et dessinateur habile bien connu des savants qui ont visité Abbeville, l'enleva mais non sans s'aider aussi de la pioche.

"Une chose qui frappa tous les spectateurs, ce fut la parfaite identité de patine ou de couleur de cette machoire, des silex taillés et des cailloux roulés, avec le banc qui les contenait, couleur brune, presque noire, contrastant singulièrement avec la teinte jaune ou grise des bancs supérieurs et la craie blanche sur laquelle elle repose.

"Mesure prise de chacune des couches supérieures, la machoire fossile était ainsi que les hachettes à 4 mètres 52 centimètres * de la superficie et tout près de la craie.

"Ce banc de Moulin-Quignon, placé sur le plateau qui domine la vallée, se trouve à 30 mètres au-dessus du niveau de la Somme et de la mer."

* About 14 feet 10 inches.

The particulars I have now to communicate as the result of my own personal examination should, I think, most completely satisfy any unprejudiced person that, on the one hand, the specimen cannot have been a "plant" contrived by the workman, and, on the other, that it could not have found its way into the bed in which it was discovered by any disturbing agency subsequent to the original deposition of that bed *.

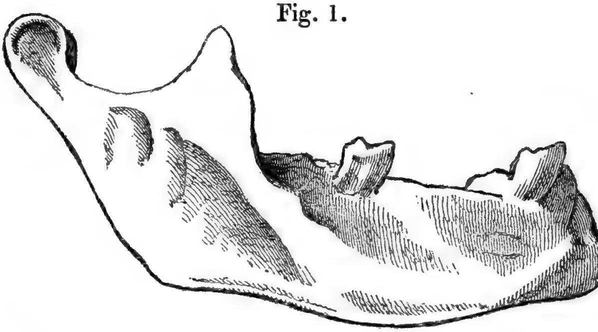
When M. Boucher de Perthes had the kindness to place in my hands this precious fragment—which consists of the right half of the lower jaw, containing three teeth—I was immediately struck with its almost black colour, its solidity, and its weight: all these peculiarities (which are in marked contrast to the characters of the bones ordinarily found in these gravel-pits) being obviously due to one and the same cause, viz. metallic (ferruginous?) infiltration. The ordinary flints, and the flint implements obtained from the same deposit, several of which are in the museum of M. de Perthes, are all of them characterized by a like depth of ferruginous tint, which is not seen in any of those taken from any other part of the same pit, or from any other gravel-pit yet opened in the neighbourhood of Abbeville.

As to the anatomical characters of this jaw, I should not wish, without a more careful comparative examination of the specimen than I had the opportunity of making, to give any decided opinion; but my impression is that they differ very decidedly from those of the same bone in any race at present inhabiting Western Europe.

* I think it right to leave my original statement as the record of the impression which was made not only upon myself, but upon other more competent observers, by the first examination of the specimen in question, which was limited to its external characters. Its *colour* has been subsequently proved to be due to the adhesion of a thin layer of the ferruginous matrix, which adhered closely to its surface, and yet could be readily washed off, leaving the bone but slightly stained. The impression of *solidity* was produced by the density of the bone itself; the lower jaw being the densest bone in the body next to the petrous portion of the temporal bone, and having in aged subjects an almost ivory-like hardness. The impression of *weight* was not really produced by metallic infiltration, as was at first supposed, but was partly due to a want of adequate allowance for the density of the bone itself, and partly to the adhesion, on one side, of a good deal of the matrix, which has been found to contain as much as 12 per cent. of oxide of iron; and it may also have been in part *subjective*, arising from a preconception, suggested by the general appearance of the bone, that it had undergone infiltration. The colour of the flint implements said to have been found in the same deposit, has proved to be removable by washing; whilst some of the ordinary flints are stained by real ferruginous infiltration.—June 15, W. B. C.

I was struck with the *thickness* of the bone, the great *breadth* of the ascending ramus, but especially with the extraordinary breadth and depth of the groove between the ramus and alveolar border, in which I could almost lay my little finger. The jaw would appear to be that of a person advanced in life; and the tooth originally found, which very probably belonged to the other half of the same jaw, seemed to me to have been "endommagée" by *caries* during life rather than by subsequent violence.

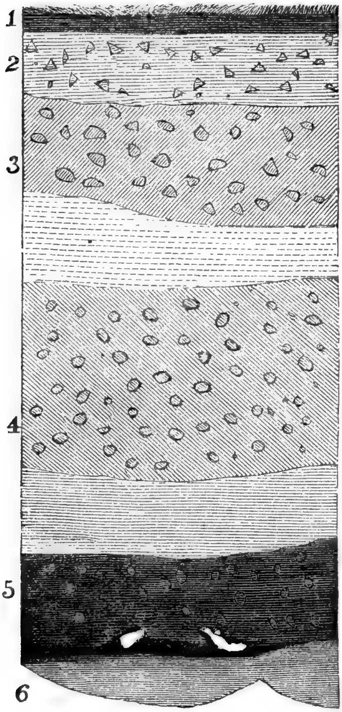
Fig. 1.



M. Boucher de Perthes had the kindness to give me the accompanying sketch of the specimen; and I can testify to the accuracy of its representation of the general form of the bone.

On Tuesday morning I repaired, in company with M. Boucher de Perthes, to the gravel-pit of Moulin-Quignon; in which he showed me, as nearly as he could, the situation in which this most interesting relic had been found. Unfortunately there had occurred, a few days previously to my visit, a *slip* of the overlying strata, by the débris of which the exact spot was covered; but a part of the same deposit was visible at a horizontal distance of a yard or two, so that I could indubitably verify its position and its general characters. This deposit, distinguished from every other by the extreme depth of its ferruginous tint (which corresponds exactly to that of the bone), lies *at the very bottom of the pit, in immediate contact with the subjacent chalk*, as shown in the accompanying representation of the section (also kindly given to me by M. Boucher de Perthes), to the general accuracy of which I can bear the most explicit testimony. I myself took away from this deposit some specimens of the small rounded flints which it contains, and which will serve to show you this peculiar tint.

Fig. 2.—*Section of the Strata in the Gravel-pit of Moulin-Quignon, near Abbeville.*



	ft.	in.
1. Vegetable mould	1	0
2. Undisturbed subsoil, consisting of grey sand with broken flints..............................	2	3
3. Yellowish argillaceous sand, with large flints but little rolled, resting on a layer of grey sand........................	5	0
4. Yellow ferruginous sand, containing smaller and more rolled flints, and divided by a second layer of less yellow sand	5	7
5. Argillaceous sand of a deep brown or almost black hue, sticking to the hand and staining it, containing small flints more rolled than those of the upper strata.—N.B. The white spaces left in this layer mark the position of the jaw and of the flint hatchet found in contiguity with it .	1	8
	15	6
6. Chalk.		

These facts must be admitted, I think, to exclude any possibility of doubt as to the truly fossil character of this bone. Its peculiar condition could not have been produced by any artificial means at present known, and most assuredly indicates that it must have been long buried in the deposit from which its metallic impregnation has been derived *.

That it could not have found its way into that deposit in any other mode than by *original imbedding*, may be fairly concluded from the entire absence of the least *indication* of disturbance in the

* The cogency of these inferences is of course invalidated by the proved incorrectness of the impressions on which they were founded, as stated in the preceding note. How far the genuineness of the specimen is supported or contravened by other evidence, is a question on which there is at present so great a diversity of opinion among experienced Palæontologists, that I think it better to abstain from any judgment in regard to it.—June 15, W. B. C.

superjacent strata, which are most regularly superposed (as seen in the accompanying section) to a depth of more than 15 feet. This complete regularity of superposition in the strata of the gravel-pits of Moulin-Quignon has, I understand, been already verified by numerous experienced geologists, whose testimony upon such a point is of far higher value than mine; but it is so obvious that I cannot imagine the least doubt to remain in the mind of any intelligent observer who may visit the locality and examine its condition for himself, of the jaw having been imbedded in the lowest stratum before the deposition of the superincumbent layers.

I have further to point out, that as the gravel-bed of Moulin-Quignon is about 100 feet above the present level of the river, it corresponds in position with the *upper* gravel of St. Acheul, not with the *lower* gravel of Menchecourt. If, therefore, we accept the conclusions of Mr. Prestwich as to the relative ages of these gravels, this human jaw was buried in the *very oldest* portion of the *earliest* of these fluviatile deposits, and therefore dates back to the very remotest period at which we have at present any evidence of the existence of Man.

Believe me, dear Mr. President,

Yours faithfully,

William B. Carpenter.

April 23, 1863.

Major-General SABINE, President, in the Chair.

The Right Hon. Sir Edmund Walker Head was admitted into the Society.

The following communications were read :—

I. "On the Diurnal Inequalities of Terrestrial Magnetism, as deduced from observations made at the Royal Observatory, Greenwich, from 1841 to 1857." By George Biddell Airy, F.R.S., Astronomer Royal. Received April 8, 1863.

(Abstract.)

The author describes this paper as one of the class which gives the epitomized results of long series of voluminous observations and laborious calculations, of which the fundamental details have been

printed in works specially devoted to these subjects. It exhibits in curves the diurnal inequalities of terrestrial magnetism, as obtained by the use of instruments essentially the same, through the whole period of seventeen years, during the last ten years of which the magnetic indications have been automatically recorded by photographic self-registration, on a system which has been continued to the present time, and is still to be continued.

From the last months of 1840 to the end of 1847, the observations were made by eye, every two hours. From the beginning of 1848, for the declination and horizontal force magnetometers, and from the beginning of 1849, for the vertical force magnetometer, the magnetic indications are recorded by Mr. Brooke's photographic apparatus.

In preparing the reductions of the magnetic records from 1848 to 1857 (which are printed in the "Results of Magnetical and Meteorological Observations for 1859," bound in the volume of 'Greenwich Observations,' 1859, and also issued separately), the days of unusual magnetic disturbance had been separated from the rest, and the reductions applied to the mass so diminished. For unity of plan, it appeared expedient to follow the same course for the reductions from 1841 to 1847. In consequence of this, the numbers which are used here differ in some cases by small quantities from those printed in the 'Greenwich Magnetical Observations from 1841 to 1847.' The numbers in the reductions from 1848 to 1857 are adopted without change.

The author remarks that, taking the number of omitted days as a rough measure of the amount of magnetic disturbance, there is no appearance of decennial cycle in their recurrence, and no distinct relation to the magnitude of diurnal changes.

The author then proceeds to the description of the curves. The first four sheets contain the curves in which the horizontal abscissa represents the declination at each hour as compared with the mean for the twenty-four hours and the vertical ordinate represents the horizontal force at each hour as compared with the mean for the twenty-four hours. On the different sheets the days are differently grouped, thus:—On sheet I. all the observations at each nominal hour throughout the year are combined; this sheet contains the separate curves for 1841, 1842, 1843, 1844, 1845, 1846, 1847. On sheet II. similar curves are formed for 1848, 1849, 1850, 1851,

1852, 1853, 1854, 1855, 1856, 1857. On sheet III. all the observations at each nominal hour through all the months January from 1841 to 1847 are combined to form the January curve; all those through the months February to form the February curve, and so on. On sheet IV. similar month-curves are formed from the period 1848 to 1857. It is remarked that the origin of coordinates necessarily represents the mean declination and mean horizontal force in each month.

The author then points out that the means for each month are themselves subject to an annual inequality, which can be ascertained with little difficulty. The values of these inequalities are exhibited, for declination and horizontal force, separately for the period 1841–1847 and for the period 1848–1857; those in the first period far exceed in magnitude those in the second (as holds also with regard to all the diurnal inequalities).

If we wished to exhibit the hourly state of magnetism, as referred to the mean state given by the supposition of uniform secular change of normal magnetism, we ought to apply these quantities with sign changed, to the origin of coordinates in each curve, in order to form a new origin of coordinates. For the year-curves, the numbers destroy each other, and no new origin of coordinates is produced; for the month-curves, however, they shift the origin materially. The author does not perceive that any facility for theoretical reference or other advantage is gained by this step.

On examining the year-curves, it is seen that from 1841 to 1848 their magnitude very slowly increases, with a small change of form, but from 1848 to 1857 their magnitude very rapidly diminishes, with a great change of form. Some great cosmical change seems to have come upon the earth, particularly affecting terrestrial magnetism. On comparing these year-curves with the month-curves, especially with those for the period 1848–1857, it appears that the change of the year-curves from 1848 to 1857 nearly resembles that of the month-curves from summer to winter; and the author points out as a possible step to a physical explanation of the change from 1848 to 1857, that the magnetic action of the sun upon the earth's southern hemisphere may have remained nearly unaltered, while that on the northern hemisphere may have undergone a great diminution.

The author then alludes to the curves representing the hourly

state of vertical force, as referred to the mean on each day. The force is here represented by a simple ordinate. The grouping is made by years and by months in the same manner as for the curves already mentioned. The month-curves of the two periods (1841–1847 and 1848–1857) differ, in the magnitude and change of magnitude of the ordinates, and in the place and change of place of node. The year-curves of the two periods have some very remarkable differences. From 1847 to 1849 the magnitude of the ordinates increases sensibly; from 1849 to 1850 still more; it then remains nearly stationary. In 1846 the descending node is at $11\frac{3}{4}^{h}$ nearly; in 1847 it is at 9^{h} nearly; in 1849 at 7^{h} nearly; in 1850 at 5^{h}; in 1851 at 4^{h}; and there it continues with little alteration. It is important to observe that, though the instrument was changed in 1848, the change in the place of the node did not then occur suddenly; it had begun with the old instrument, and continued to advance gradually for several years with the new instrument. The author states that he had verified the correctness of the node in the first period from other observations, but he had not succeeded in finding observations corresponding in date with those of the latter period.

The paper is followed by eight sheets of curves, as follows:—

I. *Diurnal Curves of combination of Declination and Horizontal Force.*

(1) Mean of all the days in each year (separately), 1841–1847.

(2) Mean of all the days in each year (separately), 1848–1857.

(3) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1841–1847.

(4) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1848–1857.

II. *Diurnal Curves of Vertical Force.*

(5) Mean of all the days in each year (separately), 1841–1847.

(6) Mean of all the days in each year (separately), 1849–1857.

(7) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1841–1847.

(8) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1849–1857.

II. "On the direct Transformation of Iodide of Allyle into Iodide of Propyle." By Maxwell Simpson, M.B., F.R.S. Received April 7, 1863.

Iodide of allyle, as is well known, combines directly with two equivalents of metallic mercury, a well-defined crystalline compound being formed. Would it be possible to make the same body combine with two equivalents of hydrogen, and thus to open a direct passage from the allylic to the propylic series of compounds? *Indirectly* this transformation has been already effected by M. Berthelot through the intervention of propylene gas.

In order to determine the above point, I submitted iodide of allyle to the action of hydriodic acid gas. On passing this gas into the iodide, the latter became strongly heated and black from the liberation of a large quantity of iodine. As soon as the gas was observed to pass unabsorbed through the liquid, the latter was allowed to cool, and filtered through asbestos. It was then decolorized by agitation with a dilute solution of caustic potash, dried over chloride of calcium, and distilled. Almost the entire quantity passed over between 90° and 95° Cent. The portion distilling between 92° and 94° Cent. I collected apart and analysed. The numbers obtained correspond with the composition of iodide of propyle, as will be seen from the following table:—

	Theory.	Per cent.	Experiment.
C_6	36	21·18	21·29
H_7	7	4·11	4·16
I	127	74·71	
	170	100·00	

The specific gravity of the iodide is 1·73 at zero.

In order to satisfy myself that the body I had in my hands was really an ether of propylic alcohol, I endeavoured to prepare that alcohol from it. This I succeeded in doing in the following manner:—About 60 grammes of the iodide were added to an equivalent of oxalate of silver contained in a flask surrounded by water. The mixture became strongly heated from the violence of the reaction, and the decomposition was soon complete. It was then digested with ether. On submitting the ethereal solution to distillation, I observed that, as soon as the ether had passed over, the thermometer

rose rapidly to 186°, and that the entire liquid, previously dissolved in the ether, distilled over between that temperature and 197° Cent. This was no doubt oxalate of propyle. On heating this body in a retort with solid caustic potash, I obtained a volatile distillate. This I then dried over chloride of calcium, and in order to secure its complete dehydration, treated it with a small piece of sodium. On re-distilling, I found that the entire liquid passed over between 83° and 88° Cent. The portion distilling between 85° and 88° gave on analysis results corresponding with the formula of propylic alcohol, as will be seen on inspecting the following table :—

	Theory.	Per cent.	Experiment.
C_6........	36	60·00	59·21
H_8........	8	13·33	13·47
O_2........	16	26·67	
	60	100·00	

By treating this body with iodine and phosphorus, I succeeded in regenerating iodide of propyle. This is a very ready method of preparing propylic alcohol when a large quantity is not required.

III. "On the Distillation of Mixtures: a Contribution to the Theory of Fractional Distillation." By J. A. Wanklyn, Esq. Communicated by Dr. Frankland. Received April 17, 1863.

There are many points in the boiling of mixtures which are obscure. The tension of the vapours at the temperature whereat the mixture boils, and the proportions in which the constituents of the mixture are present, are not the only factors which determine the relative rates at which the constituents distil. There have, for instance, to be taken into account the adhesion of the liquids to one another, and the vapour-densities of these liquids. On the present occasion I have to call attention to the influence of this latter element, which influence seems to have been lost sight of by most of those who have applied themselves to this subject.

Leaving out of account for a moment the influence of adhesion, and simplifying the influence of the proportion in which the ingredients are present by taking equal weights of two liquids of different

boiling-points, we may set down the rates at which these ingredients will distil as determined by the tensions of the liquids and the densities of the vapours. In the first instant of time the quantity of each ingredient which distils will be found by multiplying its tension at the boiling-point of the mixture by its vapour-density. It thus appears that the liquid with the highest tension will not of necessity distil the quickest, for what the other liquids want in tension they may make up by the greater density of the vapours which they give off. And so when we mix a more volatile with a less volatile liquid and proceed to distil the mixture, we shall now and then find that the less volatile liquid distils faster than the more volatile one. I will here bring forward an experiment to illustrate this point.

	Vapour-density.
Methyl-alcohol boils at 66° C.	1·107
Iodide of ethyl boils at 72° C.	5·397

I took 18 grammes of methyl-alcohol and 17 grammes of iodide of ethyl, mixed them, and distilled off rather more than one-third of the mixture. The distillate consisted of

6·0 grammes methyl-alcohol,
8·7 grammes iodide of ethyl,
14·7

which shows that in this case the less volatile constituent had boiled the faster, the less volatile iodide of ethyl having a very much higher vapour-density than methyl-alcohol.

It will be obvious that when the vapour-densities and tensions are inversely proportional, the mixture must distil over unchanged. This influence of vapour-density goes a great way to explain why homologous bodies are so difficult of separation by means of fractional distillation. The more complex the formula the higher the boiling-point, but also the higher the vapour-density, and therefore the greater the value of the vapour. Why oils, &c. distil so readily in steam is also explained; for aqueous vapour is one of the lightest, while oily vapours are generally heavy.

IV. "On the Arrangement of the Muscular Fibres of the Ventricular Portion of the Vertebrate Heart; with Physiological Remarks." By JAMES PETTIGREW, M.D. Communicated by Professor GOODSIR. Received March 26, 1863.

This paper is a revised version of a previous one, having the same title, which was communicated to the Society on the 22nd of November, 1859, and formed the substance of the Croonian Lecture delivered by the author on the 19th of April, 1860.

The author has now included the results of his researches on the structure of the ventricular portion of the heart in fishes, reptiles, and birds.

April 30, 1863.

Major-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Professor Heinrich Gustav Magnus, of Berlin, was balloted for, and elected a Foreign Member of the Society.

The following communications were read:—

I. "On Spectrum Analysis; with a Description of a large Spectroscope having nine Prisms, and Achromatic Telescopes of two-feet focal power." By JOHN P. GASSIOT, F.R.S. Received April 21, 1863.

(Abstract.)

The author, after briefly alluding to the discoveries of Fox Talbot, Wheatstone, Foucault, Kirchhoff, and Bunsen, and the importance of spectrum analysis, states that among the numerous spectroscopes which were exhibited in the International Exhibition of 1862, there was one which had been specially constructed by Messrs. Spencer, Browning, and Co., philosophical instrument makers in London, which at the time excited considerable attention. This spectroscope had two prisms, with a magnifying power of 40, its definition being remarkably clear.

The skill evinced by Mr. Browning in the construction of this instrument induced the author to have one made in which still better effects might be produced, by multiplying the number of prisms and increasing the magnifying power, with the necessary precaution to avoid as much as possible loss of light. After a few preliminary trials, it was finally arranged to use nine prisms, which is the number that can be applied with this instrument, although the arrangements are such as to allow the whole or any less number to be used with the utmost facility.

Verniers and micrometer screws are attached to the knife-edges of the slit through which the light to be observed is admitted to the collimator and to the telescope, also to the large circle of the instrument; these enable the observer to note the exact position of the lines observed in the spectrum from whatever source it is obtained, and thus enable him to repeat and verify previous results with the utmost exactitude.

When two small prisms, one refracting and the other reflecting, are fixed outside the knife-edge slit, spectra obtained from three separate sources can be simultaneously examined; and an illuminated micrometer scale enables the observer to note the precise relative position of the lines in the three spectra without reference to or reading off from the verniers. By this arrangement a most interesting spectacle may be obtained, showing in the uppermost portion of the field of view the spectrum of thallium, strontium, or lithium, ignited in the flame of a Bunsen's gas-burner; in the centre of the field the spectrum of the same substance in the oxyhydrogen blowpipe, and at the bottom one in the voltaic arc; each successive spectrum there exhibits an increased number of lines.

With this spectroscope the author has ascertained that the green line of thallium, so celebrated for its integrity, and hitherto believed to coincide with one of the lines in the spectrum of baryta, does not so coincide; for by employing the nine prisms with a power of 80 on the telescope, the thallium line is clearly seen to occupy a dark space in the baryta spectrum, close by the side of the bright line with which it was supposed to coincide.

A range of prisms is adapted to the telescope, the highest of which, when used in conjunction with the amplifying lens, gives a power of 110 with good definition.

The author states that the results already obtained by this instrument have been so satisfactory as to leave him no cause to regret the time that has been devoted to, or the expense that has been incurred in the construction of this truly beautiful apparatus.

A full description of the instrument is introduced, with several diagrams showing the construction and adaptation of the different parts of the apparatus, and two drawings, one showing the general appearance of the instrument when prepared for observation, and the other representing it as seen when viewed from above.

II. The Bakerian Lecture.—"On the Direct Correlation of Mechanical and Chemical Forces." By Henry Clifton Sorby, F.R.S. Received April 29, 1863.

Perhaps it may be thought somewhat strange that a geologist should undertake such a subject as the correlation of forces; but the very fact of my being a geologist has led to the investigations of which I now purpose to give a short preliminary account. In studying general chemical and physical geology, and especially in examining the microscopical structure of rocks, I have for a number of years been greatly perplexed with a class of facts which pointed both to a mechanical and to a chemical origin. At first I attributed them either to a mechanical or a chemical action, or to the two combined; but in most cases no satisfactory explanation could be given. At length, however, facts turned up which altogether precluded any supposition not involving direct correlation; for they most clearly indicated that mechanical force had been resolved into chemical action in the same way as, under other circumstances, it may be resolved into heat, electricity, or any other modification of force, as so ably described by Grove in his work 'On the Correlation of Physical Forces.'

The effect of pressure on the solubility of salts has already been made the subject of speculation and experiment *, and a considerable number of facts have been described, showing that pressure will more

* Perkins, Ann. de Chim. et de Phys. vol. xxiii. p. 410. Sartorius von Waltershausen, Göttinger Studien, 1857. Bunsen, Ann. der Chem. und Pharm. 1848, vol. lxv. p. 70. Favre, Comptes Rendus, vol. li. p. 1027. Thomson, Proc. Roy. Soc. vol. xi. p. 473 (1861).

or less influence such chemical actions as are accompanied by an evolution of gas, so that it may cause a compound to be permanent which otherwise would be decomposed*; but the results were for the most part so indefinite and unconnected, or of such a character, that Mr. Grove does not allude to the *direct* production of chemical action from mechanical force. That this is, however, extremely probable will be evident to all who have considered the manner in which the various physical forces are correlated; for if mechanical force can be produced by chemical action, why should not the converse be true? In this paper I shall endeavour to show that such is really the fact, and that in some cases the mechanical equivalent of the chemical force may be determined.

In order to obtain the necessary great pressure, I have made use of a modification of the method employed by Bunsen; but instead of filling the tubes at the ordinary temperature of the atmosphere and then gently heating them for several hours, I in the first instance filled them at a temperature 10° or 20° C. lower, so that when finally sealed up they contained considerably more liquid than they could hold without pressure at the ordinary temperature of the atmosphere at the time being; and thus, by its tendency to expand, this liquid and anything enclosed in the tube were subjected to a very great pressure. By keeping the tubes in various parts of the house, according as the weather varied, I have been able to maintain for several weeks or even months a pressure of, for instance, about 100 atmospheres, as measured by means of a capillary-tube pressure-gauge enclosed within the larger tube. Since in all cases I had a second tube which from first to last was treated precisely like the other, pressure excepted, I have been able to determine the effect produced by the pressure with very considerable accuracy—at all events so as to leave no doubt whatever about the general facts. At the same time I wish it to be understood that the results described below must be looked upon only as approximations to the truth.

* Sir James Hall, Trans. Roy. Soc. Edinb. 1812, vol. vi. p. 71. Wöhler, Ann. der Chem. und Pharm. vol. xxxiii. p. 125. Babinet, Ann. de Chim. et de Phys. (2) vol. xxxvii. p. 183. Lothar Meyer, Pogg. Ann. vol. civ. p. 189. Beketoff, Comptes Rendus, vol. xlviii. p. 442. Gassiot, Brit. Assoc. Report, 1854, p. 39. Favre, Comptes Rendus, vol. li. p. 1027. Berthelot et Péan de Saint-Gilles, L'Institut, 1862, p. 257. Gmelin's Handbook of Chemistry, published by the Cavendish Society, vol. ii. p. 293.

I will first call attention to the well-known influence of pressure on the fusing-point of various substances, since it is a connecting link between well-established facts and those I am about to describe. Bunsen* and Hopkins† have shown that substances which expand when fused have their point of fusion raised by mechanical pressure; that is to say, since mechanical force must be overcome in melting, the tendency to melt must be increased by heat before that opposition can be overcome; and the pressure required to keep them solid at any temperature above their natural point of fusion may be looked upon as the mechanical representative of the force with which they tend to fuse at that temperature. Professor W. Thomson‡ has shown that, on the contrary, water, which expands in freezing, has its point of fusion lowered by pressure; that is to say, since mechanical force must be overcome in crystallizing, crystallization will not take place under increased pressure unless the force of crystalline polarity be increased by reducing the temperature. Thus, calculating from his experiments and from the known latent heat of ice, and assuming that no heat is gained or lost by contact with external objects, if we had 1 part of ice and 100 of water at 0° C., and then applied a pressure of 103 atmospheres, the ice would, as it were, dissolve in the water, the whole would become liquid, and the temperature be reduced to —·792° C.; or, in other terms, at that temperature the tendency to crystallize is exactly counterbalanced by that pressure.

Now I find that similar principles hold true with respect to the solubility of salts in water. If, when they dissolve, the total bulk increases, pressure reduces their solubility; whereas if the bulk decreases, pressure makes them more soluble; in other words, solution or crystallization is impeded by pressure according as mechanical force must be overcome in dissolving or in crystallizing.

Various authors have written on the volume with which salts enter into solution§; but since the subject before us requires a different

* Pogg. Ann. 1850, vol. lxxxi. p. 562.

† British Association Report, 1854, p. 57.

‡ Trans. Roy. Soc. Edinb. vol. xvi. p. 575.

§ Playfair, Chem. Soc. Quart. Journ. vol. i. p. 139. Michel and Krafft, Ann. de Chim. 2 sér. vol. xli. p. 471. Schiff, Ann. der Chemie, vol. cix. p. 325; vol. cxi. p. 68; vol. cxiii. p. 349. Gerlach, Specifische Gewichte der Salzlosungen, &c., 1859. Tissier, L'Institut, 1859, p. 158; 1860, p. 281. Kremers, Pogg. Ann. vol. lxxxv. pp. 37 and 246; vol. xciv. p. 87; vol. xcv. p. 110; vol. xcvi. p. 39; vol. cv. p. 360; vol. cviii. p. 115. Billet, Thèse de Chimie.

class of facts to be taken into account, I shall base my conclusions on my own experiments. The volume with which salts exist when in solution, assuming that of the water to remain unchanged, varies greatly in the case of different salts, and also according to the amount in solution and the temperature. Thus, taking sal-ammoniac as an example, when there are 3 per cent. in solution the volume is as if it expanded 3·40 per cent. on dissolving; whereas when 25·55 per cent. are in solution, the expansion is 11·36 per cent.; and when nearly concentrated at about 13° C., an additional quantity expands on dissolving 15·78 per cent. In by far the greater number of cases, however, there is a contraction on dissolving, and the amount gradually diminishes for each additional quantity entering into solution, so that the mean result is very different from what occurs when the solution is dilute or nearly saturated. It is this contraction or expansion when a small additional quantity is dissolved in a nearly concentrated solution that must be taken into account in the following calculations.

In determining the influence of pressure on the solution of salts, I found it requisite to adopt somewhat different methods according to the peculiarities of the salts. In some cases I sealed up in a saturated solution portions of the salt in clean, solid crystals, and determined the effect due to pressure from their loss in weight; whereas in other cases I sealed up solutions containing more salt than could be dissolved at the temperature at which the experiments were made, and determined the effect of pressure from the difference in the weight of the crystals deposited; being of course careful to make allowance for any difference in the amount of solution in the tube with pressure and in that without, and to avoid any error that might be produced by a different temperature. In all cases I have had a tube with pressure and another without, treated from first to last in precisely the same manner, and kept at exactly the same temperature, so that pressure was the only difference; and usually the effect was so well marked that there was no doubt about the result. In the case of chloride of sodium, solution goes on so slowly, and the mechanical equivalent of the force of crystallization is so great, that if pressure had been applied for only a few hours one might have concluded, with Bunsen, that pressure has no influence on solubility; but, by maintaining it for a week or more, there was no difficulty whatever

in perceiving that a solution which was quite saturated without pressure, dissolved more under a pressure of about 100 atmospheres.

The solubility of a salt in water appears to me to result from a kind of affinity which decreases in force as the amount of salt in solution increases. This affinity is opposed by the crystalline polarity of the salt; and when the two forces are equal, the solution is exactly saturated. As is well known, a change in temperature alters this equilibrium; and, according to my experiments, mechanical pressure relatively increases one or other of these opposing forces, according to the mechanical relations of the salt in dissolving. At all events in the case of chloride of sodium the extra quantity dissolved under pressure varies directly with it for such pressures as glass tubes will resist, in the same manner as, according to Thomson's experiments, the fusing-point of ice is reduced. Thus I found that for a pressure of $49\frac{1}{2}$ atmospheres the extra solubility was ·176 per cent., and for 121 atmospheres ·431, which are almost exactly in the same ratio. Hence, if S be the amount soluble without pressure, under a pressure of p atmospheres the solubility at the same temperature would be $S+ps$, where the values of S and s are independent, and vary for different temperatures and different salts. Future experiments may perhaps show that this conclusion should be modified; but yet it will be well to adopt it provisionally, in order to compare together the mechanical relations of different salts which otherwise would not be so intelligible.

According to Michel and Krafft* and to Schiff†, sal-ammoniac is the only salt known for certain to occupy more space in solution than when crystallized. Hence under pressure mechanical force must be overcome in dissolving, and experiment shows that, on this account, the relative force of crystalline polarity is increased and the solubility decreased. This is the reverse of what results from an elevation of the temperature, so that the effect cannot be due to heat generated by the pressure, but must be the direct consequent of pressure. Calculating from an experiment where the pressure was 164 atmospheres, which gave a decreased solubility of 1·045 per cent. of the whole salt in solution, a pressure of 100 atmospheres would cause ·637 per cent. less to be dissolved than is soluble at 20° C. without pressure, and the pressure requisite to reduce the solubility to the extent of 1 per cent.

* Ann. de Chim. 3 sér. vol. xli. p. 471.

† Ann. der Chemie, vol. cix. p. 325; vol. cxiii. p. 329.

would be 157 atmospheres. Expressing this fact in other words, we may say that a pressure of 157 atmospheres is the mechanical force with which the salt tends to dissolve in a solution containing 1 per cent. less than can dissolve at the same temperature without pressure, because the two forces exactly counterbalance one another. In a still more dilute solution the force would of course be still greater, in accordance with the fact of a greater pressure being necessary to prevent the salt from being dissolved. Supposing then that we had a solution a trifle more dilute than that just named, and in such indefinitely large quantity that a cubic inch of the salt could dissolve in it and yet produce no sensible change in its strength, so that from first to last it might be considered to dissolve under a pressure of 157 atmospheres, and also supposing that it was rigidly enclosed on all sides but one, so that the whole expansion must take place in one direction over an area of one square inch, since on dissolving there is an increase in bulk from 100 to 115·78, the solution of this cubic inch would, as it were, raise 2355 lbs. through the space of ·1578 inch. This is mechanically the same as $371\frac{1}{2}$ lbs. raised 1 foot, or, the specific gravity of the salt being 1·53, the same as 171 times the weight of the salt itself raised 1 metre. Since it involves no arbitrary unit but the metre, I shall adopt the last expression as the measure of the total amount of mechanical work done by the solution of salts which expand in dissolving, and which may conversely be looked upon as the measure of the mechanical force rendered latent and, as it were, expended in the act of crystallization when crystals are deposited. The value of this mechanical equivalent of course varies with the strength of the solution, as already remarked.

In the case of salts which occupy less space when dissolved than when solid, pressure, like the increased temperature, causes them to be more soluble; mechanical force is lost when they dissolve, and is, as it were, expended in giving rise to solution. When water thus containing more of a salt than could otherwise be dissolved at the same temperature is just saturated under any given pressure, the amount of pressure represents the force of crystalline polarity tending to cause the salt to be deposited in a crystalline form, but which is exactly counterbalanced by that pressure. I will not give the details for each salt, but subjoin a Table of the results at which I have arrived for such as illustrate particular points of interest, the calculations being all made

in accordance with the principles already described. I also give them in the case of water, calculated from Thomson's experiments, assuming that, when ice melts and mixes with water, it may be looked upon as dissolving in it; and, as will be seen, the mechanical force thus deduced is of the same general order of magnitude as that generated by the crystallization of salts.

	I.	II.	III.	IV.	V.
1. Chloride of Sodium....	13·57	97	·407	·419	157
2. Sulphate of Copper....	4·83	60	1·910	3·183	7
3. Ferridcyanide of Potassium..............	2·51	86	·288	·335	42
4. Sulphate of Potash....	31·21	63	1·840	2·914	42
5. Ferrocyanide of Potassium..............	8·90	66	1·640	2·485	20
6. Water..............	8·93	..	..	·991	106

Nos. 2 and 5 are calculated as hydrated crystals.

Column I. gives the expansion of each salt in crystallizing from a nearly saturated solution in water, the volume in a crystalline state being taken at 100.

Column II. gives the actual pressure in atmospheres in the experiment.

Column III. gives the increased solubility due to the pressure given in column II., the total amount of salt dissolved without pressure being taken at 100.

Column IV. gives the increase in solubility that would be produced by a pressure of 100 atmospheres, as calculated in accordance with the principles already described, the same unit being taken as in column III.

Column V. gives the value of the mechanical work that could be done, or, so to speak, the amount of mechanical force set free when the various substances crystallized from a solution containing 1 per cent. more than would be dissolved without pressure, as measured by the number of times its own weight which any unit of the various salts could raise to the height of 1 metre in the act of crystallization. Conversely, it is the amount of mechanical force which becomes latent in the act of solution; and in the case of a still more supersaturated solution it would be greater, and *vice versâ*, in accordance with the fact of the increased solubility varying with the pressure.

On comparing together the various salts, it will be seen that their properties vary very considerably. Thus, under the same pressure, the extra quantity of sulphate of copper dissolved in nearly ten times that of ferridcyanide of potassium. The mechanical equivalents also vary even more, being (for chloride of sodium) about $22\frac{1}{2}$ times as great as for sulphate of copper. On the contrary, the mechanical equivalents of ferridcyanide of potassium and sulphate of potash are the same; but, under equal pressures, the extra quantity of the latter dissolved in nearly nine times as great, owing to the difference in the amount of expansion in crystallizing. This latter is, however, nearly the same for water and ferrocyanide of potassium, whilst, under the same pressure, the extra quantity of that salt dissolved is $2\frac{1}{2}$ times that of ice, in consequence of the much greater mechanical equivalent of the ice. It appears to me that we may provisionally conclude that the increased solubility due to pressure varies directly with the change of volume, and inversely with the mechanical equivalent of the force of crystalline polarity, so that, if S be the total amount of salt which dissolves without pressure, c be some function of the change in volume in dissolving, and m some function of the mechanical equivalent of the force of crystalline polarity, the solubility, at the same temperature, under a pressure of p atmospheres would be $S+\frac{p \cdot c}{m}$. If the salt be one that expands on dissolving, c of course is negative, and therefore under pressure the solubility becomes $S-\frac{p \cdot c}{m}$; that is to say, it is diminished, as proved by experiments with sal-ammoniac. If no change in volume took place, we may, I think, also conclude that pressure would not in any way increase or decrease the solubility of a salt. Moreover, since, when a solution is just saturated, the force with which the salt tends to crystallize is equal to that with which it tends to dissolve, their mechanical equivalents must be equal and opposite. Hence we may perhaps conclude that, other circumstances being the same, the mechanical equivalent of a salt like chloride of sodium, which so readily attracts moisture, would be greater than that of one like sulphate of copper, which so readily loses even its water of crystallization; and thus also the relative influence of equal amounts of

pressure would be very different, as is confirmed by experiment in the case of these and some other salts.

The facts I have described, therefore, show that there is a direct correlation between mechanical force and the forces of crystallization and solution. According to some chemists, the latter is an instance of real combination; but, whatever views be entertained respecting its nature, we cannot, I think, deny that the force represents some modification of chemical affinity, or is at all events most closely allied to it. In comparison with some kinds of affinity, it may indeed be, and probably is, weak; but yet, as I have shown, it sometimes has a very considerable mechanical equivalent, even when nearly counterbalanced by an opposite force; and since such pressures as glass tubes will resist have no very great influence on what we may perhaps consider a weak affinity, we cannot expect that any pressure at our command would have much influence on strong affinities. I have, however, succeeded in obtaining some results which apparently show that pressure influences undoubtedly chemical changes taking place slowly, and therefore probably due to weak, or nearly counterbalanced, affinities.

The method adopted in this part of the inquiry was to seal up some solid substance in a solution which gives rise to a slow double decomposition, taking great care to have in the tube with pressure, and in that without, pieces cut so as to be of the same size and form, and a solution of the same character, so that, with the exception of pressure, all the conditions were the same. Possibly I may be so fortunate as to discover some case where the affinity is so weak that pressure may determine whether it go forward or not, of which fact the structure of metamorphic rocks furnishes examples; but hitherto I have only been able to prove that pressure modifies the *rate* at which chemical action takes place. This branch of the inquiry is, however, beset with many difficulties, for the change in volume produced by double decomposition is small, and its determination involves several complicated questions. The volume of the solids is easily determined; but that of the salts in solution is not the same when other salts are present as when they are dissolved in pure water, and varies much according to the strength of the solution and the nature of the salts; and many points are still so obscure, that I shall only give two cases by way of example.

When a portion of Witherite is enclosed in a tube with a strong solution of protochloride of iron, there is a slow decomposition into chloride of barium, which is dissolved, and carbonate of iron, which remains firmly attached to the Witherite, and would ultimately give rise to an excellent pseudomorph. The best conclusion at which I have been able to arrive is, that there is in this change an increase in volume equal to about 10·7 per cent. of the Witherite altered, so that, under pressure, mechanical force must be overcome. In an experiment where everything went on in a very satisfactory manner, the pressure was maintained for three months at from 80 to 100 atmospheres, and for one month was under 80 atmospheres, so that, on an average, it was about 80 atmospheres; and I found that the amount of chemical change was 21·7 per cent. less than when, all other circumstances having been the same, there had been no pressure; thus clearly showing that pressure had, as it were, diminished the force of chemical affinity. If then one cubic inch had been altered under this pressure, it would have overcome a mechanical force equal to that required to raise 1200 lbs. through the space of ·107 inch, which is equivalent to raising twenty-one times its own weight to the height of 1 metre; and under the same circumstances 1·278 cubic inch would have been altered when no such mechanical force had to be overcome. Supposing then that in both cases the total energy at work was the same, but in one was altogether expended in producing a chemical result, and in the other in producing partly a chemical and partly a mechanical effect, we may say that the force which gives rise to the purely chemical change, taking place at a particular rate, is equal to that which gives rise to this chemical effect, taking place at ·783 of that rate, and to a mechanical effect equal to the force required to raise in the same space of time 34·87 times the weight of the Witherite altered to the height of 1 metre. Supposing also that the power of chemical force varies as the rate at which it gives rise to a chemical change, in the same manner as the power of a mechanical force varies as the velocity of motion imparted by it, we may perhaps conclude that this mechanical force is equal to ·217 of the chemical force, and that the whole energy of the chemical action under the conditions of the experiment was equal to the mechanical power required to raise in the same period of time 160 times the weight of the Witherite altered to

the height of 1 metre. If these principles are correct, a pressure of more than 370 atmospheres would have entirely counterbalanced the force of chemical affinity, since to produce any chemical change it would then have had to overcome a greater force than it possessed. This is so great a pressure that I fear it will be difficult to prove the deduction by experiment; and until some such case can be found, capable of being verified, these calculations must be considered as little more than suggestions, which future investigations may confirm or disprove.

When calcite is sealed up in a mixed and rather strong solution of chloride of sodium and sulphate of copper, slow double decomposition gives rise to malachite, sulphate of lime, and carbonic acid; and though this case is extremely complicated, and it is very difficult to determine what would be the change in volume, yet, so far as I am able to make out, until the solution becomes saturated with sulphate of lime, there is a decrease in volume equal to about 8 per cent. of that of the calcite altered, so that, under pressure, mechanical force is the very reverse of being opposed to the chemical change. Three experiments, all indicating the same fact, and in which, on an average, the pressure was about 90 atmospheres for two weeks, show that, as a mean of the whole, the amount of chemical change was 17 per cent. more with the pressure than without; thus proving that pressure had, as it were, increased the force of chemical affinity. Calculating according to the principles described above, we may conclude that a pressure of 530 atmospheres would have caused the action to take place at double the rate, and that therefore the chemical action is equivalent to the expenditure of that amount of mechanical force, being thus generated by it. Arguing then in a manner similar to that already described, but modified to suit the different conditions, if there be a contraction equal to 8 per cent. of the bulk of the calcite, there must be a loss of mechanical force capable of raising 28 times the weight of the calcite altered to the height of 1 metre, in the time required for the chemical change; which amount of mechanical energy, as it were, becomes latent, and is transformed into chemical action, and would again exhibit itself as a mechanical force if, by any means, the chemical affinities could be inverted and everything restored to its original state.

In a like manner, other experiments indicate that in some cases

pressure causes a slower, and in others a quicker chemical action, whilst in others it has scarcely any influence whatever; and though, for reasons already explained, I say it with some hesitation, yet, bearing in mind what is already known respecting the action of pressure on hydrate of chlorine, hydrated hydrosulphuric acid, and other substances described by the various authors referred to in the notes, I think the facts I have described make it very probable that further research will show that pressure weakens or strengthens chemical affinity according as it acts against or in favour of the change in volume; as if chemical action were directly convertible into mechanical force, or mechanical force into chemical action, in definite equivalents, according to well-defined general laws, without its being necessary that they should be connected by means of heat or electricity. On the present occasion I shall not attempt to consider the various geological and mineralogical facts which appear to me to admit of the application of the principles I have described, for many of them are peculiarities in structure of which neither myself nor any one else has ever given a description, and would therefore demand a preliminary notice. However, I may say that it appears to me that a number of facts connected with metamorphic rocks and the phenomena of slaty cleavage, which, to me at all events, have hitherto been inexplicable, are readily explained if mechanical force be directly correlated to chemical action, and if in some cases the direction in which crystals are formed be more or less related to pressure, in some such way as there is a connexion between their structure and magnetic force, as shown by the experiments of Plücker, Faraday, Tyndall, and many other observers. We may also, I think, explain the origin of the impressions on the limestone pebbles in the "Nagelflue" in Switzerland, about which so much has been written in Germany and France, without a satisfactory reason having been discovered; and the same explanation accounts for the mutual penetration of the fragments of which some limestones are formed, and for the banded structure of some which possess slaty cleavage. The curious teeth-like projections with which one bed of limestone sometimes enters into another, also to a certain extent indicate a chemical action depending on mechanical force; and probably the same may be said of some of the peculiarities of slickensides and mineral veins. It is also possible that a pressure

of several hundred atmospheres may facilitate some of the chemical changes involved in the transformation of water and carbonic acid into the organic compounds met with in animals and plants of low organization found at great depths in the ocean, and thus to a certain extent compensate for diminished light. I, however, most willingly admit that very much remains to be learnt before we can say to what extent the principles I have described are applicable; and yet, at the same time, cannot but think that henceforth they must be taken into account in many departments of chemical and physical geology, and will readily explain a number of facts which otherwise would be very obscure.

May 7, 1863.

Major-General SABINE, President, in the Chair.

In accordance with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

Edward William Cooke, Esq., A.R.A.
William Crookes, Esq.
James Fergusson, Esq.
Frederick Field, Esq.
Rev. Robert Harley.
John Russell Hind, Esq.
Charles Watkins Merrifield, Esq.
Professor Daniel Oliver.
Frederick William Pavy, M.D.
William Pengelly, Esq.
Henry Enfield Roscoe, B.A.
Rev. George Salmon, D.D.
Samuel James Augustus Salter, M.B.
Rev. Arthur Penrhyn Stanley, D.D.
Colonel Frederick M. Eardley Wilmot, R.A.

The following communications were read:—

I. "On the Physiological Properties of Nitrobenzole and Aniline." By HENRY LETHEBY, M.B., F.L.S., &c., Professor of Chemistry, and late Professor of Toxicology in the Medical College of the London Hospital. Communicated by Dr. SHARPEY, Sec. R.S. Received April 23, 1863.

It is on record that Thrasyas, the father of Botany, was so skilled in the preparation of drugs, that he knew how to compound a poison

which would remain for days in the living body without manifesting its action, and would at last kill by a lingering illness. Theophrastus speaks of this poison, and says its force could be so modified as to occasion death in two, three, or six months, or even at the end of a year or two years. The writings of Plutarch, Tacitus, Quintilian, and Livy are full of instances of what seem to be this kind of slow and occult poisoning. In fact, until recently there has been a common belief among the unlearned that a skilful poisoner could so apportion the dose and combinations of certain subtle agents that he could destroy the life of his victim with certainty, and at the same time measure his allotted moments with the nicest precision, and defy the utmost skill of the physician and the chemist. Even so late as the 16th century this belief was shared by the learned of the medical profession; for we are told, in Sprat's 'History of the Royal Society,' that among other questions which were drawn up by the earlier Fellows to be submitted to the Chinese and Indians was, "Whether the Indians can so prepare that stupefying herb, *Datura*, that they make it lie several days, months, years, according as they will have it, in a man's body without doing him any hurt, and at the end kill him without missing half an hour's time?"

Modern toxicologists have long since discarded these notions, and have set them down to the vague fears and exaggerated fancies of the ancients, rather than to the sober contemplation of facts. But the account which I am about to give of the physiological properties of nitrobenzole will show that there is one substance, at least, which realizes to a great extent the extraordinary opinions of the ancients. This compound may be given today, and yet, if the dose be not too large, it shall not manifest its action until tomorrow, or the day after, and shall then destroy life by a lingering illness, which shall not only defy the skill of the physician, but shall also baffle the researches of the medical jurist. These facts are so remarkable, that they would be hardly credited if they were not susceptible of the proof of demonstration. They are likewise the more interesting and important from the circumstance that nitrobenzole is now a common article of commerce, and is accessible to everyone.

In every manufactory where nitrobenzole and aniline are prepared on a large scale, the peculiar narcotic effects of these poisons are often observed. The vapours escaping into the atmosphere are breathed

by the workmen, and cause distressing headache and a heavy, sleepy sensation. For the most part these effects are not serious, but are quickly relieved by fresh air and a mild stimulant, as a glass of brandy and water. Now and then, however, the workmen, from carelessness in their habits, expose themselves to the action of comparatively large quantities of these poisons, and then the effects are most dangerous. Two fatal cases of poisoning by nitrobenzole have been referred to me by the coroner for investigation during the last two years, and in both instances they were the results of careless manipulation. In one case a man, forty-three years of age, spilt a quantity of the liquid over the front of his clothes, and he went about for several hours in an atmosphere saturated with the poison. In the other a boy, aged seventeen years, received a little of the liquid into his mouth while sucking at a siphon. The effects were nearly the same in both cases, notwithstanding that in one the poison was inhaled, and in the other it was swallowed. For some time there was no feeling of discomfort beyond that of drowsiness; gradually, however, the face became flushed, the expression stupid, and the gait unsteady—the sufferers had the appearance of persons who had been drinking. Little by little this stupor increased, until it passed into profound coma, and in this condition they died. The progress of each case was much the same as that of slow intoxication, excepting that the mind was perfectly clear until the coming on of the fatal coma. This was sudden, like a fit of apoplexy; and from that moment there was no return of consciousness or of bodily power—the sufferer lay as if in a deep sleep, and died without a struggle. The duration of each case was nearly the same; about four hours elapsed from the time of taking or inhaling the poison to the setting in of the coma, and the coma lasted for about five hours.

After death there were no appearances of convulsion, but rather of narcotism and apoplexy. The face was flushed; the lips were livid; the superficial vessels of the body, especially about the throat and arms, were gorged with blood; the dependent parts were turgid; the blood was everywhere black and fluid; the lungs were somewhat congested; the cavities of the heart were full; the liver was of a purple colour, and the gall-bladder distended with bile; the brain and its membranes were turgid, and in the case of the man there was much bloody serosity in the ventricles. Analysis discovered the

existence of nitrobenzole in the brain and stomach, and also of aniline.

These effects were so remarkable, that I determined to examine them still further by experiments on domestic animals. Dogs and cats were submitted to the action of from thirty to sixty drops of nitrobenzole which had been well washed with dilute sulphuric acid and water to free it from every trace of aniline. The poison was generally administered by pouring it into the mouths of the animals, but sometimes it was given by means of an œsophagus-tube. When the nitrobenzole had come into contact with the mouth, it always caused discomfort, as if from unpleasant taste, and there was profuse salivation. Its local action on the stomach, however, was never very great, for there was rarely any vomiting until the setting in of nervou symptoms, and this seemed to be due to sympathy rather than to any local irritation of the stomach. Two classes of effects were clearly observed: there was either the rapid coma which characterized the operation of the poison on the human subject, or there was a slow setting in of paralysis and coma, after a long period of inaction.

When the effects were speedily fatal, the animal was soon seized with giddiness and an inability to walk. The weakness of the limbs first appeared in the hind extremities, and was manifested by a difficulty in standing; but very soon it extended to the fore legs, and then to the head and neck. There was complete loss of voluntary power. The animal lay upon its side, with its head drawn a little back, and with its limbs in constant motion, as if in the act of walking or running. The muscles of the back were occasionally fixed in spasm, and every now and then the animal would have a sort of epileptic fit. It would look distressed, would howl as if in pain, and would struggle violently. After this it would seem exhausted, and would lie powerless. The pupils were widely dilated, the action of the heart was tumultuous and irregular, and the breathing was somewhat difficult. For some time, however, the animal retained its consciousness, for it would look up, and wag its tail when spoken to; but suddenly, and often at the close of a fit, it would become comatose—the eye would remain open, but the conjunctiva would be insensible to touch, and the movements of the limbs would nearly cease; the breathing would be slow and somewhat stertorous, and the animal would appear as if

it were in a deep sleep. This condition would last until it died—the time of death varying from twenty-five minutes to twelve hours after the administration of the poison.

When the action of the poison was slower, there was often no visible effect for hours or days. At first there was always a little discomfort from the taste of the poison, but this soon subsided, and then for a day or more the animal appeared to be in perfect health. It would go about as usual, would be quite lively in its movements, would eat its food heartily, and in fact would seem to be in no way affected by the poison. Suddenly, however, it would look distressed, it would have an attack of vomiting, and it would tumble over in an epileptic fit. When this had subsided, it was generally found that the animal was weak, or even quite paralysed in its hind extremities; and after two or three of such attacks, the loss of voluntary power would extend to the fore limbs. The animal would lie upon its side perfectly helpless; and then the progress of the case was much the same as that already described, except that it was considerably slower. Consciousness, for example, would be retained for days after the animal was paralysed, and, although it was quite unable to stand, it would take food and drink when they were put into its mouth. The condition in which it lay was most distressing: the look was anxious and full of fear; the limbs were in constant motion; and every now and then there would be a violent struggle, as if the animal was in a fit, or was making fruitless efforts to rise. This would last for days, and then there would be either a gradual restoration of voluntary power with complete recovery, or death from exhaustion. The time that elapsed from the administration of the poison to the coming on of the first symptoms, namely the epileptic fit, varied from nineteen hours to seventy-two—in most cases it was about two days; and the time of death was from four to nine days.

The *post-mortem* appearances were nearly the same in all cases, whether the death was quick or slow. The vessels of the brain and its membranes were extremely turgid; the cavities of the heart were full of blood; the lungs were but slightly congested; the liver was of a deep purple tint, and the gall-bladder distended with bile; the stomach was natural, without sign of local irritation; and the blood all over the body was black and uncoagulated. Whenever the progress of the case had been quick, and death had taken place within twenty-four

hours, the odour of the nitrobenzole was clearly perceptible in the stomach, the brain, and the lungs; and there was always unmistakeable evidence of the existence of aniline in the organs of the body. In the slower cases the odour of the poison had often entirely disappeared; but generally there were distinct traces of aniline in the brain and urine, and sometimes in the stomach and liver; occasionally, however, no poison was found.

It has appeared to me that the facts which are here elucidated are very remarkable; for they not only indicate a rare circumstance in toxicology, namely, that a poison may be retained in the system for many days without showing its effects, but also that the poison may be changed into an entirely different substance. The importance of these facts cannot be overrated; they are alike interesting to the chemist, the physiologist, and the medical jurist; for, without dwelling on a very possible occurrence—namely, the criminal administration of this poison, with the knowledge that the effects would be delayed, that the symptoms would correspond to those of natural disease, that the progress of the case would be lingering, and that there would be either no discovery of poison in the body, or the discovery of a thing different from that administered—it will be manifest that the study of these facts by the medical jurist is of public importance. To the physiologist they are also interesting, insomuch as they indicate a reducing power in the animal body by the conversion of nitrobenzole into aniline. I have endeavoured to ascertain whether this is due to a living or a dead process. In the first place, I find that dead and decomposing organic matter will effect the change alluded to; for when nitrobenzole is placed in the dead stomach, or is kept in contact with putrid flesh for several hours, there is a partial reduction of it into aniline. This may be the source of the poison found in the dead body; but, on the other hand, there is a great similarity in the physiological effects of nitrobenzole and those of aniline.

When aniline is given to dogs and cats in doses of from twenty to sixty drops, it causes rapid loss of voluntary power. The animal staggers in its gait, looks perplexed, and falls upon its side powerless. Its head is drawn back, the pupils are widely dilated, there are slight twitchings or pasms of the muscles, the breathing is difficult, the action of the hear t is tumultuous, and the animal quickly passes into a state of coma. From this it never recovers, but remains upon

its side as if in a deep sleep, and so dies in from half an hour to thirty-two hours.

The *post-mortem* appearances are much the same as the last: the brain and its membranes are turgid, the cavities of the heart are nearly full of blood, the lungs are but slightly congested, and the blood all over the body is black and uncoagulated. In every case the poison was easily discovered in the brain, the stomach, and the liver.

While, however, there seems to be a probable conversion of nitrobenzole into aniline in the living animal body by a process of reduction, there is also undoubtedly a change of an opposite character going on upon the surface of the body, whereby the salts of aniline are oxidized and converted into *mauve* or *magenta* purple. Some remarkable facts illustrative of this have been brought under my own notice, and have been the subject of clinical observation.

In the month of June 1861, a boy aged 16 was brought into the London Hospital in a semi-comatose condition. He had been scrubbing out the inside of an aniline vat, and while so doing he breathed an atmosphere charged with the vapour of the alkali, and became insensible. He did not suffer pain or discomfort, but was suddenly seized with giddiness and insensibility. When he was brought to the hospital he looked like a person in the last stage of intoxication: the face and surface of the body were cold, the pulse was slow and almost imperceptible, the action of the heart was feeble, and the breathing was heavy and laborious. After rallying a little, he complained of pain in his head and giddiness. It was then noticed that the face had a purple hue, and that the lips and lining membrane of the mouth and the nails had the same purple tint. The next day, although the narcotic effects of the poison had passed away, he was still remarkably blue, like a patient in the last stage of cholera.

In the early part of last year, sulphate of aniline was given in rather large doses to patients in the London Hospital affected with chorea. The doses ranged from a quarter of a grain to seven grains. They were frequently administered, so that large quantities of the salt were taken in a very short time. In one case as much as 406 grains were given in the course of a few days. No very remarkable effects followed beyond this—that after a few doses had been taken, and the system had become, as it were, saturated with the salt, the

face became of a leaden blue colour, the lips and gums looked as if the patients had been eating black currants, and the nails also acquired a purple hue. The colour faded a little before the time came for the administration of another dose, but soon after taking it it appeared again; and this was the subject of constant observation. Dr. Fraser and Dr. Davies have recorded the results of their experience in five cases *, from which it would seem that, although the free alkali is a powerful poison, the sulphate of it has but little action upon the animal body.

The general conclusions which appear to me to be warranted by these investigations are:—

1st. That nitrobenzole and aniline in its free state are powerful narcotic poisons.

2nd. That they exert but little action, as local irritants, on the stomach and bowels.

3rd. That although the effects may be quick, and the fatal termination of them rapid, yet nitrobenzole may remain in the system for a long time without manifesting its action.

4th. That the salts of aniline are not nearly so poisonous as the free alkali.

5th. That in rapid cases of fatal poisoning, both the poisons are readily discovered in the dead body.

6th. That in slow cases the poisons may be entirely changed or eliminated, and therefore not recognizable.

7th. That both of the poisons appear to be changed in the body by processes of oxidation and reduction, nitrobenzole being changed into aniline, and aniline and its salts into mauve or magenta.

In an appendix † are given notes of the two cases of fatal poisoning by nitrobenzole referred to in the paper, and a detailed account of twelve experiments on animals with nitrobenzole, and three with aniline; also the process employed for the recognition of aniline and nitrobenzole in the dead body, as follows:—

1st. The matters to be analysed were bruised in a mortar with a little water, and very slightly acidulated with dilute sulphuric acid.

* Medical Times and Gazette, March 8th, 1862, p. 239.

† Preserved in the Archives.

2nd. They were then submitted to distillation in a glass retort—the distilled products being saved in three or four separate portions by changing the receiver at different stages of the process. In this way the presence of nitrobenzole was discovered.

3rd. The residue in the retort, when reduced to a pulpy mass by the distillation, was treated with strong spirit of wine and filtered.

4th. The filtered alcoholic solution which contained the aniline was treated with a slight excess of subacetate of lead, and again filtered. In this way gum, dextrine, &c. were removed.

5th. The filtered solution was treated with a slight excess of a saturated solution of sulphate of soda in water. In this manner the excess of lead was precipitated as a sulphate.

6th. The clear solution was then made very alkaline with caustic potash, and distilled to dryness from an oil-bath. The aniline, together with ammonia from the animal matters, was found in the clear, colourless, distilled spirit.

7th. This was neutralized, or rather made acid, with a slight excess of dilute sulphuric acid, and evaporated nearly to dryness in a white porcelain dish. If necessary, the spirit was saved by distillation.

8th. The residue was of a pinkish colour if aniline was present, and occasionally there were little streaks of blue around the edges of the white porcelain dish. If the quantity of the saline residue was not more than a grain or so, it was at once tested by dissolving it in a few drops, or even in a single drop, of dilute sulphuric acid (1 to 1). A small portion of it was then placed upon a strip of bright platinum; and the platinum having been connected with the positive pole of a single cell of a Grove's battery, the liquid was touched with the negative pole: in a few seconds, if aniline was present, the liquid would acquire a bronze, a blue, or a pink colour; the kind of colour being dependent on the amount of aniline present—bronze being the result of much aniline, and pink of a very little. In this way at least the $\frac{1}{2000}$th part of a grain of aniline was easily recognized.

To another portion of the acid liquid placed upon a white porcelain plate, a little peroxide of lead or red prussiate of potash was added, and a blue or purple reaction followed. This test is not so delicate as the last, for it fails when the amount of aniline is less than the $\frac{1}{1000}$th of a grain.

Other tests may be resorted to if necessary, as when the quantity

of aniline is large. Thus peroxide of manganese or bichromate of potash may be used in the same way as the red prussiate of potash in the last experiment; but these tests will not answer with less than the $\frac{1}{500}$th of a grain of aniline. Lastly, a drop of a solution of chloride of lime may be added to the acid liquid, and if the quantity of aniline exceeds the $\frac{1}{100}$th part of a grain it will cause a purple reaction.

9th. If the quantity of saline residue from the last operation is large, and there is reason to believe that much ammonia is present, this alkali must be got rid of, for it greatly interferes with the success of the colour-experiments. The residue, therefore, is made moist with water, and rubbed down with about twice its bulk of neutral carbonate of soda. It is then exposed to the air for a short time until the odour of ammonia has passed away. It is then treated with strong alcohol, filtered, acidulated with dilute sulphuric acid, and again evaporated. The aniline is now fit for the colour-experiments.

There are no fallacies to these experiments; for although, as I have elsewhere shown, strychnia will give nearly the same colour-reactions, yet in the first place this alkali is not volatile like aniline, and will not therefore distil over as the latter does; and in the next place, while the best effects, in respect of colour, are developed with dilute acid and aniline, strychnia requires the concentrated acid. These differences are sufficient to prevent any embarrassment as regards the two alkaloids.

II. "On the Immunity enjoyed by the Stomach from being digested by its own Secretion during Life." By FREDERICK W. PAVY, M.D. Communicated by Dr. SHARPEY, Sec. R.S. Received April 29, 1863.

(Abstract.)

The author stated that the opposition which his view on the above subject received the evening of its announcement, in his former communication read January 8, 1863, had induced him to perform a series of additional experiments. As from these experiments some important confirmatory evidence was supplied, he deemed it desirable to present a further communication to the Society on the subject.

He had again denuded the stomach of a patch of mucous mem-

brane, and in one experiment had allowed the animal to live for ten days. It had fed in an ordinary manner every day, and when killed, reparation was found in an advanced stage towards completion, the walls opposite the denuded part having been considerably thickened by new matter that had been thrown out.

Upon much further extended observation the author found that the standard he had taken from the rabbit, as regarded the *post-mortem* action of the contents of the stomach upon the organ itself, was not just in its application to the dog. Actual experiment on the dog had shown that upon the animal being killed at a period of full digestion, and its temperature being afterwards maintained about the degree belonging to life, the effect at the end of five and six hours only amounted to more or less digestion of the mucous membrane. In the rabbit, under similar circumstances, the effect had gone on to perforation, on account, apparently, of the stronger acidity of the gastric contents. In reality, then, the effect of arresting the circulation through the stomach during life, about coincided in both rabbit and dog with what occurred, other circumstances being equal, after death.

As a counterpart to the experiments originally mentioned, where dilute non-corrosive acids had been introduced into the stomach of the dog, and the flow of blood through the organ afterwards arrested, an operation that was followed by comparatively rapid perforation, the author had used the same acids, in the same quantities, and similarly diluted, but the circulation was allowed to remain free, and now the stomach resisted digestive attack. Ligatures had of course been applied to secure the retention of the acid liquid introduced.

A mode of experimenting suggested by Dr. Sharpey had been undertaken. After an incision through the anterior wall of the stomach, a portion of the posterior wall had been drawn forward, and a ligature placed tightly around it, so as thus to arrest the circulation through a limited portion of the organ's parietes. It was found that this constricted mass underwent digestion like a morsel of food.

An experiment had been performed bearing on the explanation that had been given to account for the attack upon the living frog's legs and rabbit's ear by digestion whilst the stomach remained protected. Three drachms of muriatic acid, diluted to three ounces with water, were introduced into the stomach of a dog, and the end

of the œsophagus and the pylorus ligatured, without including the vessels, so that the circulation through the organ was left free. In one hour and forty minutes death took place, and on the parts being examined immediately, perforation, with extensive digestion of the interior of the stomach throughout, was found. The author considered that the question of result was clearly shown to resolve itself into one dependent on degree of power possessed by the acid contents of the stomach on the one hand, as against the alkaline circulation on the other. With a certain amount of acid only in the stomach, the circulation can afford the required protection; whilst with a larger amount the influence of the acid prevails, and digestive solution of the organ is the result. Allow, now, the contents of the stomach to remain the same, and vary the degree of vascularity in the parts submitted to the digestive influence. We have simply here a converse arrangement of the circumstances; and the position is represented by the situation of the stomach as compared with that of the frog's legs and rabbit's ear.

III. "On a Question of Compound Arrangement." By J. J. Sylvester, M.A., F.R.S., Professor of Mathematics in the Royal Military Academy, Woolwich. Received April 27, 1863.

My successful but as yet unpublished researches into the Theory of Double Determinants have involved the consideration of the following curious case of arrangements.

There are given $m+n-1$ counters of n distinct *colours* just capable of being packed into m *urns*. The question refers to the distribution of the counters among the urns, subject to the condition that it shall *not* be possible to form a closed circuit of double colours between any number of the urns chosen arbitrarily, *ex. gr.* we must allow no distribution of counters in which one urn contains blue and yellow, a second yellow and red, a third red and green, and a fourth green and blue, because here *blue, yellow, red,* and *green* would form a closed circuit. This condition, it is evident, excludes the same combination of colours from existing in any two of the urns, and also the repetition of any one colour in the same urn. Any distribution of counters obeying this condition may be called an excyclic distribution.

I annex two propositions, one qualitative, the other quantitative, referring to such distributions.

Qualitative Theorem.

In any excyclic distribution between m *urns of* m+n−1 *counters of* n *different colours, any set of counters selected at will must be fewer in number than the number of distinct colours which they contain added to the number of urns from which they are drawn.*

Before going on to enunciate the second proposition I must premise one or two simple definitions.

The *capacity* of an urn means the number of counters it will contain, the *frequency* of a colour the number of counters of that colour, so that the sum of all the capacities and the sum of all the frequencies must be each equal to the number of the counters.

Again, by the *diminished* capacity of any urn or *diminished* frequency of any colour, I mean such capacity or frequency respectively diminished by *unity*.

Finally, by the *polynomial function* of any set of numbers $a, b, \ldots l$, I mean the coefficient of $x^a . y^b \ldots z^l$ in the expansion of

$$(x+y+\ldots+z)^{a+b+\ldots+l}.$$

I can now enunciate the following

Quantitative Theorem.

The number of modes of excyclic distribution between m *urns of* m+n−1 *counters of* n *different colours is equal to the product of the polynomial function of the* diminished frequencies *of all the several colours multiplied by the polynomial function of the* diminished capacities *of all the several urns.*

Observation.

A double determinant means the resultant of a system of $(m+n-1)$ homogeneous equations each containing mn terms, and linear in respect to each of two systems of m and n variables taken separately, but of the second order in respect to the variables of these two systems taken collectively. Any such resultant is of the degree $\frac{\pi(m+n-1)}{\pi(m-1)\pi(n-1)}$ in respect of the given coefficients, and may be represented by an ordinary determinant of the $(m+n-1)$th order, every one of whose terms corresponds to a particular system of capacities of the m urns and of repetitions of the n colours in the question above treated.

The total number of such systems or terms will be

$$\left\{\frac{\pi(m+n-2)}{\pi(m-1)\pi(n-1)}\right\}^2.$$

Every term in this determinant will itself be a sum of simple determinants of the $(m+n-1)$th order, corresponding (each to each) with the totality of the excylcic distributions of $(m+n-1)$ counters in respect of the particular systems of m capacities and n frequencies appertaining to that term; so that the number of simple determinants whose sum constitutes a term in the grand total determinant is always the product of two polynomial coefficients. In the particular case, where one of the systems contains only *two* variables, one of these polynomial coefficients becomes unity, and the other sinks down to a binomial coefficient. The only instance of a double determinant which is believed to have been considered up to the present moment is that given by Mr. Cayley in the 'Cambridge and Dublin Mathematical Journal,' vol. ix. 1854, for the case of $m=2$, $n=2$.

IV. "On a Theorem relating to Polar Umbræ." By J. J. Sylvester, M.A., F.R.S. Received April 27, 1863.

By polar umbræ I mean such as obey in the strictest manner the polar law of sign, so that not only any two appositions or products of such umbræ derivable from one another by an interchange of two of their elements are to be considered each as the negative of the other, but also any such apposition or product becomes zero if the same element is found in it more than once.

Thus Sir W. Hamilton's i, j, k are not polar umbræ, because although $ijk=-jik=kij$, &c., ii, jj, kk, instead of being *nulls*, are in the Calculus of Quaternions taken as *unities**.

Let us now define any set arranged either in line or column of such *umbral* quantities to be multiplied by a corresponding set of *actual* quantities when each term of the one set is multiplied by the corresponding one of the other, and the sum taken of the products so

* If we use Vandermonde's condensed notation for a determinant $\begin{bmatrix}1 & 2 & \ldots & n\\ 1 & 2 & \ldots & n\end{bmatrix}$ to represent a "determinant gauche," then, since on this supposition $rs=sr$ and $rr=0$, 1, 2, 3, . . . n will be polar umbræ by definition.

obtained as in the ordinary case of the multiplication of the lines or columns of two determinants *inter se*. Thus, *ex. gr.* $(a,\ b,\ c \rangle\!\langle x, y, z)$, as also $\left(\begin{smallmatrix} a \\ b \end{smallmatrix} \rangle\!\langle \begin{smallmatrix} x \\ y \\ z \end{smallmatrix}\right)$ is to mean the same product, viz.

$$ax+by+cz.$$

Again, imagine a rectangular (square or oblong) matrix of polar umbræ, and that each line thereof is multiplied by the same line of *actual* quantities, the product of the products so obtained I call a Factorial of the Matrix. I also call the product similarly obtained when the columns of the matrix are substituted for the lines, a Factorial of the same, but distinguish between the two by giving to one the name of a Transverse, the second of a Longitudinal Factorial of the matrix. We are now in a position to enunciate the following remarkable theorem :—

The product of any longitudinal by any transverse factorial of the same polar umbral matrix is identically zero.

Ex. gr. Let $\begin{vmatrix} a & b & c \\ d & e & f \end{vmatrix}$ be a matrix of polar umbræ, but x, y, z and also ξ, η actual quantities. Then

$$(ax+by+cz)(dx+ey+fz)$$

is a transverse factorial,

$$(a\xi+d\eta)(b\xi+e\eta)(c\xi+f\eta)$$

a longitudinal factorial of the above matrix, and by the theorem their product should be zero. This is easily verified.

The two *factorials* expanded are respectively

$$adx^2+bey^2+cfz^2+(ae+bd)xy+(bf+ce)yz+(af+dc)zx,$$
$$abc\xi^3+(abf+aec+dbc)\xi^2\eta+(dec+dbf+aef)\xi\eta^2+def\eta^3\,;$$

in their product the coefficient of

$$\begin{aligned} x^2\xi^3 &= abcad=0, \\ xy\xi^3 &= abcae+abcbd=0, \\ x^2\xi^2\eta &= abfad+aecad+dbcad=0, \\ xy\xi^2\eta &= abfae+abfbd+aecae+aecbd+dbcae+dbcdb \\ &= aecbd+dbcae=aecbd-aecbd=0, \end{aligned}$$

and so for all the other terms.

This is the fundamental theorem by aid of which I obtain the

resultant of a lineo-linear system of equations in its most perfect form. It is easy to obtain two different solutions, each of them unsymmetrical in respect of the data of the question; the conversion and fusion of each of these into one and the same determinant, symmetrical in all its relations to the data, is effected instantaneously by a process derived from the above theorem. In that particular application of it, the umbræ involved each represent columns of actual quantities in number equal to the number of places in the width and length of the umbral matrix to which they belong, so that each coefficient in the product of a lateral by a longitudinal factorial represents an ordinary determinant made up of these columns, from which it is evident that the polar law of sign and nullity necessary for the truth of the theorem are satisfied in the case supposed.

V. "Notes, principally on Thermo-electric Currents of the Ritterian Species." By C. K. Akin, Esq. Communicated by Professor Stokes, Sec. R.S. Received March 26, 1863.

(Abstract.)

The electromotive force of a thermo-electric couple is a function of the nature of the metals of which it is composed, and of the temperatures of the junctions. It is expressed in this paper by

$$[x, y]_t^{\mathrm{T}},$$

where x and y are names of metals, and T and t are temperatures. In this notation Becquerel's two laws become

$$[a, b]_{t'}^{t''}=[a, b]_t^{t''}-[a, b]_t^{t'}; \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

and

$$(a, c)_t^{\mathrm{T}}=[a, b]_t^{\mathrm{T}}+[b, c]_t^{\mathrm{T}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

From (I.) we learn that the electromotive force of a couple may be expressed as the difference of two quantities which are functions of the temperature and of the nature of the circuit, or

$$[x, y]_t^{\mathrm{T}}=[x, y]_{\mathrm{T}}-[x, y]_t. \quad . \quad . \quad . \quad . \quad \text{(III.)}$$

From (II.) we learn that any number of metals with their ends at the same temperature may be introduced without effect, or

$$[a, b]_t+[b, c]_t=[a, c]_t. \quad \ldots\ldots \quad \text{(IV.)}$$

This equation will always be true if

$$[x, y]_t=[x]_t-[y]_t, \quad \ldots\ldots\ldots \quad \text{(V.)}$$

whence we may write (III.)

$$[x, y]_t^{\mathrm{T}}=[x]_t-[y]_t-[x]_{\mathrm{T}}+[y]_{\mathrm{T}};$$

or, in other words, the electromotive force of a couple may be considered as the difference of the electromotive force of two metals, each of which is found by subtracting its tension at the higher temperature from that of the lower one.

Everything therefore depends on a knowledge of the value of what may be called the electric tension of each metal at the various temperatures. This for every metal is a function of temperature, and may be called, in the language of the paper, a function of the nature (or name) of the metal and the temperature.

(The nature of the metal may be altered otherwise than chemically.)

If the temperature of the metal vary in any way throughout its length, then if it be homogeneous, the electromotive force will depend only on the temperatures of its extremities.

In a circuit of one metal, the author considers that at the junction of the ends there may be a real discontinuity of temperature while there is a continuity of electric current. He regards the explanation of the effect by the stratum of air between the unequally heated ends to be unsatisfactory. Mercury, as is known, will not produce thermo-currents in this way. The author considers that the texture, &c., as well as the chemical nature of the substance, influences the value of the thermo-electric function. He also shows the possibility of the thermo-electric inversions first discovered by Professor Cumming.

May 21, 1863.

Major-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Nature of the Sun's Magnetic Action upon the Earth." By CHARLES CHAMBERS, Esq. Communicated by the President. Received April 30, 1863.

(Abstract.)

If the sun were a magnet of sufficient power to exert a sensible attraction upon a small magnet at the distance of the earth, it would have a real influence on the earth by inducing magnetism in its soft iron, and an apparent one due to the direct action of the sun upon the magnets used for measuring the earth's variations of force. As the earth rotates upon its axis, producing a varying relation, as to position, of the place of observation with respect to the sun, a diurnal variation will thus be produced in the forces which act upon the magnetometers, which variation is shown to follow the simple law $x = A\sin(h+\alpha)$, x being the deviation of the magnet from its normal position, h the hour-angle of the sun (and for a single day), A a constant coefficient, and α a constant angle. A comparison of this result with the laws of the observed diurnal variations shows that direct and inducing action of the sun is not the sole cause of the variations.

An endeavour is then made to prove that if any part of the observed diurnal variations is due to this cause, it is small in comparison with that produced by other forces in operation. This is done by separating from the observed variations the part of them which obeys the law $x' = B\sin(h+\beta)$, and comparing the variations in the values of B and β from month to month with those of A and α, when it is seen that the former obey a law which has but little similarity to the law of variation of the latter.

II. "Numerical Elements of Indian Meteorology."—Series I. By Dr. HERMANN DE SCHLAGINTWEIT, Corr. Memb. of the Academies of Sciences of Munich, Madrid, Lisbon, &c. Communicated by the President. Received May 4, 1863.

(Abstract.)

In this paper the author communicates Plates in which the iso-

thermal lines are represented between the latitudes of 5° N. and 36° W., and longitudes of 78° E. and 98° E. of Greenwich.

1st, of the mean temperature of the year;

2nd, of the cool season, viz. December, January, and February;

3rd, of the hot season, viz. March, April, and May;

4th, of the rainy season, viz. June, July, and August;

5th, of the autumn, viz. September, October, and November.

The memoir which accompanies the Plates contains a statement of the data on which the isothermal lines are founded.

These are: 1. Meteorological researches made by the author and his brothers at various stations in India and the Indian Archipelago during the years 1854–1858.

2. The original manuscripts in thirty-nine folio volumes of meteorological observations made by various observers under the authority of the Indian Government at 207 stations in British India. In regard to the observations referred to under this head, the author considers that he possesses a special qualification for using them advantageously by having himself visited most of the stations, examined the instruments, and acquainted himself with the circumstances of their employment.

The 207 stations are divided into ten geographical groups, as follows:—

1.	Eastern India: 1, Assam; 2, Kharsia Hills..........	12
2.	Bengal and Bahar, and Delta of the Ganges and Brahmapútra	36
3.	Hindostan, the upper Gangetic plain.................	27
4.	Panjáb, including the stations west of the Indus	24
5.	Western India: Rajvára, Guzrăt, Kăch, Sindh........	10
6.	Central India: Berár, Orissa, Málva, Bandelkhănd....	15
7.	1, Southern India: hilly districts, Dékhan and Maissúr; 2, Nílgiris	29
8.	Southern India, coasts: Kónhan, Malabar, Karnátik ..	24
9.	Ceylon	
10.	Indo-Chinese Peninsula, Archipelago, and China	20

Each group has its appropriate processes of reduction, which are severally discussed.

III. "On the Structure of the so-called Apolar, Unipolar, and Bipolar Nerve-cells of the Frog." By LIONEL BEALE, M.B., F.R.S., F.R.C.P., Professor of Physiology and of General and Morbid Anatomy in King's College, London, and Physician to King's College Hospital. Received May 7, 1863.

(Abstract.)

The author adverts to the opinion generally received with regard to the existence of *apolar, unipolar, bipolar,* and *multipolar* nerve-cells, and observes that if cells having such very different relations to the nerve-fibres they are supposed to influence, as apolar, unipolar, and multipolar cells, do actually exist, as many different kinds of action must be admitted. For it is hardly likely that a nerve-cell unconnected with any fibre can affect the fibres at a distance from it in the same way as a cell acts upon fibres which are in structural continuity with it. Neither is it probable that a cell with but one fibre proceeding from it can constitute an organ which acts upon the same principle as the cell from which two or more fibres proceed. If no fibre, or but one fibre proceeds from certain cells, the formation of complete nervous circuits, at least in these instances, is impossible; and if it be admitted that circuits do not exist in every case, a strong argument is advanced against the existence of such complete circuits as a necessary or fundamental condition of a complete nervous apparatus. But if it can be shown, on the other hand, as the author maintains is the case, that all the supposed apolar and unipolar cells have at least two fibres proceeding from them, the fact must be accepted in favour of the view that such complete circuits may exist, while the fact that the fibres connected with many cells have been seen to proceed in opposite directions some distance after leaving the cell, is a very strong argument in favour of such general inference, and at the same time an explanation of many arrangements which are observed constantly in connexion with nerve-fibres in various tissues.

Many observers have described *apolar* and *unipolar* cells in ganglia in different parts of the frog. The author, on the other hand, has failed to discover any apolar or unipolar cells in this or in any other animal, and considers that the apparent absence of fibres, and the presence of one fibre only in connexion with a cell, result from the defective modes of preparation generally employed. He maintains

that every nerve-cell, *central* or *peripheral*, has at least two fibres proceeding from it*. In many cases he has demonstrated that these fibres pursue opposite directions, and he considers that such an arrangement is general, and therefore necessary. The author considers himself justified in drawing the following conclusions from observations he has made during the last three years.

1st. That in all cases nerve-fibres are in bodily connexion with the cell or cells which influence them, and this from the earliest period of their formation.

2nd. That there are no apolar cells, and no unipolar cells, in any part of any nervous system.

3rd. That every nerve-cell, central or peripheral, has at least two fibres in connexion with it.

Though the present inquiry is limited to the structure of the particular cells connected with the ganglia in different parts of the frog, the author has studied the arrangement of nerve-cells and nerve-firbes in nervous centres, as well as at their peripheral distribution, in many different animals.

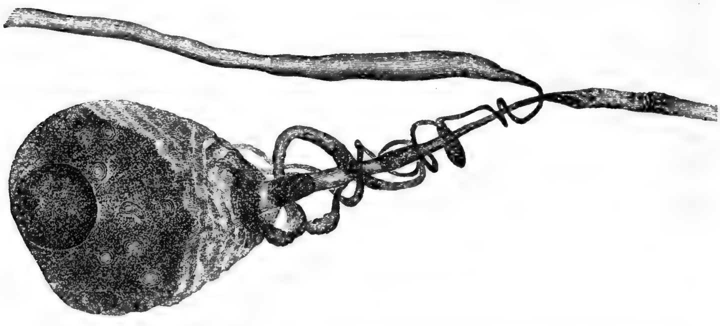
1. *General description of the ganglion-cells connected with the sympathetic and other nerves of the frog.*

The general form of these cells is oval or spherical; but the most perfectly formed ganglion-cell is more or less pear- or balloon-shaped in its general outline, and by its narrow extremity is continuous with nerve-fibres which may be followed into trunks.

The figure represents a well-formed ganglion-cell from a ganglion close to one of the large lumbar nerves of the little green tree-frog (*Hyla arborea*). The substance of the cell consists of a more or less granular material, which by the slow action of acetic acid becomes

* The word "cell" is only used in a general sense, as being shorter and more convenient than "elementary part." It consists merely of, 1st, matter in a living active state (*germinal matter*), and 2nd, matter resulting from changes occurring in this (*formed material*). In the figure, what is ordinarily termed "nucleus" and "nucleolus" consists of germinal or living matter, while the matter at the lower part of the cell and the nerve-fibres are formed material. A nerve-fibre cannot produce a new nerve-fibre, but the "nucleus" or germinal matter of a nerve-fibre can produce new nerve-fibre. The formed matter never produces matter like itself. Germinal matter can produce matter like itself, and from this formed material may result.

decomposed, oil-globules being gradually set free. Near the fundus or rounded end is seen the very large circular nucleus with its nucleolus. In some of these cells, at about the central part or a little higher, are a number of oval nuclei, some of which are in connexion with fibres. The matter of which the mass of the cell consists gradually diminishes in diameter, and contracts so as to form a fibre, in which a nucleus is often seen. At the circumference of the cell, about its middle, the material seems gradually to assume the form of fibres, which contain numerous nuclei, and these pass around



So-called "unipolar" nerve-cell, with, 1st, a *straight*, and 2nd, a *spiral* fibre emanating from it. The fibres continuous with these are seen to pursue opposite directions. Magnified 700 linear.

1000th of an inch × 700.

$\frac{1}{10000}$

the first fibre in a spiral manner. Thus in the *fully formed cell a fibre comes from the centre of the cell* (*straight fibre*), *and one or more fibres* (*spiral fibres*) *proceed from its surface.* These points are represented in the figure*.

2. *On the formation of ganglion-cells in the fully formed frog.*

This subject is arranged under the three following heads, but as it would not be intelligible without figures, it will not be given in abstract. The development of these cells and many other structures may be studied in the fully formed animal as well as in the embryo.

a. Ganglion-cells developed from a nucleated granular mass like that which forms the early condition of all tissues.

b. Ganglion-cells formed by the division or splitting up of a mass like a single ganglion-cell.

* The specimen from which this drawing was taken has been seen by many observers.

c. Ganglion-cells formed by changes occurring in what appears to be the nucleus of a nerve-fibre.

3. *Further changes in the ganglion-cell after its formation.*

Under this head the movement of the cell from the point where its growth commenced is described. It is shown that the two fibres, which at first seem to come from opposite extremities of the cell, lie parallel to each other. They increase in length, and subsequently one is seen to be twisted round the other, as shown in the figure. Sometimes the fibres below the point where the spiral arrangement exists run parallel for a long distance, but at length pursue opposite directions. The author considers that the formation of the ganglion-cell commenced at the point where the fibres diverge, and that subsequently the cell moved away,—the parallel fibres, which at length become straight and spiral, being gradually formed or drawn out as it were from the cell.

4. *Of the spiral fibre of the fully formed ganglion-cell.*

The spiral fibre or fibres can be shown to be continuous with the material of which the body of the cell is composed, as well as the straight fibre, but the former are connected with its surface, while the latter proceeds from the deeper and more central part of its substance.

There are many nuclei in connexion with the spiral fibre, and several nuclei of the same character imbedded in the substance of the mass of which the cell is composed. These latter nuclei seem to be connected with an earlier condition of the matter which becomes, when more condensed, spiral fibre. A great difference is observed with regard to the extent of the spiral fibre in cells of different ages. In the youngest cells the fibres near the cell are both parallel to each other, but as the cell grows one is seen to be coiled round the other; and the number of coils increases as the cell advances in age, while the matter of which the fundus of the cell is composed gradually becomes less—apparently in consequence of undergoing conversion into fibres. Nuclei are found in the course of the straight fibre, as well as in connexion with the spiral fibre. Nuclei have been demonstrated in connexion with the dark-bordered fibres near their origin and near their distribution in all tissues.

Next follows a discussion " on the essential nature of the changes

occurring during the formation of all nerve-cells, and on the formation of spiral fibres," but this is not adapted for an abstract. The term "nucleus" is only employed in a general sense. The author believes that the "nucleus," "nucleolus," and centres within the latter ("nucleoluli") merely represent centres of different ages. He considers that the matter of the nucleus becomes gradually transformed into the formed matter around it, and generally, that these bodies are merely centres which arise in pre-existing centres. He maintains that from the outer formed matter connected with the fibres new nerve-cells could not be produced, while he holds that from the nuclei, nucleoli, and contained centres, entirely new and complete cells could be evolved. So he considers that the difference in the properties and powers of the formed matter on the one hand, and the nuclei and nucleoli on the other, depends upon these two kinds of matter having arrived at different stages of existence. That which is formed cannot form new formed matter, nor appropriate nutrient material; but the living germinal matter of the nucleus can be resolved into formed matter, and it can appropriate inanimate pabulum, and confer upon it the same wonderful (vital) powers which it possesses itself, and which were communicated to it from pre-existing germinal matter.

7. *Of the fibres in the nerve-trunks continuous with the straight and spiral fibres of the ganglion-cells.*

The conclusions upon this important question are as follows:—

1st. That in some instances very fine fibres, not more than the $\frac{1}{60,000}$th of an English inch in diameter, are alone continuous with both straight and spiral fibres of the ganglion-cell.

2nd. That a dark-bordered fibre may be traced to the ganglion-cell as the *straight fibre*, while the spiral fibres are continued on as very fine fibres.

3rd. That the spiral fibres may be continued onwards as a dark-bordered fibre which may even be *wider*, at least for some distance, than the fibre continued from the straight fibre.

4th. That both straight and spiral fibres may be continuous with dark-bordered fibres.

It is therefore quite certain that the spiral fibre is not connective tissue, although the author considers it probable that many German

observers may adopt this view until they have an opportunity of seeing the fibres themselves.

8. *Of the ganglion-cells of the heart.*

The author's conclusions are quite opposed to those of Kölliker, who states that all the cells are *unipolar*, and that the fibre always passes in a peripheral direction, also that the transcurrent fibres of the vagus have no connexion with these cells. The author, on the contrary, affirms that the cells have at least *two fibres coming from them*, that some of the fibres pass towards the heart, and others towards the brain. He regards it as very probable that many at least of these ganglion-cells are connected with fibres of the vagus. Kölliker has also stated (1860) that many apolar cells could be seen in the heart, ganglia, and in the bladder. The author has been able to demonstrate fibres in connexion with so many cells which appeared devoid of fibres, that he considers himself justified in denying the existence of apolar and unipolar cells altogether.

Next follow some observations on "the ganglion-cells and nerves of arteries;" "on the connexion of the ganglion-cells with each other;" and the paper concludes with a description of the so-called "capsule" of the ganglion-cell, and a discussion on the nature and formation of the connective tissue and its corpuscles in the immediate neighbourhood of nerve-fibres.

The paper is illustrated with forty-seven drawings of the specimens, magnified from 700 to 1700 linear; and the author states that many of the specimens will probably retain the appearances he has copied for several months. All the preparations have been made in the same manner. An outline of the process has been already given in the author's previous communications, but the author is aware that it may be some time before the correctness of his conclusions is generally admitted, in consequence of the difficulty of preparing demonstrative specimens.

IV. "On the Belts of Jupiter," in a Note addressed to the Secretary. By JOHN PHILLIPS, M.A., LL.D., Professor of Geology, Oxford. Received May 5, 1863.

Oxford, 4th May, 1863.

DEAR SIR,—The favourable position of Jupiter for scrutiny of his physical features may perhaps have already brought to the Royal Society some notice of the aspect of his belts. Whether that be so or not, I think you will readily excuse the desire I feel to lay before the Society a sketch from my equatorial, which shows the *colours* of several celestial objects more distinctly than I am accustomed to hear is the case with some other instruments of the achromatic class.

The sketch shows the usual equatorial bands*, or rather bands nearly in the usual latitudes north and south of the equator. These, to the eyes of my friends and to mine, appear not dark grey, or greyish brown, or brown, but nearly of the colour of some ochraceous sands, or the yellower parts of what is called "red" deal. Several friends to whom I have shown the planet have immediately exclaimed, "how red the bands are;" "never saw them so red before."

The bands far from the equator are not reddened, but of a grey tint a little warmed. The space between the equatorial bands, sometimes described as yellow, appears rather bright white and silvery—much the brightest part of the surface. The outer borders of the equatorial bands are not parallel, the inner borders much unequal; in one part the two bands are connected across.

Not the faintest trace of such a tint as that conspicuous in these bands appears on any part of the moon; but it is pretty nearly the tint of the supposed "land" of Mars. In fact, it was suggested to my mind that these coloured extra-equatorial belts were land, seen between white clouds, of which the brightest band was on the equator.

JOHN PHILLIPS, M.A., LL.D., F.R.S.,
Professor of Geology, Oxford.

* The coloured drawing sent with this letter is preserved at the Royal Society.

V. "Notes of Researches on the Polyammonias, No. XXIII.—Hydrazobenzol, a new Compound isomeric with Benzidine." By A. W. Hofmann, LL.D., F.R.S., &c. Received May 7, 1863.

The discovery, among the secondary products of the manufacture of aniline, of xenylamine, the probable connexion of which with benzidine (xenylene-diamine) I have already had an opportunity of pointing out*, has induced me to submit the latter compound to some experiments. In preparing benzidine by the process originally pointed out by Zinin†, viz. by treatment of azobenzol with sulphide of ammonium, I was led to the observation of some phenomena which appear to have escaped the attention of those who have hitherto studied this substance.

It is generally supposed that the action of reducing agents upon azobenzol produces directly benzidine.

$$\underbrace{C_{12}\,H_{10}\,N_2}_{\text{Azobenzol.}}+H_2=\underbrace{C_{12}\,H_{12}\,N_2}_{\text{Benzidine.}}.$$

Such, however, is not the case. The well-defined base designated by the latter name is only a secondary product; the first compound which is generated in this process being a neutral or feebly basic body, differing in all its properties from benzidine, with which, however, it is isomeric, and into which it may be converted by simple treatment with strong mineral acids.

On passing a current of sulphuretted hydrogen into a solution of azobenzol in alcoholic ammonia, the yellowish-red liquid is rapidly decolorized, and yields on addition of water a crystalline precipitate of a peculiar camphor-like smell. This substance contains a minute quantity of the sulphur which separates, the bulk of which, however, remains dissolved as polysulphide of ammonium; it is easily purified by two or three crystallizations from very dilute alcohol. Submitted to combustion, the compound thus obtained yields numbers which coincide with those furnished by the analysis of benzidine.

The properties in which the new substance, for which I propose the name *hydrazobenzol,* differs from benzidine are the following:—Hydrazobenzol crystallizes from alcohol, and more especially from

* Proc. Roy. Soc. vol. xii. p. 389. † Journ. Prakt. Chem. vol. xxvi. p. 93.

benzol (in which it is somewhat less soluble), in well-developed white plates, benzidine being always deposited from these solvents in well-defined needles; and whilst the latter is freely soluble in boiling water, from which it separates on cooling in the form of a crystalline mass of nacreous lustre, the former is so sparingly soluble in water that it is impossible to recrystallize it from that solvent. The fusing-point of hydrazobenzol is 131° C., that of benzidine being 118° C. The basic properties of benzidine are well defined; it dissolves even in the weakest acids, such as acetic acid, in which hydrazobenzol is nearly insoluble. Stronger acids, such as hydrochloric and sulphuric acids, more especially on application of heat, dissolve hydrazobenzol; but the solution thus obtained contains no longer the unchanged body; the addition of alkali, fixed or volatile, produces a precipitate which now possesses all the properties of *benzidine.*

These characters are sufficient to individualize hydrazobenzol. There is, however, another property which marks its difference from benzidine in even a more conspicuous manner. Benzidine when submitted to a high temperature distils; a certain portion is decomposed in this process, but the larger quantity is volatilized without decomposition. On heating hydrazobenzol considerably above its fusing-point, a powerful reaction ensues, the heat evolved being sufficient to carry over nearly the whole amount of substance in the form of a deep red oil, from which, on cooling, crystals of azobenzol are deposited. On addition of an acid the oil yields a further quantity of this substance, and the acid solution is then found to contain abundance of aniline. The reaction which occurs is simple enough.

$$\underbrace{2C_{12}H_{12}N_2}_{\text{Hydrazobenzol.}} = \underbrace{C_{12}H_{10}N_2}_{\text{Azobenzol.}} + \underbrace{2C_6H_7N.}_{\text{Aniline.}}$$

I had hoped that among the products of the reaction paraniline ($C_{12}H_{14}N_2 = 2C_6H_7N$) might be met with; in this hope I have been disappointed.

The reproduction of azobenzol from hydrazobenzol may be accomplished in a variety of other ways. Nitrous acid, chlorine, bromine, iodine, chromate and permanganate of potassium, and nitrate of silver produce this effect in a most easy manner; in these processes the loosely adherent hydrogen is simply eliminated, no aniline being formed as a secondary product. Even when moistened with alcohol

and exposed to the action of the atmosphere, hydrazobenzol is gradually reconverted into azobenzol.

It deserves to be noticed that some of the chemists who have been engaged in the examination of benzidine must have occasionally worked with hydrazobenzol. Mr. Noble*, who many years ago prepared benzidine in my laboratory, especially remarks that the substance obtained by him is reconverted into azobenzol by the action of nitrous acid. I have satisfied myself that benzidine thus treated yields no trace of azobenzol.

From the experiments described, it is obvious that in the formation of benzidine from azobenzol two distinct phases have to be distinguished: in the first phase the molecule of azobenzol assimilates a molecule of hydrogen, but this hydrogen remains in a very feeble state of combination, being eliminated again by a great variety of agents. It is only under the influence of acids that the hydrogen molecule becomes incorporated in the system, if I may use this expression, and fixed benzidine, a substance of great stability, is formed.

Whatever view may be taken regarding the nature of azobenzol, the constitution of which it must be admitted is utterly unknown, the intermediate substance has to be viewed as its hydrogen compound, and it is this consideration which induced me to propose the name *hydrazobenzol.*

VI. "Note on the Composition of Aniline-Blue." By A. W. Hofmann, LL.D., F.R.S., &c. Received May 21, 1863.

The prosecution of my researches on the aniline colours has led me to a result of great simplicity, which I hasten to lay before the Royal Society.

Aniline-blue is triphenylic rosaniline.

Aniline-red, Rosaniline	$C_{20}H_{19}N_3, H_2O.$
Aniline-blue, Triphenylic Rosaniline	$C_{20}H_{16}N_3, H_2O.$ $(C_6H_5)_3.$

The commercial article is a salt of the base, the hydrochlorate for example, the composition of which corresponds to the monatomic hydrochlorate of rosaniline.

* Chem. Soc. Quart. Journ. vol. viii. p. 292.

Hydrochlorate of Rosaniline	$C_{20} H_{20} N_3 Cl.$
Hydrochlorate of Triphenylic Rosaniline . .	$C_{20} H_{17} N_3 Cl. (C_6 H_5)_3.$

Details of these experiments I hope to lay before the Society at an early meeting.

VII. "On the Calculus of Symbols."—Third Memoir. By W. H. L. Russell, Esq., A.B. Communicated by A. Cayley, F.R.S. Received May 15, 1863.

(Abstract.)

In my second Memoir "On the Calculus of Symbols," I worked out the general case of multiplication according to one of the two systems of combination of non-commutative symbols previously given. In the present paper I propose to investigate the general case of multiplication according to the other system. I commence with the Binomial Theorem, to which the second system gives rise. In my previous researches I obtained the general term of the binomial theorem when the symbols combine according to the first system by equating symbolical coefficients; here, on the other hand, I consider the nature of the combinations which arise from the symbolical multiplication, and obtain the general term by summation. I next proceed to the multiplication of binomial factors. Here the general term is obtained by considering the alteration of weight undergone by certain symbols in the process of multiplication. The multinomial theorem according to the second system is next considered and its general term calculated. I conclude the memoir with some applications of the calculus of symbols to successive differentiation. This paper completes the investigation of symbolical multiplication and division according to the two systems of combination, the general case of division having been worked out by Mr. Spottiswoode in a very beautiful memoir recently published in the Philosophical Transactions.

The Society then adjourned over the Whitsuntide Recess to Thursday, June 11, the President having announced the Meeting for the Election of Fellows to take place on Thursday, June 4, at 4 P.M.

June 4, 1863.

The Annual Meeting for the Election of Fellows was held this day.

Major-General SABINE, President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Mr. John Bishop and Mr. John Hogg were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected into the Society:—

Edward William Cooke, Esq., A.R.A.
William Crookes, Esq.
James Fergusson, Esq.
Frederick Field, Esq.
Rev. Robert Harley.
John Russell Hind, Esq.
Charles Watkins Merrifield, Esq.
Professor Daniel Oliver.
Frederick William Pavy, M.D.
William Pengelly, Esq.
Henry Enfield Roscoe, B.A.
Rev. George Salmon, D.D.
Samuel James Augustus Salter, M.B.
Rev. Arthur Penrhyn Stanley, D.D.
Colonel Frederick M. Eardley Wilmot, R.A.

June 11, 1863.

Dr. W. B. CARPENTER, Vice-President, in the Chair.

Charles Watkins Merrifield, Esq.; Professor Daniel Oliver; Frederick W. Pavy, M.D.; Samuel James Augustus Salter, M.B.; and Col. Frederick M. Eardley Wilmot, R.A., were admitted into the Society.

The CROONIAN LECTURE was delivered by Professor JOSEPH LISTER, F.R.S., "On the Coagulation of the Blood," as follows:—

The subject on which I have the honour to address you this evening, is one which lies at the foundation both of Physiology and Pathology, and, on account of its great importance, has engaged the

best energies of many very able men, among whom may be mentioned, for example, such distinguished Fellows of this Society as John Hunter and Hewson; so that it might well seem presumptuous in me to hope to communicate anything new regarding it, were it not that the constant progress of Physiology and the allied sciences is ever opening up fresh paths for inquiry, and ever affording fresh facilities for pursuing them. Indeed, my difficulty, on the present occasion, does not depend so much on the lack of materials as on the complicated relations of the subject, which make me almost despair of being able, in the short time that can be devoted to a lecture, to give, in anything like an intelligible form, even an adequate selection of the facts at my disposal.

It may, in the first place, be worth while, more especially for the sake of any present who may not be physiologists, to mention very briefly some well-known general facts respecting the constitution of the blood. The blood, if examined by the microscope within the vessels of a living animal, is seen to consist of a liquid and numerous small particles suspended in it. The liquid is termed the "liquor sanguinis," the particles the "blood-corpuscles." Of these corpuscles a few are colourless, and are named the "colourless" or "white corpuscles." The great majority are coloured and cause the red appearance of the blood, and hence are called the "red corpuscles." Soon after blood has been shed from the body, it passes from the fluid into the solid form. This depends upon the development in the blood of a solid material termed "fibrin," so called from its fibrous nature, consisting, as examined by the naked eye, of tenacious fibres, and having the same character also under the microscope. These fibres form a complicated network among the blood-corpuscles, and from their tenacity are the cause of the firmness of the clot. Soon after the process of solidification or coagulation is complete, the fibrin exhibits a disposition to shrink, and squeezes out from among the corpuscles entangled in its meshes a straw-coloured fluid termed the serum, very rich in albumen, in fact very similar in chemical composition to the fibrin, which, in its turn, may be said to be identical chemically with the material of muscular fibre.

The question before us, therefore, is, What is the cause of the development of this solid material, the fibrin? The subject may be looked at in two aspects,—first, as to the essential nature of the

process of coagulation; and secondly, as to the cause of its occurrence when the blood is removed from the body.

With regard to the first point, the essential nature of the process of coagulation, different views have been entertained. John Hunter was of opinion that the coagulation of the blood, the solidification of the fibrin, was an act of life—analogous, in some respects, to the contraction of muscular fibre. This, on the other hand, was made very unlikely by the observation of his contemporary, Mr. Hewson, that blood may be kept in the fluid state by the addition of various neutral salts, but retains the faculty of coagulating when water is added to the mixture. Mr. Gulliver, on one occasion, kept blood fluid, by means of nitre, for upwards of a year, but found that it still coagulated on the addition of water. It seems exceedingly improbable that any part of the human body should retain its vital properties after being thus pickled for more than a year. But here I would wish to make an explanation of the use of this term "vital properties." When employing it, I do not wish to commit myself to any particular theory of the nature of life, or even to the belief that the actions of living bodies are not all conducted in obedience to physical and chemical laws. But it appears that every component tissue of the human body has its own life, its own health, just as we ourselves have; and as the actions of living men will ever retain their interest whatever views be entertained of the nature of life, so must the actions of the living tissues ever continue to be essential objects of study to the physiologist and pathologist. When, therefore, I use the term "vital properties," I mean simply properties peculiar to the tissues as components of the healthy living body.

Turning now to the other aspect of the subject of coagulation—the cause of the occurrence of that process on the escape of the blood from the living body—we find that here again various theories have been held, which may be divided into mechanical, chemical, and vital. The mechanical theory was, that mere rest of the blood was sufficient to cause coagulation. I say this *was* the theory; but I believe it will be found to be still taught by many, that the cause of the coagulation of the blood in an artery which has been tied is its stagnation in the vicinity of the ligature.

As to the chemical theories they have been various. One very

natural view was, that exposure to the air was the essential cause of coagulation. Mr. Hewson believed that this was, at all events, an important element in the causes of the phenomenon; and many eminent physiologists and pathologists have held the same view, except that, instead of the air as a whole, the oxygen of the air has been supposed to be the important element.

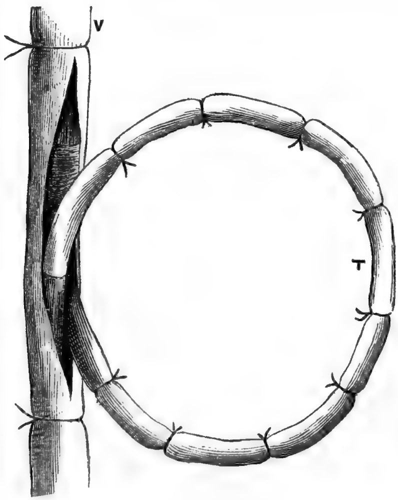
Sir Charles Scudamore considered that coagulation was greatly promoted by the escape of carbonic acid; and more recently the evolution of ammonia has been regarded as the essential cause of the change. According to the ammonia theory, due to Dr. Richardson of this city, the fluidity of the blood within the body depends on a certain amount of free ammonia holding the fibrin in solution, and the coagulation of the blood when withdrawn from the vessels is the result of the escape of the volatile alkali.

Then, as to vital theories. These have been held by many physiologists, among whom may be mentioned Sir Astley Cooper and Mr. Thackrah, who, from experiments which they performed, were led to the inference that the living vessels exert an active influence upon the blood, by which coagulation is prevented; and Mr. Thackrah went so far as to attribute this action of the vessels to nervous influence. The view that the blood is kept fluid by the operation of its natural receptacles has been advocated more recently by Brücke of Vienna, whose essay will be found in the 'British and Foreign Medical Review' for 1857. Brücke performed his experiments on turtles and frogs, in which animals the blood remains fluid in the heart for days after death; and I feel bound to say that some of the facts which he has brought forward seem to me quite sufficient to show that the ammonia theory, whatever amount of truth it may contain, cannot be the whole truth, and cannot explain the fluidity of the blood within the body. For example, Brücke found that, having shed blood from the heart of a living turtle into a basin, and transferred, with a syringe, a portion of that blood into the empty heart of another turtle just killed, the blood thus transferred into the empty heart remained fluid for hours; whereas that which was left in the basin coagulated in a few minutes. He also found that blood continued fluid in the heart of a turtle long after the injection of air into the heart through a vein till the cavities of the organ contained a foamy mixture of blood and air.

Yet it by no means follows that the vital theory and the ammonia theory are necessarily altogether inconsistent. It might be true for anything we could tell, *à priori*, that the coagulation of the blood, when shed from the body, might depend on the evolution of a certain amount of ammonia, previously holding the fibrin in solution, and yet it might, at the same time, be true that the cause of the ammonia remaining in the blood in the healthy vessels might be an action of the living vessels retaining it there. It might be that an action of the living vessels might chain down the ammonia and prevent it from escaping, whereas, when shed from the body, it would be free to escape.

This notion was, I confess, at one time entertained by myself; and one of my earliest experiments was performed with a view to the corroboration of the ammonia theory as applied to blood outside the body. It seemed to me desirable that further evidence should be afforded of the effect of mere occlusion from air in maintaining the blood fluid. If the ammonia theory were true, then if blood could be shed directly from a living vessel into an air-tight receptacle composed

Fig. 1.



of ordinary matter it ought to remain fluid. For this purpose, I made the following experiment:—I tied into the jugular vein, V, (fig. 1) of

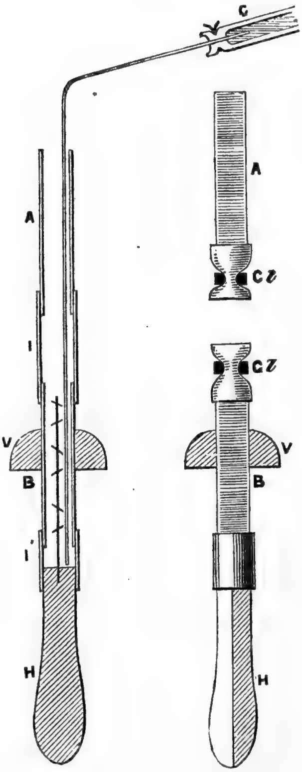
a sheep a long vulcanized india-rubber tube, T, adapted by means of short pieces of glass tube at its extremities, both ends being connected with the vessel so that the current of blood might be permitted to flow through the tube, and then continue its natural course. When it had been ascertained that the blood was circulating freely through the tube, which could be readily done by placing the finger on the cardiac aspect of the vein, which was then made to swell if the circulation was proceeding through the tube, pieces of string well-waxed were tied at intervals of about 2 inches round the tube, which was thus converted into a number of air-tight receptacles containing blood, which certainly had no opportunity for the escape of ammonia. The tube was then removed, and I found, in accordance with the view which I was then disposed to entertain, that the blood, instead of coagulating completely in a few minutes as it would have done if shed into a cup, remained partially fluid in these receptacles after the lapse of three hours. But I have since found that if the experiment be repeated in the same way as regards its earlier stages, and if, after a few of the strings have been tied on, the tube be cut across, the blood which is in the part of the tube in the vicinity of the air, just like that which is in the air-tight receptacles, remains fluid in part for two or three hours. In short, that my precautions in ensuring that these receptacles should be air-tight were, in so far as they applied to that object, utterly unnecessary. I mention this partly as an illustration of the deceptions to which one is liable in this inquiry, and partly because the experiment thus modified seems to tell as clearly against the ammonia theory as the original one seemed to tell in favour of it. Those receptacles which had been formed by the application of ligatures before the tube was opened afforded certainly no opportunity for the escape of ammonia, and yet in them the blood coagulated as quickly as in those which had communication with the air—implying that facility for the evolution of ammonia does not in itself affect the process of coagulation at all.

How then, it may be said, is the persistent fluidity of the blood under these circumstances to be explained? That will become more obvious than I can make it at present in the sequel; but in the mean time I may observe that there are probably two explanations: one is, the coolness of the tube, and the other, far more important,

that the blood, in slipping through this cylindrical tube, had had little opportunity of being influenced by its walls. The portion of the blood that came first in contact with the walls of the tube had coagulated; and it is to be observed that I never found, in these experiments, the blood altogether fluid, even after a comparatively short time: there has always been a certain amount of coagulation, and only a certain amount of fluidity. A layer of blood having thus coagulated upon the internal surface of the tube, the fresh blood which continued to flow through it, was not brought into contact with the walls of the tube at all, but with their lining of coagulated blood.

It has been long known that if blood is stirred with a rod, the process of coagulation is promoted. It seemed desirable to ascertain distinctly whether the cause of this was the contact of the foreign solid, or the opportunity given for the escape of ammonia; for it is quite true that, in the ordinary process of stirring blood, more or less air is mixed with it. For the purpose of determining this I devised a somewhat complicated experiment, which, however, it may be worth while to mention. I made an apparatus (fig. 2) of two portions of glass tube, A and B, connected in a vertical position by means of vulcanized india-rubber, I, the lower portion of the glass tube being also connected by india-rubber, I′, with a wooden handle, which handle, H, was provided with an upright piece of wire, from which spokes projected in different directions, so

Fig. 2.



that they would, when moved, act as a churn on any blood contained in the lower portion of tube. When the lower piece of tube was fixed by means of a vice, V, the flexibility of the india-rubber permitted the churn to be rotated so as to expose the blood to its influence. This having been arranged, I first poured in strong *liquor ammoniæ,* so as to get rid of any slight acidity which the constituents of the apparatus might be conceived to possess, and then, having poured out the ammonia, filled up the apparatus with water, and boiled the whole in a large glass test-tube till all bubbles of air, in any portion of it, were expelled. Having then tied into a branch of the carotid artery, C, of a calf a bent tube of small diameter, as represented, and having permitted the blood to flow till it escaped at the orifice of the tube, I compressed the artery and passed the tube down through the water to the bottom of the apparatus, and then let the blood flow again, which had the effect of displacing all the water; and when the blood appeared at the top of the apparatus, the tube was withdrawn, when two effectual clamps, Cl, Cl, were placed on the vulcanized india-rubber connecting A and B; the india-rubber was then divided between the clamps, and we had the state of things represented at the right-hand side of the diagram. The upper portion of the apparatus, the orifice of which was exposed to the air, was set aside and left undisturbed. Having ascertained that the lower portion had been effectually sealed by the clamp, and thus prevented from any opportunity of escape of ammonia, I subjected it to the action of the churn for a certain number of minutes. It so happened that the blood of that calf was very slow in coagulating. I knew this from previous experiments on the animal, and therefore continued the action of the churn for a considerable time, viz. thirty-seven minutes. I then found the wire enveloped in a mass of clot; and examination of the fluid residue with a needle indicated that the fibrin had been all withdrawn from the blood on which the churn had acted. I did not now examine the other portion of the apparatus, which had been set aside; but at the end of an hour and a quarter, when more than double the time had elapsed, I investigated this, and found the blood in it, for the most part, still fluid and coagulable. Thus the blood in the churn, which, from the time it left the artery, had no oppor-

tunity of parting with its ammonia, coagulated much more rapidly than that in an open vessel. The difference between the two was, that the lower portion of the blood had been freely exposed to the influence of the foreign solid, whereas the other had only been subjected to the action of the wall of the tube.

The same principle may be illustrated by an exceedingly simple experiment which I performed only this very day. Receiving blood from the throat of a bullock into two similar wide-mouthed bottles, I immediately stirred one of them with a clean ivory rod for 10 seconds very gently, so as to avoid the introduction of any air, and then left both undisturbed. At the end of a certain number of minutes I found that, while the blood which had not been disturbed could be poured out as a fluid, with the exception of a thin layer of clot on the surface, and an incrustation on the interior of the vessel, the blood in the other vessel, which had been stirred for so brief a period, was already a solid mass.

I have only lately been aware of the great influence exerted upon the blood by exposure for a very short time to a foreign solid, and I feel that many of my own experiments, and many performed by others, have been vitiated for want of this knowledge. Take, for example, the effect of a vacuum, which was observed by Sir Charles Scudamore to promote coagulation. This has been considered by Dr. Richardson as an illustration of his theory, the vacuum being supposed to act by favouring the escape of ammonia. I have lately inquired into this subject, and I feel no doubt whatever that the greater rapidity of coagulation in a vacuum depends simply on the greater disturbance of the fluid. I made the following experiment:— I filled three bottles, such as these, from the throat of a bullock, placed one of them under the small bell jar of an air-pump in good order and exhausted it, leaving the other two undisturbed. The blood happened to be slow in coagulating; and at the end of about forty minutes, in the vessels where the blood had been undisturbed, there was only a slight film of coagulum on the surface, whereas the blood under the vacuum was found on examination to have a very thick crust of clot upon it. But during the process of exhaustion the blood had bubbled very much. Indeed, any exhaustion of blood recently drawn which is sufficient to cause the evolution of its gases

induces great bubbling; so that the pump cannot be used freely, for fear of the froth overflowing. To this disturbance, involving the exposure of successive portions of blood in the bubbles to the sides of the vessel, I was inclined to attribute the more rapid coagulation; but in order to prove the point, I stirred for a few seconds the blood in one of the vessels hitherto undisturbed. After eight minutes I emptied the three vessels. I found that that blood which had not been disturbed at all, either by the vacuum or by the rod, was still almost entirely fluid, only showing a thin crust upon the glass and on the surface exposed to the air. The blood which had been subjected to the vacuum had a thick crust of clot on the surface, and the sides of the glass were also thickly encrusted, but it still contained a considerable quantity of fluid that could be poured out from its interior. But that blood which had been stirred for only a few seconds was a solid mass throughout. In other words, gentle stirring of the blood for a few seconds had much greater effect in producing coagulation than the protracted and efficient exhaustion which was continued for upwards of 40 minutes, which was a considerable time after all evolution of gas, as indicated by bubbles, had ceased.

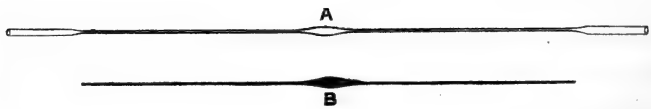
Other experiments precisely similar in their effect were performed. I therefore feel no hesitation in stating that the effects of a vacuum, regarding which, indeed, the statements of different experimenters have hitherto been conflicting, afford no evidence in favour of the ammonia theory.

There is another point of very great interest in the history of the coagulation of the blood, which has been supposed to give support to the ammonia theory; and that is, the effect of temperature. It has been long known that blood coagulates more rapidly at a high than at a low temperature, and, indeed, a little above the freezing-point remains entirely fluid. This seemed beautifully in harmony with the ammonia theory, as heat would naturally promote, and cold retard the evolution of the alkali, and a depression of temperature to near the freezing-point might be reasonably supposed to prevent its escape altogether. Indeed Dr. Richardson mentions as a fact, that ammonia artificially mixed with blood ceases to be given off under such circumstances.

Though thinking it not unlikely that this was the true explanation of the influence of temperature on coagulation, I thought it worth while to subject the matter to experiment. For that purpose I kept the blood of a horse fluid by means of a freezing-mixture, and afterwards by ice-cold water; and when the corpuscles had subsided from the upper part of the blood, I cautiously added to the liquor sanguinis extremely dilute ice-cold acetic acid till it was of distinctly acid reaction, the liquor sanguinis being of a colour that permitted the delicate application of test-paper, which is impossible with red blood. By this means any free ammonia which the fluid might have contained must have been neutralized; yet so long as it was kept in the cold it continued fluid, but when brought into a warm room, coagulated just as a specimen which had not been acidulated. Thus, when there could be no free ammonia in the liquor sanguinis at all, it was still affected as usual by temperature.

This experiment may not be satisfactory to all minds, though I confess it appears so to me; and as this is a point of very great interest, I have sought in another way for evidence regarding it. First, however, I will mention an experiment which will not at once appear to bear on the question of temperature. I drew out a fine glass tube in such a way as to produce a fusiform receptacle continued longitudinally each way into a tube of almost capillary fineness for about two inches, which again expanded at the end, as represented in fig. 3. Having squeezed out a drop of blood from my finger, I

Fig. 3.



sucked up a portion into the tube till the receptacle A and its capillary extensions were filled. I then broke off the expanded ends, and placed the little tube thus filled, B, in a bath of the strongest liquor ammoniæ. Here certainly the blood was in circumstances in which it could not lose ammonia, but where any change in its amount must be by way of increase, and yet I found, on opening the receptacle by

snapping it across after a scratch with a file, that instead of remaining longer fluid than in a watch-glass, the blood in it, being more in contact with the glass, was always more quickly coagulated, while coagulation was still more rapid in the capillary tube, where the blood was still more exposed to the influence of the foreign solid—the greater proximity to the liquor ammoniæ having no influence upon it.

It may perhaps be argued that the drop of blood employed being a small drop, and this small drop having been drawn up by suction into the tube, it might have parted with its ammonia before it got into the tube; but then (and now comes the bearing of the experiment on the effect of temperature) I found, if I placed a similar tube filled in the same way in a vessel of snow, so as not to freeze it but to keep it ice-cold, the blood in it remained fluid as long as I chose to keep it there. Now if all the ammonia had left the blood before it was introduced into the tube, cold ought, according to the ammonia theory, to have had no effect in retarding its coagulation; for, according to that theory, cold operates by retaining the ammonia. On the other hand, if we take the other alternative and suppose that any ammonia which the blood might have contained was still in these tubes, the former experiment proves clearly that the retention of ammonia has no effect in producing fluidity—no effect in preventing coagulation; and if the retention of ammonia has no effect in preventing coagulation, then cold certainly cannot prevent coagulation by retaining the ammonia, because, even if retained, it would not influence the result. In whatever way we look at them, therefore, these simple experiments prove conclusively that cold maintains the fluidity of the blood in some manner unconnected with any influence it may exert upon ammonia.

Then, again, I varied the experiment in this way. I placed such little tubes of blood in baths of liquor ammoniæ at different temperatures. By careful management, guarding against the volatilization of ammonia and consequent reduction of temperature, I succeeded in employing satisfactorily a bath of liquor ammoniæ at 100° F., the blood being in the bath within a few seconds of its leaving the vessels of my finger, and I found that the high temperature, though under such circumstances it could not possibly dissipate any ammonia

from the blood, yet accelerated its coagulation in precisely the same way as when it was applied to blood in watch-glasses exposed to the air.

It is clear, then, that the promotion of the solidification of fibrin by heat is as independent of the evolution of ammonia as the coagulation of albumen under the same agency. Indeed it seems probable that the two cases are analogous, except that a higher temperature is required in the one than in the other.

When fine tubes containing blood were placed in liquor ammoniæ, the alkali acted only upon those parts which were close to the ends of the tubes; a very small portion was rendered brown by it, and beyond that a little was kept permanently fluid, but the chief length of the blood in the tube was unaffected. Having thus ascertained that ammonia travels so slowly along tubes of this capillary fineness, I thought I might have an opportunity of giving the ammonia theory a fair test by tying such a tube as has been above described into the jugular vein of a rabbit and filling it directly from the vessel, and then ascertaining whether there was any evidence of retardation of coagulation in the blood thus imprisoned. But I could discover no such evidence, although I sought for it in confirmation of a view I then held. To this, however, there is one special exception to be made, viz. in the case of asphyxia. I found that if two such tubes were filled from the same blood-vessel of a creature, one under normal circumstances, and the other after asphyxia had been induced, there was a most remarkable difference between the rates of coagulation of the blood in the two tubes, the asphyxial blood coagulating very much more slowly than the ordinary blood; but when the asphyxial blood was shed into a watch-glass and air was blown through it, it coagulated rapidly, showing that in the state of asphyxia there must be some volatile element in the blood which has an effect in retarding coagulation.

Supposing at first that this volatile element must be ammonia, I hoped to be able by chemical means to find evidence of its accumulation in asphyxia, and thus add a fact of great interest to physiology. Imitating experiments previously made by Dr. Richardson, I passed air successively through blood and through hydrochloric acid, and then estimated the amount of ammonia acquired by the latter by means of

oichloride of platinum. In order to prevent the possibility of the loss of any ammonia, I directed blood from the carotid artery of a calf fairly into a Woulfe's bottle by means of a vulcanized india-rubber tube tied into the vessel, and then drew a certain volume of air through it by means of an aspirating jar, the experiment being performed first before, and then during asphyxia. The same procedure was adopted with a second calf, the animal being in each case under chloroform, which does not interfere with the development during asphyxia of the peculiarity in the blood above alluded to; but I could not find satisfactory evidence of accumulation of ammonia; and without going further into the question at present, I may say that it seems much more probable that the effect is due to carbonic acid, which is known to have a retarding influence on coagulation, and which probably accumulates greatly in asphyxial blood.

But in justice to the author of the ammonia theory, and to myself, too, who at one time expressed a qualified belief in it, it is but fair to say that this theory is extremely plausible. It has been well shown by Dr. Richardson that ammonia is a substance well fitted to keep the blood fluid if it be present in a sufficient quantity. An experiment of my own illustrates very well the same point. I drew out a tube about a quarter of an inch in calibre (fig. 4), so that while

Fig. 4.

for two inches at one end it retained its original width, the rest (some ten inches) was pretty narrow, though far from having the capillary fineness of those before described. Into the thick part I introduced a drop of strong liquor ammoniæ, A, and then securely corked that end of the tube, C. The object of this was that there should be a strong ammoniacal atmosphere in the narrow part of the tube. I then opened a branch of a vein, V, in the neck of a sheep, introduced the narrow end of the tube into the vessel, and pushing it in so that its

orifice should be in the current of the main trunk of the vein, tied it in securely. I then removed the cork and made pressure on the vein at the cardiac side, causing the vessel to swell and blood to pass into the fine part of the tube; and before the blood had reached the part of the glass moistened by the ammonia, I put in the cork again and withdrew the tube. In a short time, on introducing a hook of fine wire into the extremity of the tube, I found the blood already coagulated; but on filing off a small portion of the tube, I found the blood there fluid. The portion of blood thus exposed soon coagulated, when, a second small piece of the tube being removed by the file, fluid blood was again disclosed, which again soon coagulated; and this proceeding was repeated with the same results time after time, till, near the thick part of the tube, the ammonia in the blood was so strong as to prevent coagulation altogether.

This experiment illustrates how fitted the ammonia is to maintain the fluidity of blood, and also how apt it is, when present in the blood, to fly speedily off from it, leaving it unimpaired in its coagulating properties; and it must be confessed that the end of the tube sealed with a small clot resembled most deceptively the extremity of a divided artery similarly closed. But although the experiment seems in so far to favour the ammonia theory, it will tell differently when I mention the object with which it was performed. It appeared to me that, if the cause of the fluidity of the blood was free ammonia, then, if I provided an ammoniacal atmosphere in the tube, and introduced blood by pressure directly from the vein into this ammoniacal atmosphere, this blood, lying between the strong ammoniacal atmosphere on the one side and the ammonia naturally present in the blood within the vein on the other side, ought to remain fluid; and if it did remain fluid, this would tend to confirm the ammonia theory by making it appear that the volatile material was the same at both ends of the tube. But, to my disappointment, I invariably found that if I drew away the tube after a few minutes only had elapsed, there was already a clot in its extremity; in other words, the ammonia had diffused from the end of the tube into the blood within the vein as into a non-ammoniacal atmosphere. This experiment alone, if duly considered, would, I think, suffice to show that the blood does not contain enough ammonia to account for its fluidity.

Fig. 5.

One more experiment, however, may be adduced with the same object. I mounted a short but wide glass tube, open at both ends (T, fig. 5), upon the end of a piece of strong wire, W, and connected with the latter a coil of fine silver wire, S, so that it hung freely in the tube. I then opened the carotid artery of a horse, and through the wound instantly thrust in the apparatus so far that I was sure the tube lay in the common carotid, which in veterinary language means the enormous trunk common to both sides of the neck of the animal. The tube being open at both ends, and slightly funnel-shaped at that end which was directed towards the heart, had thus a full current of arterial blood streaming through it. Having ascertained how long the arterial blood took to show the first appearance of coagulation in a watch-glass, I very soon after removed the apparatus, and, on taking out the coil of silver wire, found that it was already crusted over with coagulum. Yet here assuredly there had been no opportunity for the escape of ammonia.

From this experiment it is obvious that there is a very great difference between ordinary solid matter and the living vessels in their relation to the blood. But the same conclusion may be drawn much more simply from experiments which I had the opportunity of performing after making an observation which it seems strange should have been left for me to make, and which, I may say, was made by myself purely accidentally; and this is, that the blood of mammalia, although it coagulates soon after death in the heart and the principal arterial and venous trunks, remains fluid for an indefinite period in the small vessels. If, therefore, a ligature be tied round the foot of a living sheep a little below the joint which is divided by the butcher, the foot being removed and taken home with the blood retained in the veins by the ligature, we have a ready opportunity of investigating the subject of coagulation, and of making observations as satisfactory as they are simple. Here are two feet provided in the way I have alluded to. A superficial vein in each

foot has been exposed. The veins I see have contracted very much since I reflected the skin from them before our meeting; and I may remark that such contraction, dependent on muscular action, may occur days after amputation, indicating the persistence of vital properties in the veins. Now as I cut across this vein, blood flows out, fluid but coagulable. Into the vein of this other foot has been introduced a piece of fine silver wire, and when I slit up the vein you will see the effect it has produced. Exactly as far as the silver wire extends, so far is there a clot in this vessel. Now this experiment, very simple as it is, is of itself sufficient to prove the vital theory in the sense that the living vessels differ entirely from ordinary solids in their relation to the blood. It is perfectly clear that by introducing a clean piece of silver wire (and platinum or glass or any other substance chemically inert would have had the same effect) I do not add any chemical material or facilitate the escape of any, and yet coagulation occurs round about the foreign solid.

Again, if a blood-vessel be injured at any part, coagulation will occur at the seat of injury. As a good illustration of this, and also as bearing upon the ammonia theory, I may mention the following experiment. Having squeezed the blood out of a limited portion of one of the veins of a sheep's foot, and prevented its return by appropriate means, I treated the empty portion with caustic ammonia, the neighbouring parts of the vein being protected from the irritating vapour by lint steeped in olive oil. After the smell of ammonia had passed off, I let the blood flow back again and left it undisturbed for a while, when I found on examination a cylindrieal clot in the part that had been treated with ammonia, while in the adjacent parts of the same vessel the blood remained fluid. I repeated this experiment several times and always with the same result. Where the ammonia had acted there was a clot. The chemical agent used here was one which, so long as any of it remained, would keep the blood fluid; yet its ultimate effect was to induce coagulation, the vital properties of the vein having been destroyed by it.

If a needle or a piece of silver wire is introduced for a short time into one of the veins of the sheep's foot, it is found on withdrawal to be covered over with a very thin crust of fibrin, whereas the wall of the vessel itself is never found to have fibrin or coagulum adhering to it unless it has been injured. Now this seems to imply that the

ordinary solid is the active agent with reference to coagulation—that it is not that the blood is maintained fluid by any action of the living vessels, but that it is induced to coagulate by an attractive agency on the part of the foreign solid. We see at any rate that the foreign solid has an attraction for fibrin which the wall of the vessel has not.

And yet I own I was at first inclined to think that the blood-vessels must in some way actively prevent coagulation. There were two considerations that led to this view. One was, that the blood remained fluid in the small vessels after death, but coagulated in the large. Now why should that be? It seemed only susceptible of explanation from there being some connexion between the size of the vessel and the circumstance of coagulation. It looked as if in the small veins the action of the wall of the vessel was able to control the blood and keep it fluid, but that the large mass in the principal trunks could not be so kept under control. The other circumstance was, the rapid coagulation of a large quantity of blood shed into a basin. Why should this occur unless there was some spontaneous tendency in the blood to coagulate? It seemed scarcely credible that it was the result of contact with the surface of the basin.

Both these notions, however, have since been swept away. In the first place, I have observed recently that it is by no means only in small vessels that the blood remains fluid after death. If blood be retained within the jugular vein of a horse or ox by the application of ligatures, either before or after the animal has been struck with the poleaxe, it will often continue fluid, but coagulable, in that vessel, which is upwards of an inch in diameter, for twenty-four or even forty-eight hours after it has been removed from the body. I say often, but not always. The jugular vein seems to be in that intermediate condition, between the heart and the small vessels, in which it is uncertain whether it will retain its vital properties for many hours, or will lose them in the course of one hour or so. Unfortunately for my present purpose, it happens that in this jugular vein, removed from an ox six hours ago, coagulation has already commenced, as I can ascertain by squeezing the vessel between my fingers. But now that I lay open the vessel, you observe that the chief mass of its contained blood is still fluid, and we shall at all events have an opportunity of seeing that what is now fluid will in a short time be coagulated. It is an interesting

circumstance with reference to the question which we are now considering, that the coagulation always begins in contact with the vein, indicating that it is not the wall of the vessel that keeps the blood fluid, but that, on the contrary, the wall of the vessel, when deprived of vital properties, makes the blood coagulate.

The observation of the persistent fluidity of the blood in these large vessels furnished the opportunity of making a very satisfactory experiment, which I hoped to have exhibited before the Society; but as there was some clot in the vein, I did not think fit to run the risk of failure. The experiment is performed in the following way. A piece of steel wire is wound spirally round one of the veins in its turgid condition, and with a needle and thread the coats of the vessel are stitched here and there to the wire, care being taken to avoid puncturing the lining membrane, and thus the vessel is converted into a rigid cup. Two such cups being prepared, and the lining membrane of the vein being everted at the orifice of each so as to avoid contact of the blood with any injured tissue, I found that, after pouring blood to and fro through the air in a small stream from one venous receptacle into the other half a dozen times, and closing the orifice of the receptacle to prevent drying, the blood was still more or less completely fluid after the lapse of eight or ten hours. On the other hand, if a fine sewing-needle is pushed through the wall of an unopened vessel so that its end may lie in the blood, it is found on examination, after a certain time has elapsed, that the needle is surrounded with an encrusting clot. It is scarcely necessary to point out how entirely the ammonia theory and the oxygen theory, as well as that of rest, fail to account for facts like these.

While the blood may remain fluid for forty-eight hours in the jugular vein of a horse or an ox, it coagulates soon after death in the heart of very small animals, such as mice; so that it is obvious that the continuance of fluidity in small vessels is not due to their small size.

It is a very curious question, What is the cause of the blood remaining so much longer fluid in some vessels than in others? I believe that we must accept it simply as an ultimate fact, that just as the brain loses its vital properties earlier than the ganglia of the heart, so the heart and principal vascular trunks lose theirs sooner than the smaller vessels of the viscera, or than more superficial vessels, be they

large or small. We can see a final cause for this, so to speak. So long as the heart is acting, circulation will be sure to go on in the heart and principal trunks; whereas, on the contrary, the more superficial parts are liable to temporary causes of stagnation, and occasionally to what amounts to practical severance from vascular and nervous connexion with the rest of the body; and it is, so to speak, of great importance that the blood should not coagulate so speedily in the vessels of a limb thus circumstanced as it does in the heart after it has ceased to beat. Were it not for this provision, the surgeon would be unable to apply a tourniquet without fear of coagulation occurring in the vessels of the limb. As an illustration of the importance of a knowledge of these facts, I may mention a case that once occurred in my own practice. I was asked by a surgeon in a country district to amputate an arm which he despaired of. The brachial artery had been wounded, as well as veins and nerves, and at last, being foiled with the hemorrhage, he wound a long bandage round the limb at the seat of the wound as tightly as he possibly could. It had been in this condition with the bandage thus applied for forty-eight hours when I reached the patient; and the limb had all the appearance of being dead. It was perfectly cold, and any colour which it had was of a livid tint. But having been lately engaged in some of the experiments which I have been describing, and having thus become much impressed with the persistent vitality of the tissues and the concomitant fluidity of the blood, I determined to give the limb a chance by tying the brachial artery. Before I left the patient's house he had already a pulse at the wrist, and I afterwards had the satisfaction of hearing that the arm had proved a useful one.

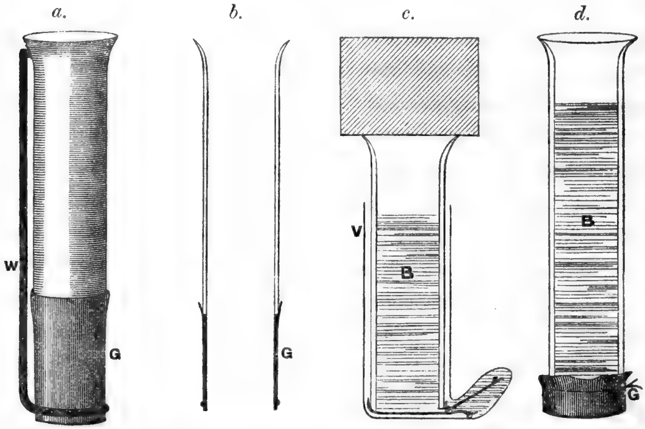
One of the two arguments in favour of activity on the part of the vessels as a cause of the fluidity of the blood having been completely disposed of, let us now consider the other, viz. the rapid coagulation of blood shed into a basin, appearing at first sight to imply a spontaneous tendency of the blood to coagulate, such as would have to be counteracted by the vessels. This also has proved fallacious.

In the first place it appears that the coagulation, after all, does not go on in a basin so suddenly as one would at first sight suppose, but always commences in contact with the foreign solid. When blood has been shed into a glass jar, if, on the first appearance of a film at the surface, you introduce a mounted needle curved at the end be-

tween the blood and the side of the glass and make a slight rotatory movement of the handle, you see through the glass the point of the needle detaching a layer of clot whatever part you may examine. The process of coagulation having thus commenced in contact with the surface of the vessel into which the blood is shed, may under favourable circumstances be ascertained to travel inwards, like advancing crystallization, towards the centre of the mass. It appears, however, that this extension of the coagulating process would not take place had not the blood been prepared for the change by contact, during the process of shedding, with the injured orifice of the blood-vessel and with the surface of the receptacle. I have only very recently become acquainted with the remarkable subtlety of the influence exerted upon blood by ordinary solids. I was long since struck with the fact, that if I introduced the point of an ordinary sewing-needle through the wall of a vein in a sheep's foot and left it for twelve hours undisturbed, the clot was still confined to a crust round the point of the needle, implying that coagulum has only a very limited power of extension. I thought, therefore, that by proper management it might be possible to keep blood fluid in a vessel of ordinary solid matter lined with clot. But various attempts made with this object failed entirely, till I lately adopted the following expedient. Having opened the distal end of an ox's jugular vein containing blood and held in the vertical position, taking care to avoid contact of any of the blood with the wounded edge of the vessel, I slipped steadily down into it a cylindrical tube of thin glass, somewhat smaller in diameter than the vein, open at both ends, and with the lower edge ground smooth in order that it might pass readily over the lining membrane, and so disturb the blood as little as possible by its introduction, and influence only the circumferential parts of its contents. The tube was then kept pressed down vertically upon the bottom of the vein by a weight, in a room as free as possible from vibration, and I found on examining it at the end of twelve hours that the clot was a tubular one, consisting of a crust about one-eighth of an inch thick next the glass and the part exposed to the air, but containing in its interior fluid and rapidly coagulable blood. In another such experiment, continued for twenty-fours, though the crust of clot was thicker, the central part still furnished coagulable blood.

But it may perhaps be argued by those who say that the blood-vessels are active in maintaining fluidity, that the small portion of the vein covering the end of the tube was acting upon the blood, which certainly was fluid where in contact with it, the clot being in the form of a tube open at the lower end. To guard against such an objection I made the following experiment:—I extended a tube like that above described by means of thin sheet gutta percha, G (fig. 6 *a*),

Fig. 6.



contriving that the internal surface of the gutta percha should be perfectly continuous with that of the glass tube as represented in section in fig. 6 *b*. The lower part of the gutta-percha tissue was strengthened by a ring of soft flexible wire such as is used by veterinary surgeons for sutures, and the wire W was also extended upwards to the top of the glass so as to maintain the rigidity of the gutta-percha portion during its introduction into a vein, but at the same time, from its softness, permit the gutta-percha part to be bent at a right angle after it had been introduced, and so close the orifice of the glass tube with ordinary solid matter. In fig. 6 *c* the tube is represented pressed down by a weight in a vein V, with blood B in the glass portion, while the gutta-percha part closes it below. At the same time I performed a comparative experiment, to which I would invite particular attention, although I am sorry at this late hour to occupy the attention of the Society so long. I tied a thin piece of gutta-percha tissue over the lower end of a similar glass tube, and simply

poured blood into it from the jugular vein of an ox. I wished to compare the condition of blood which had been simply poured into a tube, with blood which had been introduced without any disturbance of its central parts. But in order to make the experiment a fair one, as it might be said that the blood poured from the vein had been more exposed to the air than that into which the tube was slipped, I proceeded in the following way:—I obtained a long vein containing plenty of blood, and having first filled the second tube, with the gutta-percha bottom (fig. 6 *d*), by simply pouring blood into it from the vein, I cut off a portion of the vein which had been thus emptied, and having tied one end and everted the lining membrane of the other end, and having also everted the lining membrane of the orifice of the remainder of the vessel which was full, I poured the blood from the full portion through the air into the empty part. In doing this I had difficulty in getting blood enough, and it passed through the air in slow drops, and that only when the vein was squeezed by my warm hand. At last, having introduced sufficient for the purpose, I slipped down the compound tube and bent its gutta-percha portion, as represented in fig. 6 *c*, and left both tubes for a while undisturbed. At the end of three hours and a half I found that the blood which had been simply poured in was a mass of clot, and fluid squeezed from it yielded no threads of fibrin, coagulation being complete. How long it had been so I do not know. I did not examine the other blood until seven hours and three quarters had expired, and then found that, just as in the cases where a simple glass tube was introduced, the clot was tubular, and the chief part of the blood was still fluid in its interior, the only difference being that in this case the clot formed a complete capsule, being continued over the gutta percha instead of being deficient below, as it was when the vein closed the end of the tube. Now if we consider the two parts of this comparative experiment, we see that the receptacles in which the blood was ultimately contained were precisely similar in the two cases, viz. glass tubes closed below with gutta percha; and that the blood which was simply poured into the tube was much less exposed to the air than the other, and also was not subjected, like it, to elevation of temperature, a circumstance which promotes coagulation; but yet this blood became completely coagulated in a comparatively short time, whereas the other after a much

longer time was coagulated only in a layer in contact with the foreign solid. But in the latter case the blood had been so introduced as to avoid direct action of ordinary matter on any but the circumferential parts of it; whereas in the former, though poured quickly, it had run down the side of the glass, and as a consequence of this almost momentary contact with the foreign solid, the central parts, like the circumferential, underwent the process of coagulation.

Mysterious as this subtle agency of ordinary solids must appear, its occurrence is thus matter of experimental demonstration, and by it the coagulation of blood shed into a basin is accounted for; while it is also shown conclusively from this experiment that the blood, as it exists within the vessels, has no spontaneous tendency to coagulate, and therefore that the notion of any action on the part of the blood-vessels to prevent coagulation is entirely out of the question. The peculiarity of the living vessels consists not in any such action upon the blood, but in the circumstance, remarkable indeed as it is, that their lining membrane, when in a state of health, is entirely negative in its relation to coagulation, and fails to cause that molecular disturbance or, if we may so speak, catalytic action which is produced upon the blood by all ordinary matter.

I afterwards found that the simplest method of maintaining blood fluid in a vessel composed entirely of ordinary matter was to employ a glass tube similar to those above described, except that its upper end was closed by a cork perforated by a narrow tube terminating in a piece of vulcanized india-rubber tubing that could be closed by a clamp. This tube was slipped down into a vein till the blood, having filled it completely, showed itself at the orifice of the india-rubber tubing, to which the clamp was then applied. The whole apparatus was now quickly inverted, and the vein was drawn off from over the mouth of the tube, which was then covered with gutta-percha tissue to prevent evaporation. After the inverted tube had been kept undisturbed in the vertical position for nineteen hours and three quarters, coagulable blood was obtained from the interior of the clot.

We have seen that a clot has but very slight tendency to induce coagulation in its vicinity unless the blood has been acted on by an ordinary solid; and it is probable that with perfectly healthy blood

it would be unable to produce such an effect at all. This appears to me to be very interesting physiologically, but especially so with reference to pathology. I must not go now fully into the circumstances that lead me to it; but I may express the opinion I have formed, that clot must be regarded as living tissue in its relation to the blood. It is no doubt a very peculiar form of tissue, in this respect, that it is soft, easily lacerable, and easily impaired in its vital properties. If disturbed, as in an aneurism, it will readily be brought into that condition which leads to the deposition of more clot; but if undisturbed, it not only fails to induce further coagulation, but seems to undergo spontaneous organization. I have seen a clot in the right side of the heart, and extending into the pulmonary artery and its branches, unconnected with the lining membrane of auricle or ventricle or with the pulmonary artery except at one small spot where it had a slight adhesion, developed into perfect fibrous tissue by virtue, it would appear, of its own inherent properties. Another observation which I once made, and which then completely puzzled me, now seems capable of explanation. In laying open the blood-vessels of a dead body, I observed in many of the veins a delicate white lace-like tissue which evidently must have been formed from a clot. This I now believe to have had the same relation to the coagulum as the flimsy cellular tissue of old adhesions has to lymph.

It may not be altogether superfluous to mention some other facts illustrative of the active influence of ordinary matter in promoting coagulation, and the negative character of the lining membrane of the vessels. I find that a needle introduced into one of the veins of the foot of a sheep for a much shorter time than is necessary to produce the first appearance of the actual deposit of fibrin upon it, leads after a while to coagulation where the needle had lain—in other words, that a foreign solid, by a short period of action on the blood, brings about a change that results in coagulation, though the blood still lies in the living vessels. I have also ascertained that after blood has been made to coagulate in a particular vessel by introducing a needle into it, if the coagulum as well as needle is removed, and more fluid blood is allowed to pass in, this blood remains fluid for an indefinite period, showing that the needle had not impaired the properties of the vessel by its presence; so that the previous coagulation

must be attributed not to any loss of power in the vein, but simply to the action of the foreign solid.

In seeking for an analogy to this remarkable effect of ordinary solids upon the blood, we are naturally led to the beautiful observations of Professor Graham, lately published in the Philosophical Transactions. He has there shown what insignificant causes are often sufficient to induce a change from the fluid or soluble to the "pectous," or insoluble condition of "colloidal" forms of matter. Indeed Mr. Graham has himself alluded to the coagulation of fibrin as being probably an example of such a transition.

There is, however, another remarkable circumstance that must be taken into consideration, of which I myself have been only recently aware, and which may be new to several Fellows of the Society; and that is, that in spite of the influence of an ordinary solid the liquor sanguinis is not capable of coagulating *per se*. It was observed many years ago by my colleague, Professor Andrew Buchanan, of Glasgow, that the fluid of a hydrocele, generally regarded as mere serum, coagulated firmly if a little coagulum of blood diffused in water was added to it—an effect which he was disposed to attribute to the agency of the white corpuscles*. I repeated Dr. Andrew Buchanan's observations last year, and satisfied myself first that the diffused clot did not act simply by providing solid particles to serve as starting-points for the coagulating process. I tried various different materials in a finely divided state, and found that none of them, except blood, produced the slightest effect. But I found that if a mixture of serum and red corpuscles from a clot was added to some of this hydrocele-fluid, it was soon converted into a firm solid mass. If a small quantity of the serum and corpuscles was dropped into the fluid and allowed to subside without stirring, coagulation rapidly took place in those parts where the red corpuscles lay, while other parts of the fluid remained for a long time uncoagulated. This seemed to indicate that the red corpuscles had a special virtue in inducing the change. I confess, however, that till very lately I was inclined to suppose that in the hydrocele-fluid the fibrin must be in some peculiar spurious form. We know that the buffy coat of the horse's blood coagulates in a glass without addition of clot, and we know that lymph coagulates; so that I did not doubt

* Proceedings of the Glasgow Philosophical Society, February 19, 1845.

that liquor sanguinis would always undergo the change when influenced by ordinary matter. But an observation which I made not many days ago, shows that this was a mistake. I obtained the jugular vein of a horse, and having kept it for a while in a vertical position till I could see through its transparent coats that the red corpuscles had fallen from the upper part, I removed all bloody tissue from that part of the vein, and punctured it so as to let out the liquor sanguinis into a glass. Finding after eighteen minutes that the liquid had not begun to coagulate, I added a drop of serum and corpuscles to a portion of it, and within seven minutes there was a clot wherever the corpuscles lay, whereas the rest of the fluid was still very imperfectly coagulated after another half hour had elapsed. That the liquor sanguinis to which no addition had been made coagulated at all, was sufficiently explained by microscopic investigation, which showed not only abundant white corpuscles, but also several isolated red ones that had not subsided. This observation was made three hours after the death of the horse, but I obtained essentially similar results on repeating the experiment in another horse an hour after death; so that there can be no doubt whatever that the fibrin was in the same condition as it is in the blood-vessels of a living animal. The observation appears also particularly satisfactory on this account, that the liquor sanguinis was not separated from the corpuscles by any process of transudation through the walls of the blood-vessels, which might be conceived to involve retention of some constituent of the liquid, which, though in solution, might be unable to pass through their pores, but simply by the subsidence of the corpuscles, which must have left all the materials of the liquor sanguinis behind them. Hence it is proved beyond question that if the liquor sanguinis could be separated completely from the blood-corpuscles, it would resemble the fluid of hydrocele in being incapable of coagulation when shed into a cup.

Now this struck me as a very satisfactory and beautiful truth, inasmuch as it clears away all the old mystery of the distinction between inflammatory exudations and dropsical effusions. Dropsical effusions, exhibiting little disposition to coagulate, have been supposed to consist almost exclusively of serum, and the exudation of the entire liquor sanguinis has been regarded as the special characteristic of inflammation; and very unsatisfactory theories have been

put forward by ingenious pathologists to account for this difference. But it now appears that a dropsical effusion, like that of hydrocele, is undistinguishable from pure liquor sanguinis.

Various dropsical effusions have been lately investigated with reference to their coagulability on the addition of blood-corpuscles, by Dr. Schmidt of Dorpat, who finds that while they differ from one another in the amount of water they contain (just as is the case with serum filtered artificially through animal membranes under different degrees of pressure), yet they are all but universally coagulable. Schmidt has also carried the investigation further. He has found that by chemical means he can extract from the red corpuscles a soluble material which, when added to these exudations, leads to coagulation. In other words, he shows that the corpuscles do not act as living cells, but by virtue of a chemical material which they contain, which can be used in the state of solution, free from any solid particles whatever. He found also that the aqueous humour made a dropsical effusion coagulate, and that the same effect was produced by a material extracted from the non-vascular part of the cornea. Hence he regards the blood-corpuscles as only resembling other forms of tissue in possessing this property. These observations are extremely interesting, if trustworthy; and that they are so, I do not at all doubt; but having only read Schmidt's papers within the last day or two, I have not yet had opportunity of verifying his statements*.

It remains to be ascertained what share the material derived from the corpuscles has in the composition of the fibrin. Schmidt inclines to the opinion that the fibrin is probably composed, in about equal proportions, of a substance furnished by them and one present in the liquor sanguinis. If this be true, the action of an ordinary solid in determining the union of the components of the fibrin may be compared to the operation of spongy platinum in promoting the combination of oxygen and hydrogen.

* Since this lecture was delivered I have verified an important observation made by Schmidt, viz. that a given amount of corpuscles causes complete coagulation of only a limited quantity of hydrocele-fluid. From this he draws the inference, that the action of the corpuscles cannot be of the nature of fermentation—the coagulative efficacy of the corpuscles being not continued indefinitely, but becoming exhausted in the process of coagulation. For Schmidt's papers, see Archiv für Anat. Phys. &c. 1861 and 1862.

It may be asked, How comes it that when the blood of a horse is shed into a cup, the buffy layer coagulates as rapidly, or nearly so, as the lower parts rich in corpuscles?

This is indeed a question well worthy of careful study. We know that the liquor sanguinis left by the subsidence of the red corpuscles within a healthy vein is incapable of coagulating when shed, except in a slow manner, which is accounted for by the corpuscles that remain behind in it. Hence it appears that when the blood as a whole is shed into a glass, the agency of the ordinary solid leads the corpuscles to communicate to the liquor sanguinis, before they subside, a material or at least an influence which confers upon it a disposition to coagulate, though it still remains fluid for some time after they have left it. Just as we have seen that a very short time of action of the ordinary solid upon the blood as a whole is sufficient to give rise to coagulation, so we now see that, provided an ordinary solid be in operation, the presence of the corpuscles for but a little while is enough to make the liquor sanguinis spontaneously coagulable, though not immediately solidified. We shall see, before concluding, an illustration of the importance of this fact to pathology.

It remains to be added, that serous membranes resemble the lining membrane of the blood-vessels in their relations to the blood, as is implied by John Hunter's observation that blood, which had lain for several days in a hydrocele, coagulated when let out. The same thing is well illustrated in a frog prepared like this which I now exhibit. About four hours ago, a knife having been passed between the brain and cord to deprive the creature of voluntary motion in the limbs and trunk, the peritoneal cavity was laid open in the middle line, and its edges being kept raised and drawn aside by pins, I seized the apex of the ventricle of the heart with forceps and removed it with scissors. In a short time the whole of the animal's blood was in the peritoneum, and it may be seen that it is still fluid in spite of this long-continued exposure. When I first performed the experiment three years and a half ago, the weather being cool (about 45° Fahr.) and a piece of damp lint being kept suspended above the frog to prevent evaporation and access of dust, I found that the blood remained fluid in the peritoneal cavity for four days, except a thin film on the surface, and a crust of clot on the wounded part of the heart; but a piece of clean glass placed in the blood in the

peritoneum became speedily coated with coagulum. Here, it will be observed, not merely the liquor sanguinis, but the corpuscles also were present in the serous cavity, yet no coagulation took place in contact with its walls.

I think it probable, though not yet proved, that all living tissues have these properties with reference to the blood. We know that the interstices of the cellular tissue contain coagulable fluid, and I have seen anasarcous liquid coagulate after emission; but this indeed may possibly have been merely liquor sanguinis, coagulating in consequence of slight admixture of blood-corpuscles from the wounds made in obtaining it.

Looking now at the principal results which we have arrived at, it must, in the first place, be admitted that the ammonia theory is to be discarded as entirely fallacious. The fact that this theory is exceedingly plausible, and has been supported by many ingenious arguments and experiments, is of course no reason why we should retain it if unsound. On the contrary, the more specious it is the more necessary is it that it should be effectually cleared away; for it mystifies the subject of coagulation most seriously; and I may say, for my own part, that it has cost me an amount of experimental labour of which the illustrations brought forward this evening convey but little idea. Still these have been, I trust, sufficient to show that the coagulation of the blood is in no degree connected with the evolution of ammonia, any more than with the influence of oxygen or of rest. The real cause of the coagulation of the blood, when shed from the body, is the influence exerted upon it by ordinary matter, the contact of which for a very brief period effects a change in the blood, inducing a mutual reaction between its solid and fluid constituents, in which the corpuscles impart to the liquor sanguinis a disposition to coagulate. This reaction is probably simply chemical in its nature; yet its product, the fibrin, when mixed with blood-corpuscles in the form of an undisturbed coagulum, resembles healthy living tissues in being incapable of that catalytic action upon the blood which is effected by all ordinary solids, and also by the tissues themselves when deprived of their vital properties.

These principles have, of course, very extensive applications to the study of disease; but I must content myself with alluding very

briefly to inflammation, the most important of all pathological conditions.

If we inquire what is the great peculiarity of inflamed parts in relation to the blood as examined by the naked eye, we see that it consists in a tendency to induce coagulation in their vicinity—implying, according to the conclusions just stated, that the affected tissues have lost for the time being their vital properties, and comport themselves like ordinary solids. Thus, when an artery or vein is inflamed, coagulation occurs upon its interior, in spite of the current of blood, precisely as would take place if it had been artificially deprived of its vital properties. On one occasion I simulated the characteristic adherent clot of Phlebitis by treating the jugular vein of a living sheep with caustic ammonia, and then allowing the circulation to go on through the vessel for a while, when, on slitting it up, I found its lining membrane studded with grains of pink fibrin which could be detached only by scraping firmly with the edge of a knife. Again, comparing an inflammatory exudation into the pericardium or into the interstices of the cellular tissue with dropsical effusions into the same situations, we are struck with the fact that, while the liquor sanguinis effused in dropsy remains fluid, the inflammatory product coagulates. Now we know that in intense inflammation the capillaries are choked more or less with accumulated blood-corpuscles, which must cause great increase in the pressure of the blood upon their walls; and from what we know of the effect of venous obstruction in causing dropsical effusion of liquor sanguinis through increased pressure, we are sure that we have in the inflammatory state the physical conditions for a similar transudation of fluid through the walls of the capillaries. And the natural interpretation of the difference in the two cases as regards coagulation seems to be, that whereas in dropsy the fluid is forced through the pores of healthy vessels, in inflammation the capillary parietes have lost their healthy condition, and act like ordinary matter; so that the liquor sanguinis, having been subjected, immediately before effusion, to the combined influence of the injured tissue and the blood-corpuscles, has acquired a disposition to coagulate, just like the buffy coat of horses' blood shed into a glass, or like the frog's liquor sanguinis filtered by Müller from its corpuscles, the injured vessels acting upon the blood like the filter.

This view of the condition of intensely inflamed parts is exactly that to which I was led some years ago by a microscopic investigation, the results of which were detailed in a paper* that received the honour of a place in the Philosophical Transactions. It was there shown, as I think I may venture to say, that the tissues generally are capable of being reduced under the action of irritants to a state quite distinct from death, but in which they are nevertheless temporarily deprived of all vital power, and that inflammatory congestion is due to the blood-corpuscles acquiring adhesiveness such as they have outside the body, in consequence of the irritated tissues acting towards them like ordinary solids.

I cannot avoid expressing my satisfaction that this inquiry into the coagulation of the blood has furnished independent confirmation of my previous conclusions regarding the nature of inflammation.

June 18, 1863.

Major-General SABINE, President, in the Chair.

E. W. Cooke, Esq., James Fergusson, Esq., The Rev. R. Harley, W. Pengelly, Esq., and H. E. Roscoe, Esq., were admitted into the Society.

Pursuant to notice given at the last Meeting, Professor Ernst Edward Kummer, of Berlin, and Professor Johannes Japetus Smith Steenstrup, of Copenhagen, were balloted for and elected Foreign Members of the Society.

The following communications were read:—

I. "On the Molecular Mobility of Gases." By THOMAS GRAHAM, F.R.S., Master of the Mint. Received May 7, 1863.

(Abstract).

The molecular mobility of gases is here considered in reference chiefly to the passage of gases, under pressure, through a thin porous plate or septum, and to the partial separation of mixed gases which can be effected, as will be shown, by such means. The investigation

* "On the Early Stages of Inflammation," Phil. Trans. for 1858.

arose out of a renewed and somewhat protracted inquiry regarding the diffusion of gases (depending upon the same molecular mobility), and has afforded certain new results which may prove to be of interest in a theoretical as well as in a practical point of view.

In the diffusiometer, as first constructed, a plain cylindrical glass tube, rather less than an inch in diameter and about ten inches in length, was simply closed at one end by a porous plate of plaster of paris, about one-third of an inch in thickness, and thus converted into a gas receiver*. A superior material for the porous plate is now found in the artificially compressed graphite of Mr. Brockedon, of the quality used for making writing-pencils. This material is sold in London in small cubic masses about 2 inches square. A cube may easily be cut into slices of a millimetre or two in thickness by means of a saw of steel spring. By rubbing the surface of the slice without wetting it upon a flat sand-stone, the thickness may be further reduced to about one-half of a millimetre. A circular disk of this graphite, which is like a wafer in thickness but possesses considerable tenacity, is attached by resinous cement to one end of the glass tube above described, so as to close it and form a diffusiometer. The tube is filled with hydrogen gas over a mercurial trough, the porosity of the graphite plate being counteracted for the time by covering it tightly with a thin sheet of gutta percha. On afterwards removing the latter, gaseous diffusion immediately takes place through the pores of the graphite. The whole hydrogen will leave the tube in forty minutes or an hour, and is replaced by a much smaller proportion of atmospheric air (about one fourth), as is to be expected from the law of the diffusion of gases. During the process, the mercury will rise in the tube, if allowed, forming a column of several inches in height—a fact which illustrates strikingly the intensity of the force with which the interpenetration of different gases is effected. The native or mineral graphite is of a lamellar structure, and appears to have little or no porosity. It cannot be substituted for the artificial graphite as a diffusion-septum. Unglazed earthenware comes next in value to graphite for this purpose.

The pores of artificial graphite appear to be really so minute, that

* "On the Law of the Diffusion of Gases," Transactions of the Royal Society of Edinburgh, vol. xii. p. 222; or Philosophical Magazine, 1834, vol. ii. pp. 175, 269, 351.

a gas *in mass* cannot penetrate the plate at all. It seems to be molecules only which can pass; and these may be supposed to pass wholly unimpeded by friction, for the smallest pores that can be imagined to exist in the graphite must be tunnels in magnitude to the ultimate atoms of a gaseous body. The sole motive agency appears to be that intestine movement of molecules which is now generally recognized as an essential property of the gaseous condition of matter.

According to the physical hypothesis now generally received*, a gas is represented as consisting of solid and perfectly elastic spherical particles or atoms, which move in all directions, and are animated with different degrees of velocity in different gases. Confined in a vessel, the moving particles are constantly impinging against its sides and occasionally against each other, and such collisions take place without any loss of motion, owing to the perfect elasticity of the particles. Now if the containing vessel be porous, like a diffusiometer, then gas is projected through the open channels, by the atomic motion described, and escapes. Simultaneously the external air or gas, whatever it may be, is carried inwards in the same manner, and takes the place of the gas which leaves the vessel. To the same atomic or molecular movement is due the elastic force, with the power to resist compression, possessed by gases. The molecular movement is accelerated by heat and retarded by cold, the tension of the gas being increased in the first instance and diminished in the second. Even when the same gas is present both within and without the vessel, and is therefore in contact with both sides of the porous plate, the movement is sustained without abatement—molecules continuing to enter and leave in equal number, although nothing of the kind is indicated by change of volume or otherwise. If the gases in communication be different but possess sensibly the same specific gravity and molecular velocity, as nitrogen and carbonic oxide do, an interchange of molecules also takes place without any change in volume. With gases opposed of unequal density and molecular velocity, the amount of penetration ceases of course to be equal in both directions.

* D. Bernoulli, J. Herapath, Joule, Krönig, Clausius, Clerk Maxwell, and Cazin. The merit of reviving this hypothesis and first applying it to the facts of gaseous diffusion, is fairly due to Mr. Herapath. See 'Mathematical Physics,' in two volumes, by John Herapath, Esq. (1847).

These observations are preliminary to the consideration of the passage through a graphite plate, in one direction only, of gas under pressure, or under the influence of its own elastic force. It is to be supposed that a vacuum is maintained on one side of the porous septum, and that air or some other gas, under a constant pressure, is in contact with the other side. Now a gas may pass into a vacuum in three different modes, or in two modes besides that immediately before us.

1. The gas may enter the vacuum by passing through a minute aperture in a thin plate, such as a puncture in platinum foil made by a fine steel point. The rate of passage of different gases is then regulated by their specific gravities, according to a pneumatic law which was deduced by Professor John Robison from Torricelli's well-known theorem of the velocity of efflux of fluids. A gas rushes into a vacuum with the velocity which a heavy body would acquire by falling from the height of an atmosphere composed of the gas in question, and supposed to be of uniform density throughout. The height of the uniform atmosphere will be inversely as the specific gravity of the gas, the atmosphere of hydrogen, for instance, sixteen times higher than that of oxygen. But as the velocity acquired by a heavy body in falling is not directly as the height, but as the square root of the height, the rate of flow of different gases into a vacuum will be inversely as the square root of their respective densities. The velocity of oxygen being 1, that of hydrogen will be 4, the square root of 16. This law has been experimentally verified*. The times of the effusion of gases, as I have spoken of it, are similar to those of the law of molecular diffusion; but it is important to observe that the phenomena of effusion and diffusion are distinct and essentially different in their nature. The effusion movement affects masses of gas, the diffusion movement affects molecules; and a gas is usually carried by the former kind of impulse with a velocity many thousand times greater than by the latter. The effusion velocity of air is the same as the velocity of sound.

2. If the aperture of efflux be in a plate of increased thickness, and so becomes a tube, the effusion-rates of gases are disturbed. The rates of flow of different gases, however, assume again a constant ratio to each other when the capillary tube is considerably

* "On the Motion of Gases," Phil. Trans. 1846, p. 573

elongated, when the length exceeds the diameter at least 4000 times. These new proportions of efflux are the rates of the "Capillary Transpiration of Gases" *. The rates were found to be the same in a capillary tube composed of copper as they are in a tube of glass, and appear to be independent of the material of the capillary. A film of gas no doubt adheres to the inner surface of the tube, and the friction is really that of gas upon gas, and is consequently unaffected by the nature of the tube-substance. The rates of transpiration are not governed by specific gravity, and are indeed singularly unlike the rates of effusion.

The transpiration-velocity of oxygen being 1, that of chlorine is 1·5, that of hydrogen 2·26, of ether vapour at low temperatures the same or nearly the same number as hydrogen, of nitrogen and carbonic oxide half the velocity of hydrogen, of olefiant gas, ammonia, and cyanogen 2 (double or nearly double that of oxygen), of carbonic acid 1·376, and of the gas of marshes 1·815. In the same gas the transpirability of equal volumes increases with density, whether occasioned by cold or pressure. The transpiration-ratios of gases appear to be in constant relation with no other known property of the same gases, and they form a class of phenomena remarkably isolated from all else at present known of gases.

There is one property of transpiration immediately bearing upon the penetration of the graphite plate by gases. The capillary offers to the passage of gas a resistance analogous to that of friction, proportional to the surface, and consequently increasing as the tube or tubes are multiplied in number and diminished in diameter, with the area of discharge preserved constant. The resistance to the passage of a liquid through a capillary was observed by Poiseuille to be nearly as the fourth power of the diameter of the tube. In gases the resistance also rapidly increases; but in what ratio, has not been observed. The consequence, however, is certain, that as the diameter of the capillaries may be diminished beyond any assignable limit, so the flow may be retarded indefinitely, and caused at last to become too small to be sensible. We may therefore have a mass of capillaries of which the passages form a large aggregate, but which are individually too small to permit a sensible flow of gas under pressure. A porous solid mass may possess the same reduced penetrability as

* Phil. Trans. 1846, p. 591, and 1849, p. 349.

the congeries of capillary tubes. Indeed the state of porosity described appears to be more or less closely approached by all loosely aggregated mineral masses, such as lime plaster, stucco, chalk, baked clay, non-crystalline earthy powders like hydrate of lime or magnesia compacted by pressure, and in the highest degree perhaps by artificial graphite.

3. A plate of artificial graphite, although it appears to be practically impenetrable to gas by either of the two modes of passage previously described, is readily penetrated by the agency of the molecular or diffusive movement of gases. This appears on comparing the time required for the passage of equal volumes of different gases under a constant pressure. Of the following three gases, oxygen, hydrogen, and carbonic acid, the time required for the passage of an equal volume of each through a capillary glass tube, in similar circumstances as to pressure and temperature, was formerly observed to be as follows :—

	Time of capillary transpiration.
Oxygen	1
Carbonic acid	0·72
Hydrogen	0·44

Through a plate of graphite, of half a millimetre in thickness, the same gases were now observed to pass, under a constant pressure of a column of mercury of 100 millimetres in height, in times which are as follows :—

	Time of molecular passage.	Square root of density (oxygen 1).
Oxygen	1	1
Hydrogen	0·2472	0·2502
Carbonic acid	1·1886	1·1760

It appears then that the times of passage through the graphite plate have no relation to the capillary transpiration-times of the same gases first quoted above. The new times in question, however, show a close relation to the square roots of the densities of the respective gases, as is seen in the last Table; and so far they agree with theoretical *times of diffusion* usually ascribed to the same gases.

The experiments were varied by causing the gases to pass into a Torricellian vacuum, and consequently under the full pressure of the

atmosphere. The times of penetration of equal volumes of gases were now—

	Times.		$\sqrt{\text{Density}}$.
Oxygen	1	....	1
Air	0·9501	....	0·9507
Carbonic acid	1·1860	....	1·1760
Hydrogen	0·2505	....	0·2502

This penetration of the graphite plate by gases appears to be entirely due to their own proper molecular motion, quite unaided by transpiration. It seems to offer the simplest possible exhibition of the molecular or diffusive movement. This pure result is to be ascribed to the wonderfully fine porosity of the graphite. The interstitial spaces, or channels, appear to be sufficiently small to extinguish transpiration, or the passage of masses, entirely. The graphite becomes a molecular sieve, allowing molecules only to pass through.

With a plate of stucco, the penetration of gases under pressure is very rapid, and the volumes of air and hydrogen passing in equal times are as 1 to 2·891, which is a number for hydrogen intermediate between its transpiration-volume 2·04 and diffusion-volume 3·8, showing that the passage through stucco is a mixed result.

With a plate of biscuitware, 2·2 millimetres in thickness, the volume of hydrogen rose to 3·754 (air = 1), approaching closely to 3·8, the molecular ratio.

The rate of passage of a gas through graphite appeared also to be closely proportional to the pressure.

Further, hydrogen was found to penetrate through a graphite plate into a vacuum, with sensibly the same absolute velocity as it diffused into air, establishing the important fact that the impelling force is the same in both movements. The molecular mobility may therefore be spoken of as the diffusive movement of gases; the passage of gas through a porous plate into vacuum, as diffusion in one direction or single diffusion; and ordinary diffusion, or the passage of two gases in opposite directions, as double, compound, or reciprocal diffusion.

Atmolysis.—A partial separation of mixed gases and vapours of unequal diffusibility can be effected by allowing the mixture to permeate through a graphite plate into a vacuum, as was to be expected from the preceding views. As this method of analysis has a practical character and admits of wide application, it may be convenient to

distinguish it by a peculiar name. The amount of the separation is in proportion to the pressure, and attains its maximum when the gases pass into a nearly perfect vacuum. A variety of experiments were made on this subject, of which perhaps the most interesting were those upon the concentration of the oxygen in atmospheric air. When a portion of air confined in a jar is allowed to penetrate into a vacuum through graphite or unglazed earthenware, the nitrogen should pass more rapidly than the oxygen in the proportion of 1·0668 to 1, and the proportion of oxygen be proportionally increased in the air left behind in the jar. The increase in the oxygen actually observed when the air in the jar was reduced from 1 volume

To 0·5	volume,	was	0·48	per cent.
0·25	,,	,,	0·98	,,
0·125	,,	,,	1·54	,,
0·0625	,,	,,	2·02	,,

Or, the oxygen increased from 21 to 23·02 per cent. in the last sixteenth part of air left behind in the jar.

The most remarkable effects of separation are produced by means of the *tube atmolyser*. This is simply a narrow tube of unglazed earthenware, such as a tobacco-pipe stem two feet in length, which is placed within a shorter tube of glass and secured in its position by corks, so as to appear like a Liebig's condenser. The glass tube is placed in communication with an air-pump, and the annular space between the two tubes is maintained as nearly vacuous as possible. Air or any other mixed gas is then allowed to flow in a stream along the clay tube, and collected as it issues. The gas so atmolysed is of course reduced in volume, much gas penetrating through the pores of the clay tube into the air-pump vacuum; and the slower the gas is collected the greater the proportional loss. In the gas collected, the denser constituent of the mixture is thus concentrated in an arithmetical ratio, while the volume of the gas is reduced in a geometrical ratio. In one experiment the proportion of oxygen in the air after traversing the atmolyser was increased to 24·5 per cent., or 16·7 upon 100 oxygen originally present in the air. With gases differing so much in density and diffusibility as oxygen and hydrogen, the separation is of course much more considerable. The explosive mixture of two volumes of hydrogen and one volume of oxygen, gave oxygen con-

taining only 9·3 per cent. of hydrogen, in which a taper burned without explosion; and with equal volumes of oxygen and hydrogen, the proportion of the latter was easily reduced from 50 to 5 per cent.

Interdiffusion of Gases—double diffusion.—The diffusiometer was much improved in construction by Prof. Bunsen, from the application of a lever arrangement to raise and depress the tube in the mercurial trough. But the mass of stucco forming the porous plate in his instrument was too voluminous, in my opinion, and, from being dried by heat, had probably detached itself from the walls of the glass tube. The result obtained of 3·4 for hydrogen, air being 1, is, I understand, no longer insisted upon by that illustrious physicist. It is indeed curious that my old experiments generally rather exceeded than fell short of the theoretical number for hydrogen, $\sqrt{\frac{1}{0 \cdot 06926}} = 3 \cdot 7997$. With stucco as the material, the cavities in the porous plate form about one-fourth of its bulk, and affect sensibly the ratio in question, according as they are or are not included in the capacity of the instrument. Beginning the diffusion always with these cavities filled with hydrogen, the numbers now obtained with a stucco plate of 12 millims. in thickness, dried without heat, were 3·783, 3·8, and 3·739 when the volume of the cavities of stucco is added to the air and hydrogen, and 3·931, 3·949, and 3·883 when such addition is not made to these volumes. The graphite plate, on the other hand, being thin, and the volume of its pores too minute to require to be taken into account, its action is not attended with the same uncertainty. With a graphite plate of 2 millims. in thickness, the number for hydrogen into air was 3·876, and of hydrogen into oxygen 4·124, instead of 4. With a graphite plate of 1 millim. in thickness, hydrogen gave 3·993 to air 1. With a graphite plate of 0·5 millim. in thickness, the proportional number for hydrogen to air rose to 3·984, 4·068, and 4·067. A similar departure from the theoretical number was observed when hydrogen was diffused into oxygen or carbonic acid, instead of air. All these experiments were made over mercury and with dried gases. It appears that the numbers are most in accordance with theory when the graphite plate is thick, and the diffusion slow in consequence. If the diffusion be very rapid, as it is with the thin plates, something like a current is possibly formed in the channels of the graphite,

taking the direction of the hydrogen and carrying back in mass a little air, or the slower gas, whatever it may be. I cannot account otherwise for the slight predominance which the lighter and faster gas appears always to acquire in diffusing through the porous septum.

Speculative ideas respecting the constitution of matter.

It is conceivable that the various kinds of matter, now recognized as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies. We know the anxiety with which this point was investigated by Newton, and the care he took to ascertain that every kind of substance, "metals, stones, woods, grain, salts, animal substances, &c.," are similarly accelerated in falling, and are therefore equally heavy.

In the condition of gas, matter is deprived of numerous and varying properties with which it appears invested when in the form of a liquid or solid. The gas exhibits only a few grand and simple features. These again may all be dependent upon atomic and molecular mobility. Let us imagine one kind of substance only to exist, ponderable matter; and further, that matter is divisible into ultimate atoms, uniform in size and weight. We shall have one substance and a common atom. With the atom at rest the uniformity of matter would be perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific motion of an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements as they have been considered.

What has already been said is not meant to apply to the gaseous volumes which we have occasion to measure and practically deal with, but to a lower order of molecules or atoms. The combining atoms hitherto spoken of are therefore not the molecules of which the movement is sensibly affected by heat, with gaseous expansion as

the result. The gaseous molecule must itself be viewed as composed of a group or system of the preceding inferior atoms, following as a unit laws similar to those which regulate its constituent atoms. We have indeed carried one step backward and applied to the lower order of atoms ideas suggested by the gaseous molecule, as views derived from the solar system are extended to the subordinate system of a planet and its satellites. The advance of science may further require an indefinite repetition of such steps of molecular division. The gaseous molecule is then a reproduction of the inferior atom on a higher scale. The molecule or system is reached which is affected by heat, the diffusive molecule, of which the movement is the subject of observation and measurement. The diffusive molecules are also to be supposed uniform in weight, but to vary in velocity of movement, in correspondence with their constituent atoms. Accordingly the molecular volumes of different elementary substances have the same relation to each other as the subordinate atomic volumes of the same substances.

But further, these more and less mobile or light and heavy forms of matter have a singular relation connected with equality of volume. Equal volumes of two of them can coalesce together, unite their movement, and form a new atomic group, retaining the whole, the half, or some simple proportion of the original movement and consequent volume. This is chemical combination. It is directly an affair of volume, and only indirectly connected with weight. Combining weights are different, because the densities, atomic and molecular, are different. The volume of combination is uniform, but the fluids measured vary in density. This fixed combining measure—the *metron* of simple substances—weighs 1 for hydrogen, 16 for oxygen, and so on with the other "elements."

To the preceding statements respecting atomic and molecular mobility, it remains to be added that the hypothesis admits of another expression. As in the theory of light we have the alternative hypotheses of emission and undulation, so in molecular mobility the motion may be assumed to reside either in separate atoms and molecules, or in a fluid medium caused to undulate. A special rate of vibration or pulsation originally imparted to a portion of the fluid medium enlivens that portion of matter with an individual existence, and constitutes it a distinct substance or element.

With respect to the different states of gas, liquid, and solid, it may be observed that there is no real incompatibility with each other in these physical conditions. They are often found together in the same substance. The liquid and the solid conditions supervene upon the gaseous condition rather than supersede it. Gay-Lussac made the remarkable observation that the vapours emitted by ice and water, both at 0° C., are of exactly equal tension. The passage from the liquid to the solid state is not made apparent in the volatility of water. The liquid and solid conditions do not appear as the extinction or suppression of the gaseous condition, but something *superadded* to that condition. The three conditions (or constitutions) probably always coexist in every liquid or solid substance, but one predominates over the others. In the general properties of matter we have, indeed, to include still further (1) the remarkable loss of elasticity in vapours under great pressure, which is distinguished by Mr. Faraday as the Caignard-Latour state, after the name of its discoverer, and is now undergoing an investigation by Dr. Andrews, which may be expected to throw much light upon its nature; (2) the colloidal condition or constitution, which intervenes between the liquid and crystalline states, extending into both and affecting probably all kinds of solid and liquid matter in a greater or less degree. The predominance of a certain physical state in a substance appears to be a distinction of a kind with those distinctions recognized in natural history as being produced by unequal development. Liquefaction or solidification may therefore not involve the suppression of either the atomic or the molecular movement, but only the restriction of its range. The hypothesis of atomic movement has been elsewhere assumed, irrespective of the gaseous condition, and is applied by Dr. Williamson to the elucidation of a remarkable class of chemical reactions which have their seat in a mixed liquid.

Lastly, molecular or diffusive mobility has an obvious bearing upon the communication of heat to gases by contact with liquid or solid surfaces. The impact of the gaseous molecule, upon a surface possessing a different temperature, appears to be the condition for the transference of heat, or the heat movement, from one to the other. The more rapid the molecular movement of the gas the more frequent the contact, with consequent communication of heat. Hence, probably, the great cooling power of hydrogen gas as compared with air

or oxygen. The gases named have the same specific heat for equal volumes; but a hot object placed in hydrogen is really *touched* 3·8 times more frequently than it would be if placed in air, and 4 times more frequently than it would be if placed in an atmosphere of oxygen gas. Dalton had already ascribed this peculiarity of hydrogen to the high "mobility" of that gas. The same molecular property of hydrogen recommends the application of that gas in the air-engine, where the object is to alternately heat and cool a confined volume of gas with rapidity.

II. "Results of the Magnetic Observations at the Kew Observatory, from 1858 to 1862 inclusive."—No. I. By Major-General EDWARD SABINE, P.R.S. Received May 21, 1863.

(Abstract.)

The first three sections of this paper are occupied by a discussion of the Laws of the Disturbances of the Magnetic Declination at Kew, derived from the photographic records of the Kew Observatory between January 1, 1858, and December 31, 1862. In the first section a synoptical table is given, showing the direction and amount of the easterly and of the westerly deflections of the declination magnet at 24 equidistant epochs on each of 95 days of principal disturbance occurring in the years 1858 to 1862 inclusive. The deflections are measured from the normals of the same month and hour, computed from the undisturbed positions at the same epochs on the 1825 days comprised in the five years since the commencement of the photographic records. The phenomenal laws of the disturbances on the 95 days are then investigated, and are compared with the corresponding laws derived from a far larger number of observations in the same years, taken out by the well-known process employed by the author in the reduction of the observations of the colonial magnetic observatories. The result is shown to be that, so far as the laws of the disturbances are concerned, the two processes furnish mutual confirmation—the laws being approximately the same whether they are derived from the whole body of the hourly positions, or from that portion only which includes 95 days (or on an average 19 days in each year) which were specially affected by disturbance,—but that, for the purpose of eliminating the effects of the disturbances in the

subsequent investigation of the secular, periodical, and other minor magnetic variations, the process of elimination introduced by the author and employed by him for several years past in the reduction of the colonial observations has the advantage of separating from the whole body of the observations a far greater portion of the disturbing influence than would be gained by the simple omission of the observations on the 95 days. The laws of the *disturbance-diurnal* variation, thus found to be approximately the same whether obtained from the narrower or from the wider basis of investigation, are then stated, and are compared with the results of similar investigations recorded in the author's previous publications—the points of accordance or of difference being severally discussed in the third section.

The fourth section contains Tables of the "Diurnal Inequality," and of the "Solar-diurnal Variation" at Kew, showing the mean values at each hour and in each month. The "Diurnal Inequality" is explained as consisting of two principal constituents, viz. the "Disturbance-diurnal Variation," and the "Solar-diurnal Variation." It is obtained for each month by taking the differences between the mean positions of the magnet at each of the 24 hours, in the month, and the mean position in the month itself (viz. the mean of all the days and all the hours)—no omission whatsoever being made of disturbed observations.

The "Solar-diurnal Variation" is obtained by a similar process, after the separation and omission of all the observations which differed by a certain small and constant value from the normals of the same month and hour. By this process the effects of the "Casual and Transitory changes" are in a very great degree eliminated, and a very close approximation is obtained to the systematic diurnal action of the sun upon the direction of the horizontal magnet, apart from the effects of disturbances. The solar-diurnal variation thus obtained at Kew is compared with results similarly obtained at six other stations, viz. three stations in the interior of the two great northern continents, one equatorial station, and two stations in the middle latitudes of the southern hemisphere—thus generalizing upon a very extensive scale the action of the sun in producing the phenomena under notice.

The fifth section is occupied by a similar generalization of the facts which have placed in evidence the existence of a semiannual inequality

in the solar-diurnal variation, having its epochs coincident, or very nearly so, with the sun's passage of the equator, and dependent consequently on the earth's position in its orbit. The sun's action in producing this semiannual inequality is shown to be characteristically different from that which is manifested in the solar-diurnal variation itself, pointing apparently to a difference in the mode of the sun's action in the two cases.

The sixth section contains a tabular view of the "Lunar-diurnal Variation" at Kew, in each of the five years during which the photographic record has been maintained there; this is followed by a comparison with similar results at other stations on the globe, and a statement of the principal points of agreement or of difference which are shown thereby.

III. "Results of the Magnetic Observations at the Kew Observatory, from 1858 to 1862 inclusive."—No. II. By Major-General Edward Sabine, P.R.S. Received June 18, 1863.

(Abstract.)

This paper is a continuation of the preceding one. It consists of two sections, the seventh and eighth. In the seventh section the author discusses the secular change and annual variation of the declination; and in the eighth section, the annual variation or semiannual inequality of the inclination and of the horizontal and total magnetic force.

Seventh Section.—The positions of the horizontal magnet at 24 equidistant epochs in the day, tabulated from the photograms of the Kew declinometer, with the omission of the disturbed observations, as described in the former paper, are grouped in weekly means, forming 52 mean values, corresponding to the number of weeks in the year. A Table is given of these weekly values, comprehending, in five columns, the five years from January 1858 to December 1862 inclusive, and from these a sixth column is formed, representing the mean declination in each of the 52 weeks of a mean or typical year, corresponding in this instance to the year 1860. The mean declination obtained from all the weekly results in the five years, and corresponding to its middle epoch July 1, 1860, is $21° 39' 18''\cdot1$; and from a comparison of the mean declinations corresponding to July 1 in the

columns which severally present the weekly values in the years from 1858 to 6182 inclusive, the mean value of the secular change corresponding to the period comprised in the Table is deduced. A proportional part of the secular change is then applied with its appropriate sign to each of the weekly values in the mean or typical year. These should all correspond with the mean declination of the whole Table (viz. 21° 39′ 18″ ·1), or should exhibit only such small and unsystematic differences as might reasonably be ascribed to casual errors. The final column of the Table contains these differences, in which it is at once seen that they divide themselves into two distinct categories, distinguished by the — sign in the semiannual period from March 21 to Sept. 21, and by the + sign from Sept. 21 to March 21. Hence the author infers the existence of a variation in the declination at Kew having an annual period, and consisting of a semiannual inequality with epochs coincident, or nearly so, with the sun's passage of the equator—the magnet being deflected towards the east when the sun is north, and towards the west when he is south of the equator. The amount of the semiannual inequality, as shown by the Table, averages — 28″·95 in the weeks from March 21 to Sept. 21, and + 29″·9 in those from September to March. The whole amount of the annual variation at Kew is therefore 58″·85.

The result thus obtained from the observations at Kew is compared with the result of an investigation of the corresponding phenomena at Hobarton in the southern hemisphere, obtained from hourly observatious of the declination during five years, commencing in October 1843, and terminating in September 1848. The observations themselves are published in the 2nd and 3rd volumes of the Hobarton Observations, and are treated, for the purposes of this paper, precisely in the same way as those of the Kew Observatory, forming a table strictly analogous to the one, previously described, at Kew. The final column of the Hobarton Table exhibits the differences, in each of the 52 weeks of the typical year, from the mean declination derived from the whole of the observations in the five years. The + and — signs in this column attest in as striking a manner as do those at Kew, the existence at Hobarton of a semiannual inequality of which the epochs coincide, or very nearly so, with the sun's passage of the equator: the direction of the deflection is the same as at Kew, viz., of the north end of the magnet

towards the east when the sun is north of the equator, and to the west when he is south of the equator. The amount of the deflection in the first-named semiannual period is, on the average, $19''\cdot1$ in each week; and in the opposite semiannual period $19''$; making together an annual variation of $38''\cdot1$.

The author then refers to the result of a similar investigation of the phenomena at St. Helena in the equatorial zone, the particulars of which have been already published in the 2nd volume of the St. Helena Observations. The result, derived from eight years of observation, of which five years were hourly, evidences at that station also the existence of a semiannual inequality with epochs coinciding, or nearly so, with the equinoxes—the deflections being also in the same directions as those at Kew and Hobarton, viz., to the east when the sun is north, and to the west when he is south, of the equator. The amount of the annual variation thus produced is less at St. Helena than at either Kew or Hobarton—the semiannual difference being about $7''$, and the annual variation $14''$.

The author remarks that the difference in the amount of deflection at the three stations may, in part at least, be occasioned by the difference in amount of the antagonistic force of the earth's magnetism, tending to retain the magnet in its mean position in opposition to all disturbing causes. The antagonistic force, viz. the horizontal component of the earth's magnetic force, is approximately 5·6 (in British units) at St. Helena, 4·5 at Hobarton, and 3·8 at Kew.

In a note appended subsequently to the delivery of this paper, viz. on June 19, 1863, the author refers to a similar investigation of the phenomena at the Cape of Good Hope, published in 1851, in the 1st volume of the magnetical observations at that station. The volume contains the fortnightly means of the declination from July 1842 to July 1846, corrected for secular change, and collected in Table III., page v, of that volume. The differences of the declination in each fortnight, so corrected, from the mean declination of the whole period, are shown in its final column. The mean of the thirteen fortnights (in the four years) between March 26 and Sept. 23, is $0'\cdot4$ more *easterly*, and of the thirteen fortnights between September 24 and March 25, $0'\cdot4$ more *westerly* than the mean value—showing an annual variation of $0'\cdot8$ (or $48''$), or a semiannual inequality averaging $24''$ to the east in the thirteen fortnights from March 26

to Sept. 23, and 24″ to the west in the thirteen fortnights from Sept. 24 to March 25. This is in accordance with the conclusions at all the other stations at which the phenomena have been subjected to a suitable investigation. The antagonistic horizontal component of the earth's magnetism is approximately 4·5.

Eighth Section.—In the eighth section the author examines the evidence which the monthly determinations of the dip and of the horizontal component of the magnetic force at Kew afford of the existence of a semiannual inequality in the absolute values of the dip and of the total magnetic force. The results of the monthly determinations from April 1857 to March 1863 are exhibited in two Tables, one appropriated to the dip, and the other to the horizontal force. The whole series of the determinations of the horizontal force were made with the same unifilar magnetometer and the same collimator magnet throughout, and also by the same observer, Mr. Chambers, one of the assistants at the Kew Observatory. In the monthly determinations of the dip from April 1857 to September 1860, twelve different circles, and their twenty-four needles were occasionally employed, the mean of all the observations in each month being taken as the mean result in that month. There were also several observers in this part of the series, chiefly four. In the months from October 1860 to March 1863, one circle with its two needles were the sole instruments, and Mr. Chambers the sole observer. The probable error of a single monthly determination of the dip in the first part of the series, when several instruments and several observers were employed, is stated to be $\pm\ 0'{\cdot}69$; and in the second part of the series, obtained by a single circle and the same observer throughout, the probable error is $\pm 0'{\cdot}75$; whence it is inferred that the greater number of partial results which contributed to produce the monthly mean in the earlier period more than counterbalanced the diversities which might have been occasioned by the peculiarities of the different observers and of the different instruments. The probable error of a single monthly determination of the dip, after the application of the corrections for secular change and annual variation, is stated to be $\pm\ 0'{\cdot}71$, and of a single monthly determination of the horizontal force derived from the 72 monthly determinations $\pm\ {\cdot}0024$.

The results of the monthly determinations at Kew, as bearing

upon the question of annual variation, may be briefly stated as follows:—1st. The dip is subject to an annual variation, which, on the average of the six years, amounts to 1'·35; consisting of a semiannual inequality with epochs coinciding, or very nearly so, with the equinoxes; the mean dip being on the average 0'·65 lower than its annual mean value in the six months from April to September, and 0'·7 higher than its annual mean value in the six months from October to March. 2nd. That the horizontal force is subject to a semiannual inequality having the same epochs—being on the average ·0013 higher than its annual mean in each of the six months from April to September, and ·0013 lower than its annual mean in each of the months from October to March. 3rd. That, combining the results of the dip and horizontal force, the total terrestrial magnetic force is expressed in British units by 10·3002 as its mean value in the months from April to September, and by 10·30347 in the months from October to March,—there being thus a difference of ·00327, by which the intensity of the magnetic force of the earth is greater in the months when the sun is south of the equator than in the months when he is north of the equator.

This conclusion is compared with the results obtained in a corresponding manner from the published observations of the Hobarton Observatory, viz., with the monthly determinations of the horizontal force in the five years from January 1846 to December 1850 inclusive, and with those of the dip in the ten years from January 1841 to December 1850 inclusive. From these data the conclusions are drawn, 1st, that at Hobarton the dip is subject to an annual variation amounting to 1'·18, consisting of a semiannual inequality with epochs coinciding or nearly so with the equinoxes—the (south) dip being on the average 0'·59 less in the months from April to September, and 0'·59 greater in the months from October to March than the mean annual value; and 2nd, that the horizontal force is subject to a similar semiannual inequality, being ·0007 less than its mean value in the months from April to September, and ·0005 greater in the months from October to March; and combining these two results, that the total force at Hobarton is expressed in British units by 13·56882 in the months from October to March, and by 13·55195 in the months from April to September: the difference, ·01687, expresses the measure of the greater intensity of the earth's

magnetic force when the sun is south than when he is north of the equator.

The author concludes this section of his investigations by drawing the attention of the Royal Society to this concurrent evidence, from the observations of three observatories situated in parts of the globe so distant from each other, of a semiannual inequality having such strong features of resemblance in both hemispheres, and remarks that it seems difficult to assign such effects to any other than to a cosmical cause. The "inequalities" may in themselves seem to be small; but judged of *scientifically, i. e.* in the proportions they bear to their respective probable errors, they are large.

IV. "Experiments, made at Watford, on the Vibrations occasioned by Railway Trains passing through a Tunnel." By Sir JAMES SOUTH, LL.D., F.R.S., Member of the Board of Visitors of the Royal Observatory, Greenwich. Received June 17, 1863.

(Abstract.)

These experiments were made in consequence of an attempt in 1846 to run a line of railway through Greenwich Park, in what seemed to several competent judges a dangerous proximity to the Royal Observatory.

It was abandoned, but (as Sir James South was informed) only for a time; and he thought it right to make some examination of the probable effects of such a vicinity, especially as to the power of a tunnel in deadening the vibrations.

The Watford tunnel was chosen as the observing station, being, on the high authority of the late Mr. Warburton, in ground very analogous to that on which the Royal Observatory stands; and every facility for making observations was afforded by the late Earl of Essex, through whose park and preserves this tunnel passes.

As the chief inconvenience to be feared from the proposed railway was the disturbance of the observations by reflexion in mercury, it seemed best to take a series of these under circumstances as nearly as possible resembling those which might be expected at Greenwich. An Observatory was therefore erected, in which a large and powerful

transit-instrument was mounted, with all the attention to stability that could be given in a first-class Observatory; and it had sufficient azimuthal motion to enable the observer to follow the Pole-star in its whole course; so that night or day (if clear), he could have the reflected image of the star in the mercurial vessel, ready to testify against the tremors caused by any train.

The distance of the vessel from the nearest part of the tunnel was 302 yards, that proposed for Greenwich belng 286 yards. The length of the tunnel is 1812 yards; its southern or London end is 643 yards from where the mercury was placed, its northern or Tring end 1281 yards; and about 64 feet of chalk and gravel lie above the brickwork of its crown. The author's preparations were not complete till December 1846, and then a continuance of cloudy weather interfered with observation till January the 11th, 1847, when and on the following nights he obtained results so decisive that he felt it his duty to communicate them at once to the *then* First Lord of the Admiralty, the late Lord Auckland, who was so satisfied with them, that in a letter to Sir James, dated "Admiralty, Jan. 26, 1847," he recorded the impression they had made on his mind in the following words:—"*They would be quite conclusive if the question of carrying a tunnel through Greenwich Park were again to be agitated.*" Sir James, however, continued the work to the end of March.

With the ordinary disturbance to which an Observatory is liable (as wind, carriages, or persons moving near it), the reflected image of a star breaks up into a line of stars, perpendicular to the longest side of the mercury-vessel. With increased agitation, another line of stars perpendicular to the first appears, making a cross. With still more the cross becomes a series of parallel lines of stars; still more makes the images oscillate; and at last all becomes a confused mass of nebulous light. The first of these (the line) is not injurious to onec lass of observations; but the others are, and therefore the second (the cross) was taken as a measure of the beginning and end of injurious disturbance. Signal shots were fired when a train passed the southern entrance of the tunnel, and a shaft 1162 yards from it. Hence the train's velocity was obtained, and thence its position at any given time.

Upwards of 230 observations are given in detail, and their most important results are shown in a Table, which contains the date, the

distances at which the cross of stars begins and ceases to be visible, those at which the series of parallel lines is seen, the velocity in miles per hour, the weight of each engine, and also the length and weight of each train (when it could be identified).

This Table proves that *in all cases* but one (which in fact is scarcely an exception) there is sufficient vibration to excite the cross at 670 yards, and that in 24 per cent. of the number it is seen beyond 1000, its maximum being 1176. At the southern end such distances reach far beyond the tunnel, while at the north they fall within it. From comparing them in the two cases, the author infers that the train's agitation extends laterally as far when it is in the tunnel as when in the open cutting. The amount of disturbance does not depend solely on the velocity and weight of the train, but also on other circumstances, of which prolonged action and length of train are the chief. In one instance, with only a velocity of 11·4 miles, the cross was seen at 1110 yards—a proof that no regulation of the speed in passing an Observatory at a distance of 300 or 400 yards would be of any avail.

The system of parallel lines is only seen between lines making angles of 45° with the perpendicular to the rails, that is, at distances under 427 yards; it scarcely ever is produced unless the cross be visible beyond 1000 yards.

These forms are also produced by the reports of cannon of twelve ounces calibre, at distances from 300 to 3000 yards; in the last case there is but a faint trace of the cross. In all, the appearance is momentary, not lasting in any case more than a second and a half. They are not produced by the roar of a two-pound rocket fired 82 feet from the mercury, though very loud. When the cannon were fired *in the tunnel*, where the perpendicular meets it, *two* sets of tremors were seen—one, he believes, propagated through the ground, the other through the air about a second later, the sound escaping probably through the shafts. Attempts were made to substantiate or refute this hypothesis; but the difficulties of *rapidly* shifting and unshifting the coverings prepared for the purpose were such as to compel him to relinquish them.

These observations were reduced in 1847; but conceiving all danger to the Royal Observatory was past, the author did not think it necessary then to proceed with them. As, however, no Observatory

can now be considered secure from railway injury, he wishes to make them public, in hopes that they may be useful, not only to practical astronomy, but to some other departments of science.

V. "Preliminary Notice of an Examination of *Rubia munjista*, the East-Indian Madder, or Munjeet of Commerce." By JOHN STENHOUSE, LL.D., F.R.S. Received June 18, 1863.

It is rather remarkable that while few vegetable substances have been so frequently and carefully examined by some of the most eminent chemists than the root of the *Rubia tinctorum*, or ordinary madder, the *Rubia munjista*, or munjeet, which is so extensively cultivated in India and employed as a dye-stuff, has been, comparatively speaking, very much overlooked, never having been subjected, apparently, to anything but a very cursory examination. Professor Runge, at the close of his very elaborate memoir upon madder, published in 1835, details a few experiments which he made upon the tinctorial power of munjeet, the constituents of which he regarded as very similar to those of ordinary madder. Professor Runge stated that munjeet contains twice as much available colouring matter as the best Avignon madder. This result was so unexpected that the Prussian Society for the Encouragement of Manufactures, to whom Professor Runge's memoir was originally addressed, referred the matter to three eminent German dyers, Messrs. Dannenberger, Böhm, and Nobiling. These gentlemen reported, as the result of numerous carefully conducted experiments, that, so far from munjeet being richer in colouring matter than ordinary madder, it contained only half the quantity. This conclusion has been abundantly confirmed by the experience of my friend Mr. John Thom, of Birkacre, near Chorley, one of the most skilful of the Lancashire printers. From some incidental notices of munjeet in Persoz and similar writers, and a few experiments which I made some years ago, I was led to suspect that the colouring matters in munjeet, though similar, are by no means identical with those of ordinary madder, and that probably the alizarine or purpurine of madder would be found to be replaced by some corresponding colouring principle. This hypothesis I have found to be essentially correct; for the colouring matter of munjeet, instead of consisting of a mixture of alizarine and purpurine, contains

no alizarine at all, but purpurine and a beautiful orange colouring matter crystallizing in golden scales, to which I purpose giving the name of "munjistine." Munjistine exists in munjeet in considerable quantity, and can therefore be easily obtained.

The colouring matter of munjeet may be extracted in various ways; that which I have found most suitable is as follows:—each pound of munjeet in fine powder is boiled for four or five hours with two pounds of sulphate of alumina and about sixteen of water. The whole of the colouring matter is not extracted by a single treatment with sulphate of alumina; the operation must be repeated therefore two or three times. The red liquid thus obtained is strained through cloth filters while still very hot, and the clear liquor acidulated with hydrochloric acid. It soon begins to deposit a bright red precipitate, the quantity of which increases on standing, which it should be allowed to do for about twelve hours. This precipitate is collected on cloth filters and washed with cold water till the greater portion of the acid is removed. It is then dried, reduced to fine powder, and digested in a suitable extracting apparatus with boiling bisulphide of carbon, which dissolves out the crystallizable colouring principles of the munjeet, and leaves a considerable quantity of dark-coloured resinous matter. The excess of the bisulphide of carbon having been removed by distillation, the bright red extract, consisting chiefly of a mixture of munjistine and purpurine, is treated repeatedly with moderate quantities of boiling water and filtered. The munjistine dissolves, forming a clear yellow liquid, while almost the whole of the purpurine remains on the filter. When this solution is acidulated with hydrochloric or sulphuric acid, the munjistine precipitates in large yellow flocks. These are collected on a filter and washed slightly with cold water. The precipitate is then dried by pressure, and dissolved in boiling spirit of wine slightly acidulated with hydrochloric acid to remove any adhering alumina. As the munjistine does not subside from cold alcoholic solutions, even when they are largely diluted with water, about three-fourths of the spirit are drawn off by distillation, when the munjistine is deposited in large yellow scales. By two or three crystallizations out of spirit in the way just described the munjistine is rendered perfectly pure.

I have likewise succeeded in extracting munjistine directly from munjeet by boiling it with water, filtering the solution, which has a dark brownish-red colour, and then acidulating with hydrochloric

acid. The precipitate which falls is collected on a filter, washed, dried, and treated with boiling spirit of wine, which leaves a large quantity of pectine undissolved. The munjistine which dissolves in the alcohol is obtained in a pure state by repeated crystallizations in the way already indicated. The first process which I have described is, however, by far the best. The colouring matter of munjeet can likewise be extracted with boiling solutions of alum; but I find sulphate of alumina greatly preferable, as the alum, by its tendency to crystallize, very much impedes the filtration of the liquids. I likewise attempted to employ Professor E. Kopp's process with sulphurous acid, which gives such excellent results with ordinary madder, but I found it wholly inapplicable to munjeet.

Munjistine, prepared by the processes described, when crystallized out of alcohol, forms golden-yellow plates of great brilliancy. It is but moderately soluble in cold, but dissolves pretty readily in boiling water, forming a bright yellow solution, from which it is deposited in flocks when the liquid cools. Saturated solutions almost gelatinize. It dissolves to some extent in cold, but more readily in boiling spirit of wine, and is not precipitated by the addition of water. It dissolves in carbonate of soda with a bright red colour. In ammonia it forms a red solution with a slight tinge of brown: caustic soda produces with it a rich crimson colour. Both its aqueous and alcoholic solutions, when boiled with alumina, form beautiful flakes of a bright orange colour, almost the whole of the munjistine being withdrawn from solution. These flakes are soluble in a large excess of caustic soda, with a fine crimson colour. Munjistine dyes cloth mordanted with alumina a bright orange. With iron mordant it yields a brownish-purple colour, and with Turkey-red mordant a pleasing deep orange. These colours are moderately permanent, and bear the application of bran and soap tolerably well. The munjistine sensibly modifies the colours produced by munjeet, giving the reds a shade of scarlet, as has been long observed.

Commercial nitric acid dissolves munjistine with a yellow colour, but does not appear to decompose it even on boiling. Fuming nitric acid (1·5) dissolves munjistine in the cold, and on application of heat decomposes it, no oxalic acid being produced. It readily dissolves in cold sulphuric acid with a bright orange colour; and the solution may be heated nearly to boiling without blackening or

giving off sulphurous acid; it is reprecipitated by water in yellow flocks apparently unaltered. When bromine water is added to a strong aqueous solution of munjistine, a pale-coloured flocculent precipitate is immediately produced; this, when collected on a filter, washed and dissolved in hot spirit, furnishes minute tufts of crystals, evidently a substitution product. I may remark, in passing, that when alizarine is treated with bromine water in a similar way, it also forms a substitution product crystallizing in needles. I am at present engaged in the examination of both these compounds.

When munjistine is strongly heated on platinum foil, it readily inflames and leaves no residue; when it is carefully heated in a tube, it fuses, and crystallizes again on cooling. It sublimes more readily than either purpurine or alizarine, forming golden scales which consist apparently of unaltered munjistine, as they give the characteristic rich crimson coloration with caustic alkalies. Baryta water produces a yellow precipitate with munjistine. Acetate of lead throws down a bright crimson precipitate, both in its aqueous and alcoholic solutions. I expect, from this and the bromine substitution compound, very shortly to ascertain the atomic weight of this body; in the mean time I submit the results of its ultimate analysis.

I. ·314 grm. of munjistine yielded ·732 grm. of carbonic acid and ·106 grm. of water.

II. ·228 grm. munjistine yielded ·535 grm. carbonic acid and ·0765 grm. water.

	I.	II.
C per cent.	63·6	64·0
H „	3·77	3·73
O „	32·63	32·27
	100·00	100·00

The munjistine operated upon in each case was prepared at different times; moreover No. 1 was burnt with oxide of copper, No. 2 with chromate of lead.

Munjistine in some of its properties bears considerable resemblance to Runge's madder-orange, the "rubiacine" of Dr. Schunck: it is, however, essentially different from rubiacine, both in several of its properties, such as its solubility in water and alcohol, &c., and in the amount of its carbon—rubiacine, according to Dr. Schunck's analysis,

containing 67·01 per cent. of that element, while munjistine contains only 64. The spectra afforded by solutions of the two substances, as may be seen from the following extract from a letter received from Professor Stokes, are decidedly different.

"The two substances are perfectly distinguished by the very different colour of their solution in carbonate of soda, when a small quantity only of substance is used. The solution of munjistine is red inclining to pinkish orange, that of rubiacine a claret-red. The tints are totally different, and indicate a different mode of absorption. Both present a single minimum in the spectrum; but while that of rubiacine extends from about D to F, that of munjistine extends from a good way beyond D to some way beyond F. The beginning and end of the band in each case is not very definite, and varies of course with the strength of the solution; but by comparing the substances with different strengths of solution, there can be no doubt of the radical difference in the position of the band of absorption. In this way it is easy to convince oneself that the difference of colour is not to be explained by the possible admixture of some small impurity present in one or other specimen. With caustic potash munjistine gives as nearly as possible the same colour as rubiacine, agreeing with the colour of rubiacine in carbonate of soda. There appears to be a slight difference in the spectrum of the munjistine and rubiacine solutions, but not enough to rely on; so that the substances are not to be distinguished by their solutions in *caustic* alkalies.

"A second perfectly valid distinction is, however, afforded by the different colour of the fluorescent light of the ethereal solutions. The solid substances themselves and their ethereal solutions are fluorescent to a considerable degree; but the tint of the fluorescent light of the ethereal solution of rubiacine is orange-yellow, while that of the ethereal solution of munjistine is yellow inclining to green. The examination in a pure spectrum shows that the difference is not due to the admixture of a small impurity, itself yielding a fluorescent solution; but the tints may be readily contrasted by daylight, almost without apparatus, by the method I have described in a paper 'On the existence of a second crystallizable fluorescent substance in the bark of the horse-chestnut' (Quart. Journal Chem. Soc. vol. ii. p. 20). I consider either of the two points of difference I

have mentioned sufficient by itself to establish the non-identity of munjistine and rubiacine" *.

The purpurine which I succeeded in extracting from munjeet and in purifying from munjistine in the way already described, formed beautiful dark crimson needles, having all the usual properties of that substance. When examined by Professor Stokes, they gave the very characteristic spectra of purpurine.

·3285 grm. of purpurine gave ·8005 grm. carbonic acid and ·1050 grm. water.

	Analysis.		
	Theory.	Found.	Debus (mean).
C........	66·67	66·46	66·40
H........	3·70	3·55	3·86
O........	29·63	29·99	29·74
	100·00	100·00	100·00

From the results above detailed there can therefore be no doubt that the colouring matter of munjeet, as already stated, consists of purpurine and munjistine.

I cannot conclude this preliminary notice without acknowledging the essential services I have received from Professor Stokes, who kindly submitted the different products obtained by me to optical examination. Though it is plain that a substance optically pure, that is, containing no impurities affecting the spectrum, may still be far from being chemically so, yet the spectroscope is extremely useful in indicating admixtures of kindred substances of very similar properties, having a great affinity for each other, and therefore not readily separable. I feel certain therefore that if Professor Stokes would draw up a short treatise embodying his extensive and accurate observations on the spectra of the colouring matters and similar substances, he would confer a great boon on the cultivators of organic chemistry.

Postscript.—Received July 18, 1863.

Since the preceding paper was communicated to the Royal Society I have been enabled to examine the action of nitric acid on munjis-

* I may mention that the rubiacine which Professor Stokes examined was prepared by Dr. Schunck himself.

tine much more fully. When munjistine is digested with moderately strong nitric acid, as already stated, copious fumes are given off, the munjistine gradually dissolving and forming a colourless solution. When this is evaporated to dryness on the water-bath, a white crystalline mass is obtained, consisting almost entirely of phthalic acid contaminated with a small quantity only of oxalic acid. The oxalic acid may be easily removed by washing the mass with a little cold water and then pressing between folds of bibulous paper, or by neutralizing the mixture of the two acids with lime and then treating with boiling water, which dissolves the phthalate of lime. The acid freed from oxalic acid by either of these methods presents all the usual reactions of phthalic acid. One of the most convenient ways of purifying it consists in subliming it repeatedly in a Mohr's apparatus, when the anhydrous acid is obtained in beautifully white iridescent four-sided prisms, frequently several inches in length. ·3745 grm. of the crystals of the anhydride, burnt with chromate of lead, gave ·891 grm. carbonic acid and ·095 grm. of water.

	Theory.	Expt.	Marignac.	Laurent.
C_{16}....96	64·86	64·89	64·88	64·70
H_4.... 4	2·70	2·81	2·71	2·38
O_6....48	32·44	32·30	32·41	32·92

From this result it is evident that the acid chiefly produced by the action of nitric acid upon munjistine is phthalic acid, which, as is well known, may also be procured from alizarine and purpurine. This reaction, therefore, indicates a very close relationship between these three substances, the only true colouring principles of madder with which we are at present acquainted.

VI. "Notes of Researches on the Poly-Ammonias.—No. XXIV. On Isomeric Diamines." By A. W. Hofmann, LL.D., F.R.S. Received May 26, 1863.

In a former paper* I have described phenylene-diamine, an aromatic diamine which is formed by the action of powerful reducing

* Proc. Roy. Soc. vol. xi. p. 518.

agents upon dinitrobenzol. Phenylene-diamine, the last product of this reaction, is preceded by the formation of an intermediate compound, *nitraniline*, a substance discovered many years ago by Dr. Muspratt and myself *,

$$\begin{array}{cccc} C_6H_6 & C_6H_4\begin{matrix}NO_2\\NO_2\end{matrix} & C_6H_4\begin{matrix}NO_2\\NH_2\end{matrix} & C_6H_4\begin{matrix}NH_2\\NH_2.\end{matrix} \\ \text{Benzol.} & \text{Dinitrobenzol.} & \text{Nitraniline.} & \text{Phenylene-diamine.} \end{array}$$

Nitraniline, as might have been expected, was found to be readily convertible into phenylene-diamine.

By the researches of M. Arppe †, chemists have become acquainted with a *second nitraniline*, which is obtained by the action of fuming nitric acid upon phenyl-pyrotartramide and subsequent treatment of the nitro-compound with potassa. This substance, which, as I afterwards found, may be more readily prepared by a similar treatment of other less difficultly obtainable phenylamides, such as phenyl-acetamide or phenyl-succinamide, is isomeric with ordinary nitraniline, but differs from the latter compound both in its physical and chemical properties, so as to leave no doubt regarding the individuality of the two compounds, which have accordingly been distinguished as *alpha-nitraniline* and *beta-nitraniline*. This singular isomerism, which has been traced also in other phenyl-derivatives, remains unexplained ‡. Whilst engaged with the examination of phenylene-diamine, the idea naturally suggested itself, to ascertain whether beta-nitraniline, when submitted to reducing agents, would yield a body isomeric but differing from the phenylene-diamine obtained from dinitrobenzol and alpha-nitraniline.

Beta-nitraniline is readily reduced by a mixture of iron and acetic

* Chem. Soc. Mem. vol. iii. p. 112.

† Chem. Soc. Journ. vol. viii. p. 175.

‡ Among the various attempts I have made to decipher this isomerism, I may mention the treatment of the two nitranilines with the iodides of methyl and ethyl. But these substances are not acted upon by the reagents in question, and I take this opportunity of correcting an error which has crept into my paper on the molecular constitution of the volatile organic bases (Phil. Trans. 1850, vol. i. p. 93). In this paper I state that the action of iodide of ethyl on nitraniline gives rise to the formation of hydriodate of ethyl-nitraniline. This statement is based upon a single platinum determination. The platinum-salt of nitraniline contains 28·66 per cent. of platinum, that of ethyl-nitraniline 26·53 per cent. Analysis had furnished me 26·23 per cent. I have since satisfied myself that the salt was the imperfectly purified platinum-salt of nitraniline.

acid. The basic compound which distils over has the same composition as phenylene-diamine, viz. $C_6 H_8 N_2$, and presents in its properties many analogies with this substance, but it is far from being identical with it. The two diatomic bases obviously are related to each other in the same manner as the two nitranilines from which they are derived, and I propose therefore to distinguish them as *alpha-phenylene-diamine* and *beta-phenylene-diamine*. Beta-phenylene-diamine differs from alpha-phenylene-diamine by its superior crystallizing power: whilst the latter for days and often for weeks remains liquid, the former immediately, when separated from one of its salts by an alkali, solidifies into a beautifully crystalline mass. The fusing-point of alpha-phenylene-diamine is 63° (corr.), that of beta-phenylene-diamine is 140° (corr.); the former boils at 287° (corr.), the latter at 267° (corr.). Beta-phenylene-diamine is remarkable for the facility with which it sublimes even at temperatures below its boiling-point. The experiment succeeds particularly well in a current of hydrogen gas, when the base is obtained in splendid crystalline plates resembling pyrogallic acid.

The salts of beta-phenylene-diamine, although they are more soluble than the corresponding alpha-phenylene-diamine salts, are distinguished by the same superior crystallizing power. They are all remarkable for the facility with which they yield beautiful and mostly well-formed crystals.

I have examined only two of these salts somewhat more minutely.

Hydrochlorate of Beta-phenylene-diamine.—This salt crystallizes in large prisms, which are at present in the hands of M. Quintino Sella. Extremely soluble in water, difficultly soluble in hydrochloric acid, it contains

$$C_6 H_8 N_2, 2HCl.$$

Hydrobromate of beta-phenylene-diamine resembles in every respect the hydrochlorate. The crystals, which were found to have the formula

$$C_6 H_8 N_2, 2HBr,$$

are apt to assume a reddish tint when left in contact with the air.

Platinum-salt.—Light-yellow plates extremely soluble in water and readily decomposed by heat. Composition:

$$C_6 H_8 N_2, 2HCl, 2PtCl.$$

The sulphate and nitrate are easily crystallizable salts, somewhat less soluble in water than the hydrochlorate.

Beta-phenylene-diamine and its salts are remarkable for the facility with which they are converted into violet- and blue-coloured compounds under the influence of oxidizing agents such as chlorine, bromine, chromic acid, ferric and platinic chloride, &c.

Both alpha- and beta-phenylene-diamine are readily attacked by the iodides of the alcohol radicals; and a means was thus afforded of ascertaining whether both substances exhibit the same degree of substitution. Since only the last products of substitution presented any interest, I have submitted the two bases to methylation. This experiment showed that both alpha- and beta-phenylene-diamine are capable of absorbing six equivalents of methyl to produce ammonium compounds of perfect substitution, and that both bases must therefore be represented by the formula

$$\left.\begin{matrix} C_6\,H_4 \\ H_2 \\ H_2 \end{matrix}\right\} N_2.$$

In the case of both bases, alternate treatment with iodide of methyl and oxide of silver or distillation with soda, thrice repeated, leads to the formation of a well-crystallized iodide of the formula

$$C_{12}\,H_{22}\,N_2\,I_2 = \left[\left.\begin{matrix} (C_6\,H_4)'' \\ (C\,\,H_3)_6 \end{matrix}\right\} N_2\right]'' I_2.$$

Whether prepared from alpha- or from beta-phenylene-diamine, this salt crystallizes in plates extremely soluble in water, less so in alcohol. I have found no other difference except that the beta-phenylene-compound is more soluble than the derivative of the alpha-base.

Whilst studying the methylated derivatives of the two bases, I have, of course, repeatedly obtained the lower, still volatile bases, which on this occasion were submitted to a few experiments.

The compound

$$C_{10}\,H_{16}\,N_2 = \left.\begin{matrix} (C_6\,H_4)'' \\ (C\,\,H_3)_4 \end{matrix}\right\} N_2,$$

procured from beta-phenylene-diamine, when submitted to the action of iodide of methyl, was found to produce, in the first place, a rather difficultly soluble iodide,

$$C_{11}\,H_{19}\,N_2\,I = \left.\begin{matrix} (C_6\,H_4)'' \\ (C\,\,H_3)_4 \end{matrix}\right\} N_2,\,CH_3\,I,$$

before it was converted into the final product

$$C_{12}H_{22}N_2I_2=\left.\begin{matrix}(C_6H_4)''\\(C\ H_3)_4\end{matrix}\right\}N_2,\ 2CH_3I=\left[\left.\begin{matrix}(C_6H_4)''\\(C\ H_3)_6\end{matrix}\right\}N_2\right]''I_2.$$

The pentamethylated iodide, when treated with hydriodic acid instead of iodide of methyl, furnished the di-iodide of pentamethyl-phenylene-diammonium,

$$C_{11}H_{20}N_2I_2=\left[\left.\begin{matrix}(C_6H_4)''\\(C\ H_3)_5H\end{matrix}\right\}N_2\right]''I_2.$$

The two phenylene-diamines are thus seen not only to be isomeric, but to have actually the same degree of substitution, as far as the latter may be rendered transparent by the action of iodide of methyl. Under these circumstances I was pleased to observe some additional phenomena which removed every doubt regarding their individuality.

On mixing a solution of beta-phenylene-diamine in sulphuric acid with peroxide of manganese, the odour of *kinone* becomes at once perceptible, and on heating the mixture, kinone distils over—the residuary liquid containing the sulphates of manganese and ammonium,

$$\underset{\text{Beta-phenylene-diamine.}}{C_6H_8N_2}+2H_2SO_4+Mn_2O_2=\underset{\text{Kinone.}}{C_6H_4O_2}+Mn_2SO_4+(H_4N)_2SO_4.$$

The reaction proceeds with such ease that a few milligrammes of the diamine, when submitted to this treatment in the test-tube, yield a distinct crystalline sublimate of kinone, readily recognizable by its many salient properties. Alpha-phenylene-diamine, when similarly treated, evolves a faint odour of kinone, but does not yield crystals of this substance.

The elegant and easy formation of kinone in this reaction presents some interest, inasmuch as the process appears to be of general application, and will probably lead to the preparation of the higher homologues of kinone.

I have observed the formation of beta-phenylene-diamine in two additional reactions, which, in conclusion, I beg leave to mention.

In the hopes of obtaining the triamine of the phenyl-series

$$C_6H_9N_3=\left.\begin{matrix}(C_6H_3)'''\\H_3\\H_3\end{matrix}\right\}N_3,$$

I submitted dinitrophenylamine,

$$C_6H_5N_3O_4 = \left.\begin{matrix} [C_6H_3(NO_2)_2] \\ H \\ H \end{matrix}\right\} N,$$

to distillation with iron and acetic acid, but, instead of the compound which I endeavoured to procure, I invariably obtained beta-phenylene-diamine and ammonia,

$$\underbrace{C_6H_9N_3}_{\text{Phenylic tria-mine.}} + H_2 = \underbrace{C_6H_8N_2}_{\text{Beta-phenylene-diamine.}} + H_3N.$$

Again, dinitrazobenzol,

$$C_{12}H_8N_4O_4 = C_{12}\begin{matrix} H_8 \\ (NO_2)_2 \end{matrix} N_2,$$

when submitted to the action of powerful reducing agents, yields likewise beta-phenylene-diamine

$$\underset{\text{Dinitrazobenzol.}}{C_{12}H_8N_4O_4} + 8H_2 = 4H_2O + \underset{\text{Beta-phenylene-diamine.}}{2C_6H_8N_2}.$$

Here also beta-phenylene-diamine is the final product of the reaction, the formation of which is preceded by that of another base, the *diphenine* of Messrs. Gerhardt and Laurent. Diphenine, according to these chemists, is

$$C_{12}H_{12}N_4,$$

a formula chiefly supported by the unequivocal presence of C_{12} in the molecule of azobenzol, $C_{12}H_{10}N_2$, whence it derives. The facility, however, with which diphenine under the influence of nascent hydrogen, by treatment with sulphuric acid and zinc, for instance, is converted into beta-phenylene-diamine renders it probable that the molecule of diphenine is

$$C_6H_6N_2,$$

when the two bases become related to each other as kinone and hydrokinone,

Kinone	$C_6H_4O_2$	Diphenine	$C_6H_6N_2$
Hydrokinone ..	$C_6H_6O_2$	Beta-phenylene-diamine	$C_6H_8N_2$.

I have hitherto vainly tried to transform kinone into diphenine or beta-phenylene-diamine; but it deserves to be noticed that M. Woskresensky *, by treating kinone with ammonia, has obtained a green

* Woskresensky, Journ. Pract. Chem. vol. xxxiv. p. 251.

beautifully crystalline mass, *kinonamide*, C_6H_5NO, which stands midway between kinone and diphenine,

$$\underset{\text{Kinone.}}{C_6H_4O_2}+H_3N=\underset{\text{Kinonamide.}}{C_6H_5NO}+H_2O.$$

Another similar step of transformation would lead to diphenine,

$$\underset{\text{Kinonamide.}}{C_6H_5NO}+H_3N=\underset{\text{Diphenine.}}{C_6H_6N_2}+H_2O.$$

The preparation of azobenzol in large quantity, its transformation into dinitrazobenzol, and, lastly, the conversion by means of sulphide of potassium of the nitro-compound into diphenine, present so little difficulty, that treatment of diphenine with nascent hydrogen affords the easiest and simplest means of procuring beta-phenylene-diamine in appreciable quantity.

VII. "Contributions towards the History of the Colouring Matters derived from Aniline." By A. W. Hofmann, LL.D., F.R.S. Received June 2, 1863.

In a short paper recently submitted to the Royal Society, I pointed out the existence of two aromatic diamines, both represented by the formula

$$C_6H_8N_2=\left.\begin{matrix}(C_6H_4)''\\H_2\\H_2\end{matrix}\right\}N_2,$$

and closely resembling each other, but differing in some of their fundamental characters to such an extent that I did not hesitate to assert their individuality, and to distinguish them as alpha-phenylene-diamine and beta-phenylene-diamine.

The existence of two closely allied bodies among the diatomic derivatives of the phenyl-series very naturally suggested the idea of searching for two similarly related monatomic bases of the same group, and accordingly I undertook during the last week a careful comparison of specimens of aniline prepared by different processes. This comparative study is still incomplete, but I beg leave to record even now an observation which appears to merit the attention of chemists.

I have, in the first place, examined aniline obtained by distillation of isatin (indigo) with hydrate of potassium.

The base prepared in this manner boils at 182°, and possesses the general characters attributed to aniline. But neither by treatment

with mercuric or stannic chloride nor with arsenic acid is this substance converted into aniline-red.

Aniline derived from benzol was next submitted to examination. The benzol employed for the preparation of the base was partly obtained by the distillation of benzoic acid with lime, partly by the fractional distillation of coal-tar naphtha, and the ultimate solidification of the product, boiling between 80° and 83°.

Both varieties of benzol were treated with fuming nitric acid, and the nitro-compound thus obtained converted into aniline by means of iron and acetic acid.

The base prepared from benzoic benzol boils at 182°. Like indigo-derived aniline, it refuses to yield the red colour by treatment with the agents previously mentioned.

Aniline obtained from coal-tar benzol, as might have been expected, likewise boils at 182°, and neither mercuric nor stannic chloride nor arsenic acid converts this substance into aniline-red.

When I communicated these observations to my friend Mr. E. C. Nicholson, I found that in this case, as in so many others, practice is far in advance of theory. The facts which I have mentioned had been long known to this distinguished manufacturer, who in reply to my note transmitted to me a gallon of absolutely pure aniline boiling at 182°, prepared from coal-tar benzol, and perfectly incapable of yielding aniline-red.

During the last few months I have had occasion to examine a great variety of commercial specimens of aniline, more especially samples which had been kindly supplied to me by Messrs. Simpson, Maule, and Nicholson, of London, and by Messrs. Renard Brothers and Franc, of Lyons. All these specimens furnished, by the ordinary processes, very notable quantities of aniline-red, but they also invariably boiled at a higher temperature, exhibiting in fact boiling-points varying between 182° and 220°.

It is thus obvious that commercial aniline contains a base different from normal aniline, the cooperation of which is indispensable for the production of aniline-red.

Is this base an isomeric variety of aniline, an aniline holding to the normal aniline a relation somewhat similar to that which obtains between alpha- and beta-phenylene-diamine? * It is well known

* Phil. Mag. S. 4. vol. xiii. p. 415 (June 1857).

that Mr. Church has separated from coal-tar naphtha a hydrocarbon isomeric with benzol, *parabenzol*, which boils at 97°·5. This substance is readily converted into a nitro-compound, and ultimately into the corresponding base. Is it the base thus formed which gives rise to the formation of aniline-red?

Or is it not more probable that commercial aniline contains another base analogous or homologous with aniline which is involved in the generation of the red?

These are questions equally interesting for theory and practice, and the solution of which will probably throw considerable light upon the still enigmatical genesis of rosaniline.

VIII. "Contributions towards the History of the Colouring Matters derived from Coal-tar." By A. W. Hofmann, LL.D., F.R.S. Received June 9, 1863.

In a previous Note I have shown that the red colouring matter cannot be obtained from *normal* aniline by the action of the agents usually employed for the preparation of this colour on a large scale. This observation naturally induced me to seek for the constituent in the commercial aniline which gives rise to the formation of aniline-red.

I have already remarked that the commercial product which is best suited for the manufacture of the red colour, boils at a temperature appreciably higher than the boiling-point of normal aniline. The idea presented itself of submitting this substance to a fractional distillation, or else of effecting a methodical separation of the hydrocarbons which constitute the starting-point for the manufacture of the bases; but, as is well known, these processes are difficult and tedious, and there is little chance of success unless the operation be performed on a very large scale.

In the hope of accelerating the inquiry, I examined the action of mercuric and stannic chlorides upon the homologues of aniline, of which I fortunately possessed some pure specimens. The contiguous term *toluidine* was the first to fix my attention. The presence of this base in commercial aniline could not be doubted, since the benzol employed in the manufacture of this substance almost invariably boils at temperatures between 80° and 100°, or even higher. Indeed Mr. Nicholson having convinced himself that pure aniline is not

available for the preparation of rosaniline, was at one time disposed to consider toluidine the true source of the so-called aniline-red.

But toluidine the purity of which was established by combustion when submitted under the most varied circumstances to the action of the agents already mentioned, does not produce a trace of colouring matter. The subject, which thus appeared to become more and more obscure, was elucidated by a happy experiment.

A mixture of pure aniline and pure toluidine, when heated with mercuric chloride, stannic chloride, or with arsenic acid, instantaneously produced a magnificent red of most intense tinctorial power. This experiment appears to show that the red belongs to both the phenic and toluic series.

I have not as yet pursued my researches further in the new field opened by this experiment.

In conclusion I may be allowed to state that by transforming into oxalate commercial aniline, and especially a specimen of aniline which was furnished to me by Mr. Nicholson as particularly well adapted for the preparation of the red, I have been enabled to obtain considerable quantities of toluidine in a state of perfect purity.

Having thus at my disposal the necessary material, I hope soon to acquire further experimental data for the explanation of the formation of rosaniline.

IX. "On the Measurement of the Chemical Brightness of various portions of the Sun's Disc." By HENRY ENFIELD ROSCOE, B.A., F.R.S. Received June 12, 1863.

The author has applied the method of measurement of the chemical action of sunlight, which Professor Bunsen and he described in a memoir presented to the Royal Society in November last*, to the measurement of the chemical brightness of various portions of the solar disc; and although the observations which have as yet been made are only preliminary, yet he thinks that the results obtained are of sufficient interest to warrant his bringing them before the Society.

Secchi has shown† that the calorific radiation of the centre of

* Abstract, Proc. Roy. Soc. vol. xii. p. 306; Memoir, Phil. Trans. 1863.

† Astron. Nachr. Nos. 806, 833.

the sun's disc is nearly double that from its borders, and that the equatorial regions are somewhat hotter than the polar, whilst observers have long noticed a great difference in luminosity between the centre and edge of the disc.

For the purpose of obtaining a measurement of the relative chemical brightness of various portions of the solar disc, the image of the sun, of about 4 inches in diameter, obtained by a 3½-inch refractor*, was allowed to fall into a camera placed on the instrument, upon a sheet of standard photographic paper prepared according to the method described in the above-mentioned research. The peculiar property of this standard paper is that it can always be prepared of one and the same degree of sensitiveness, and is perfectly homogeneous. The exposure lasted for from 30 to 120 seconds, the sun's motion being carefully followed by a tangent-screw. After exposure, the shade of tint at several points on the picture was determined by comparison with a graduated photographic strip insolated in the pendulum-photometer, and the chemical intensities corresponding to these shades obtained by reference to the Table given in the memoir above cited. The following numbers give the chemical brightness, thus obtained, at various points on the sun's disc on May 9th, 1863. From these numbers it is seen that the intensity of the chemically active rays at the centre is from three to five times as great as that at the edge of the disc, the chemical rays thus showing a wider variation than the calorific rays exhibited as determined by Secchi. This is doubtless owing to the relatively greater absorption effected by the solar atmosphere on the more refrangible chemical rays.

Chemical Brightness of Sun's Disc on May 9, 1863.

	1. At centre of Sun's Disc.	2. At 15° from edge of Sun's Disc.			3. At edge of Sun's Disc.		
		N. Pole.	Equator.	S. Pole.	N. Pole.	Equator.	S. Pole.
No. 1.	100·0	38·8	48·4	58·1	18·7	30·2	28·2
No. 2.	100·0	52·8	......	56·6	30·5	......	41·0

Hence it is likewise seen that on May 9th the chemical brightness of the south polar regions was considerably greater than that of the north polar regions, whilst about the equator the brightness was between that of the poles.

* Kindly placed at my disposal by S. W. Williamson, Esq., of Manchester.

In order to show that the sensitive paper, when exposed to ordinary sunlight, becomes homogeneously tinted, the author appends the readings, taken in the way described, from various portions of a piece of the standard paper used for the sun-pictures exposed for some seconds to direct sunlight.

	Reading.		Deviation from mean.
Portion No. 1 ..	101·4	....	+0·93
„ 2 ..	100·7	....	+0·23
„ 3 ..	98·5	....	−1·97
„ 4 ..	101·6	....	+1·13
„ 5 ..	99·9	....	−0·57
„ 6 ..	100·7	....	+0·23
Mean ..	100·47		

The sun-pictures obtained on the sensitive paper must possess only a slight tint, otherwise the differences in shade cannot be accurately observed; they then exhibit a peculiar coarse mottled appearance, which is not due to imperfections in the paper or the lenses, nor to the action of the earth's atmosphere.

Perhaps these irregular dark and light patches are owing to clouds in the solar atmosphere, and they may have an intimate connexion with the well-known phenomenon of the red prominences.

Mr. Baxendell and the author propose to carry out, according to this method, a regular series of observations of the variation of the relative amounts of brightness on the sun's disc, and they hope before long to be able to present the Society with some further details.

X. "On the Contractility of Healthy and Paralysed Muscles as tested by Electricity." By HARRY LOBB, Esq. Communicated by JOHN SIMON, Esq. Received April 30, 1863.

If a moist conductor from the positive pole of the finer wire of an electro-magnetic battery* be placed upon the skin covering the origin of a healthy muscle, and the moist conductor from the negative pole, upon its belly, and a current of moderate intensity be allowed to pass,

* The apparatus used in these experiments is the small portable machine of M. Duchenne, made by Charrière.

the muscle will contract tonically as long as the current passes; and if it be increased in intensity, cramp will eventually be induced.

The positive pole may be placed upon almost any part of the body to produce this effect; only as it is removed further from the muscle to be acted on, the intensity of the current must be progressively increased.

A healthy muscle contracts with more vigour if the current be direct—that is to say, the positive pole towards the centre, the negative pole towards the periphery.

If a muscle paralysed from recent injury to the brain be acted upon in the same way, it will be found to contract more vigorously than a healthy one under the same intensity of current.

If an extensor muscle paralysed and wasted, the result of poisoning by lead, be treated in the same way, no contraction can be induced even with the highest power of the apparatus; the unparalysed flexors will alone contract.

If a muscle paralysed and wasted from loss of nutrition, as in those local paralyses which are the sequelæ of fever, the exanthemata, convulsions, irritation during teething, &c., be acted on in the same way, no contraction can be induced; if the current is increased in intensity, the healthy or antagonistic muscles contract.

In these two latter instances—after treatment by the continuous galvanic current, when circulation has been re-established, and the paralysed muscles are better nourished—if the current be reversed, the positive pole placed on the insertion of the muscle, and the negative pole on the belly, and if the current is not too strong, faint contraction takes place, gradually increasing until the muscle is sufficiently restored to contract under the direct stimulus.

A singular fact in connexion with these paralysed and wasted muscles is, that they will contract at the will of the patient, for some time, before they will do so to the stimulus of the current; but the paralysed muscles are not safe from a relapse until they contract vigorously to the ordinary direct electrical stimulus.

At a certain stage of improvement, when the paralysed muscle will neither contract to the will nor to the electro-magnetic current, it will do so to the combination of the two.

XI. "On the Influence of Temperature on the Electric Conducting-Power of Alloys." By A. Matthiessen, F.R.S., and C. Vogt, Ph.D. Received June 11, 1863.

(Abstract.)

The subject of this paper has been divided into four parts, viz.:—

I. Experiments on the influence of temperature on the electric conducting-power of alloys composed of two metals.

II. Experiments on the influence of temperature on the electric conducting-power of some alloys composed of three metals.

III. On a method by which the conducting-power of a pure metal may be deduced from that of the impure one.

IV. Miscellaneous and general remarks.

In the first part, after having given the numerical results, we proceed to explain the law which regulates this property. It is as follows:—

The observed percentage decrement in the conducting-power of an alloy between 0° *and* 100° *C. is to that calculated between* 0° *and* 100° *C. as the observed conducting-power at* 100° *C. is to that calculated at* 100° *C.*

Or in symbols,

$$Po : Pc :: \lambda_{100^\circ} : \lambda'_{100^\circ},$$

where Po and Pc represent the observed and calculated percentage decrements in the conducting-power of the alloy between 0° and 100° C.; and λ_{100° and λ'_{100° its observed and calculated conducting-power at 100° C., Pc is equal in nearly all cases to 29·307 *, the exceptions being only in the instances of thallium and iron alloys†.

The above law holds good for most of the alloys belonging to the first and third groups, as well as for a part of those belonging to the second group‡.

Now, if the above proportion,

$$Po : Pc :: \lambda_{100^\circ} : \lambda'_{100^\circ}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

be converted into terms of resistance, the following formula is obtained,

$$r_{100^\circ} - r_{0^\circ} = r'_{100^\circ} - r'_{0^\circ}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where r_{100°, r_{0°, r'_{100° and r'_{0° represent the observed and calculated

* Phil. Trans. 1862. † Proc. R. S. xii. 472. ‡ Phil. Trans. 1860, p. 161.

resistances at 0° and 100° C. The formula, however, expresses the fact *that the absolute difference between* 0° *and* 100° *C. in the resistance of an alloy is equal to the absolute difference between* 0° *and* 100° *in the calculated resistance of the alloy.*

Formula 2 may also be written

$$r_{100^\circ}-r'_{100^\circ}=r_{0^\circ}-r'_{0^\circ},$$

which, if correct, leads to the expression

$$r_t-r'_t=r_{0^\circ}-r'_{0^\circ};$$

that is, *the absolute difference between the observed and calculated resistances of an alloy at any temperature equals the absolute difference between the observed and calculated resistances at* 0° *C.*; *or, in other words,*

$$r_t-r'_t=\text{a constant}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

After giving various examples to show the correctness of the above, we prove that from the expression

$$r_t-r'_t=\text{a constant} \qquad (3)$$

we may deduce the formula for the correction of resistance or conducting-power for temperature of an alloy as soon as we know its composition and its resistance at any temperature; for, as r'_{100°, r'_{0°, and r'_t may be calculated with the help of the formula given for the correction of conducting-power for temperature for most of the pure metals, if the constant $rt-r't$ be determined, then

$$r_{100^\circ}=r'_{100^\circ}+\text{constant},$$
$$r_t\ \ =r'_t\ \ +\text{constant},$$
$$r_{0^\circ}\ \ =r'_{0^\circ}\ \ +\text{constant};$$

and from these terms the formula for the correction of resistance or conducting-power for temperature may be calculated, which in most cases will be found very near the truth.

In the second part we show by a few experiments that most alloys of three metals will probably be governed by the same law with respect to the influence of temperature on their conducting-power as alloys of two metals.

In the third part we deduce

$$P : P' :: M_{100^\circ} : M'_{100^\circ} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

(where P and P′ represent the observed and calculated percentage

decrements in the conducting-power of impure and pure metals between 0° and 100° C., $M_{100°}$ and $M'_{100°}$ their conducting-powers at 100° C.; P′ is for most metals 29·307) from

$$Po : Pc :: \lambda_{100°} : \lambda'_{100°} . \quad (1)$$

For when we consider the last two terms of the proportion, and bear in mind that a trace of another metal has very little or no effect upon $\lambda'_{100°}$ (when it represents the conducting-power of an alloy consisting of one metal with only a trace of another metal), while it alters $\lambda_{100°}$ to a very marked extent, it is evident that $\lambda'_{100°}$ may be replaced by $M_{100°}$.

We verify this by comparing the conducting-power of a pure metal directly determined, with the conducting-power of the same metal deduced from a determination of the conducting-power of its alloy with small quantities of other metals. It is a curious fact, that the deduced values from experiments upon hard-drawn wires are in reality the conducting-powers of the annealed wire of the pure metal. After having thus verified the method, we have not hesitated to employ it in the determination of the conducting-power of certain metals which have not yet been experimented upon in a state of purity.

In the fourth part we point out, first, that the percentage decrement in the conducting-power of alloys between 0° and 100° is never greater than that of the pure metals composing them; secondly, that the conducting-power of alloys decreases with an increase of temperature (some bismuth alloys form an exception to this law); thirdly, that in some cases the percentage composition of an alloy may be deduced from its conducting-power, with the aid of the percentage decrement in its conducting-power; fourthly, the method which we have used for determining the class to which the metals belong in respect to the conducting-power of their alloys; and fifthly, that the results which we have obtained and described in this memoir fully bear out the views put forward in a former one on the chemical nature of alloys.

XII. "On the Peroxides of the Radicals of the Organic Acids." By Sir B. C. Brodie, Bart., Professor of Chemistry in the University of Oxford. Received June 18, 1863.

(Abstract.)

In a former notice published in the 'Proceedings of the Royal Society' (vol. ix. p. 361), an announcement was made of the discovery of a new group of organic combinations, the peroxides of the radicals of the organic acids—bodies which in the systems of the combinations of these radicals occupy the same relative position as is held by the peroxides of hydrogen, barium, or manganese in the systems of the combinations of those elements. An account was given of the mode of preparation and properties of two members of this group, the peroxides of benzoyl and of acetyl, $C_{14} H_{10} O_4$ and $C_4 H_6 O_4$. The present paper contains an extension of this inquiry. In it is given an account of several other peroxides of monatomic radicals, the peroxides of nitro-benzoyl, of cumenyl, of butyl, and of valeryl, and also an inquiry into the action of the peroxide of barium on the bibasic anhydrides.

The nitro-benzoic peroxide is formed by the action of fuming nitric acid on the peroxide of benzoyl. It stands to peroxide of benzoyl in the same relation as anhydrous nitro-benzoic acid stands to anhydrous benzoic acid, and may be regarded as derived from that peroxide by the substitution in it of two atoms of peroxide of nitrogen for two of hydrogen. The formula of the substance is $C_{14} H_8 (NO_2)_2 O_4$.

		Calculated.	Found.
C_{14}	168	50·60	50·60
H_8	8	2·41	2·58
N_2	28	8·43	8·49
O_8	128	38·56	38·33
	332	100·00	100·00

The peroxide of cumenyl is procured by a process strictly analogous to that by which the peroxide of benzoyl is formed; it has the constitution $C_{20} H_{22} O_4$.

The peroxides of butyl and valeryl are prepared by the action of hydrated peroxide of barium on the anhydrous acid. It is only necessary to mix in a mortar equivalent quantities of the two sub-

stances. The peroxide is separated by solution in ether from the water in which it is dissolved and suspended. These substances are dense oily fluids, exploding slightly when heated, but not so readily decomposible as the peroxide of acetyl. The analysis of the peroxide of butyl, dried by chloride of calcium, gave results corresponding with the formula $C_8 H_{14} O_4$.

		Calculated.	Found.
C_8	96	55·17	55·11
H_{14}	14	8·05	8·28
O_4	64	36·78	36·61
	174	100·00	100·00

The analysis of the peroxide of valeryl gave results corresponding with the formula $C_{10} H_{18} O_4$.

		Calculated.	Found.
C_{10}	120	59·40	59·39
H_{18}	18	8·91	9·17
O_4	64	31·69	31·44
	202	100·00	100·00

The mode of formation of these peroxides is given in the equation

$$2 R_2 O + Ba_2 O_2 = 2 Ba\, RO + R_2 O_2.$$

These substances are decomposed as well as formed by the action of the alkaline peroxide, according to the equation

$$R_2 O_2 + Ba_2 O_2 = 2 Ba\, RO + O_2,$$

giving a striking example of those consecutive actions referred to in a former paper as the cause of certain catalytic decompositions.

The action of the bibasic anhydrides on the alkaline peroxides is of special interest.

When anhydrous succinic acid, lactide, or anhydrous camphoric acid is mixed with an equivalent of hydrated peroxide of barium, a solution is obtained possessing the most powerful oxidizing properties, which bleaches indigo, evolves chlorine with hydrochloric acid, and oxidizes the protosalts of iron and manganese, but which does not discolour permanganic acid, or give with chromic acid the blue colour formed by peroxide of hydrogen. When boiled, the solutions evolve oxygen, and afterwards contain a salt of the acid employed—in the case of succinic acid, giving a crystalline precipitate of succinate of

barium, and in the case of camphoric acid, giving with acetate of lead a precipitate of camphorate of lead. These solutions are in a state of continual decomposition. Only in one instance, that of camphoric acid, was it found possible to analyse the substance, and that only by indirect processes. The oxygen contained in the organic peroxide was estimated in a measured portion of the solution by means of a standard solution of iodine; the camphoric acid formed on boiling was determined by precipitation with acetate of lead in another measured portion; and in a third portion the barium was estimated as sulphate. The results of these determinations are given below, the camphoric acid being assumed as correct. They lead to the conclusion that the solution contains the elements of one equivalent of anhydrous camphoric acid, one of oxygen, and one of baryta.

	Atomic weight.	Calculated ratio.	Found.
$C_{10}H_{14}O_3$	182	25·12	25·12
O	16	2·20	2·07
Ba_2O	153	21·12	21·51

the reaction being

$$C_{10}H_{14}O_3+Ba_2O_2=C_{10}H_{14}O_5Ba_2.$$

That the substance formed is to be regarded as the baryta salt of the peroxide of camphoryl, and not as the camphorate of the peroxide of barium, is proved by the reactions of the solution, which does not give peroxide of hydrogen when decomposed by acids, or a precipitate of the hydrated peroxide of barium when heated with a solution of baryta.

The organic peroxides constitute a new and peculiar group of chemical substances characterized by reactions never hitherto found in any compound of carbon, and which materially extend our views of the possible properties of the so-called organic combinations, and of their analogies to inorganic substances. They are the organic representatives of chlorine in the same sense as the oxides of the compound ammoniums are the representatives of potash, and in a yet closer sense than ether and alcohol resemble the oxide and its hydrate, or than ethyl or marsh-gas are analogous to hydrogen. This analogy is of a profound character, not consisting merely in the analogy of symbolic form, but in the absolute identity of reactions.

The admitted analogies of the peroxide of chlorine have as it were their maximum in the organic peroxide. Not only is chlorine represented in the peroxide, but hydrocloric acid is represented in the organic acid, and a series of parallel equations may readily be constructed, showing the identical character of the reactions of the two classes of substances. Both bleach a solution of indigo, oxidize the protosalts of iron and manganese, decompose water under the influence of sunlight, and evolve oxygen with an alkaline peroxide, forming the salt of the corresponding acid.

XIII. "Explorations in Spitzbergen, undertaken by the Swedish Expedition in 1861, with the view of ascertaining the practicability of the measurement of an Arc of the Meridian." By Dr. Otto Torell, Professor of Zoology in the University of Lund. Communicated by the President. Received June 2nd, 1863.

In the year 1858 I made a voyage to Spitzbergen, in company with two other naturalists, in order to investigate the Natural History of that country.

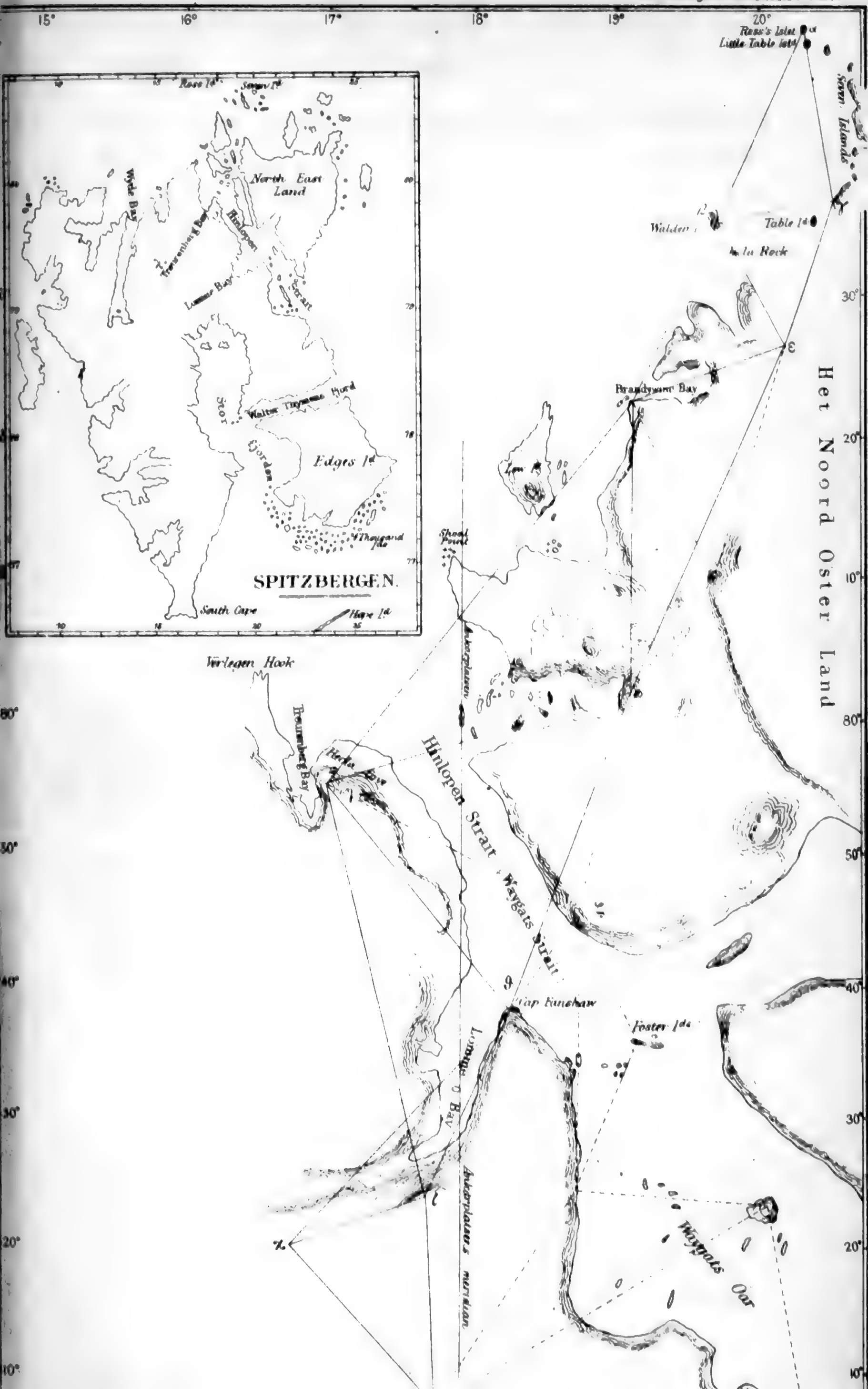
I was thereby induced to study the history of the various Arctic expeditions that had gone out from England.

In Beechey's and Barrow's works I saw mentioned a suggestion which attracted my attention in a high degree. A letter is there given from Captain Edward Sabine to Mr. Davies Gilbert, in which the writer, on his return from his celebrated Pendulum Expedition, proposes to explore Spitzbergen, with the view of ascertaining whether the measurement of an arc of the meridian could be carried out there*.

An arc from Ross Islet to Hope Island would comprise nearly $4\frac{1}{2}°$ of latitude—equivalent to an arc of 9° in the mean latitude of France, and of 7° in the mean latitude of Great Britain.

The difficulties opposed by climate and ground were not considered by Captain Sabine to be so great as not to be surmounted; and he offered, in company with another officer and a sergeant of

* Quarterly Journal of the Royal Institution, vol. xxi. art. xi. pp. 101–108.


15°
16°
17°
18°
19°
20°
Ross's Islet
Little Table Islet
Seven Islands
Walden I.
Table I.
Brandywine Bay
Het Noord Oster Land
Shoal Point
Verlegen Hook
Treurenberg Bay
Hecla Cove
Hinlopen Strait
Waygats Strait
Cap Fanshaw
Foster I.
Lomme Bay
Waygats Oar
SPITZBERGEN.
North East Land
Wyde Bay
Hinlopen
Strait
Lomme Bay
Stor
Walter Thymens Fiord
Gorden
Edges I.
Thousand Is.
South Cape
Hope I.
Ross I.
30°
20°
10°
80°
50°
40°

Artillery, to show either the feasibility or, once for all, the impossibility of the undertaking. A special advantage is pointed out by Spitzbergen being divided into two nearly equal parts from north to south, thus very materially facilitating communications between the different angular points. Admiral Beechey says that Captain Sabine's plan was placed before the Royal Society in 1825 by Sir John Herschel, taken into consideration in the autumn of the same year, and warmly supported by Mr. Davies Gilbert, Sir Humphry Davy, the then President, and by other members of the Royal Society. The reasons why it was not carried out are not mentioned; but Sir John Herschel leaves us to infer that Captain Sabine was called upon to display his powers in another scientific undertaking of a more arduous though not less important kind. This explanation appeared to be natural, and thus the whole matter was shelved for many years.

The plan in question seemed to me so simple and practical, and at the same time so useful in a scientific point of view, that I could not help espousing it with a very warm interest. In the year 1860, the Swedish government and Diet, as well as Prince Oscar, granted funds for a new scientific expedition to Spitzbergen. Being placed at the head of this undertaking, in which a rather large number of scientific men were willing to take part, I did not fail to call the attention of the Academy of Sciences to the plan proposed by General Sabine in 1825. The Academy were alive to its importance; and their two astronomical members, Professor Selander and Assessor Lindhagen, who had themselves taken part in the Swedish-Norwegian triangulation, considered that the explorations ought to be carried out, and for that purpose they issued the requisite directions to two of the participators in the expedition, Messrs. Dunér and Chydenius. To them it was confided to investigate whether suitable angular points could be found from the islands north of Spitzbergen to Hope Island in the South, either along the western coast of Spitzbergen or through Hinloopen Strait and Weide Jans Water, which nearly divide Spitzbergen into two from north to south. One went on board the ship which, according to the plan, was to explore the north of Spitzbergen and Hinloopen Strait, and the other on board the other ship which was to explore the west of Spitzbergen and Weide Jans Water. At the end of May 1861 the two vessels reached Amsterdam Island, in

nearly 80° latitude; and at the commencement of June they passed Verlegen Hook, and anchored in Treurenburg Bay, whence Parry, in 1827, made his celebrated attempt to reach the North Pole. But the polar ice immediately afterwards pressed against the coast, and imprisoned both vessels more than a month in Treurenburg Bay. The pack was so close that no boat excursions of any extent could be made. The explorations for survey were a good deal impeded by this circumstance; for the investigation of the western coast of Spitzbergen could not be commenced until a much later period than intended. The survey of Weide Jans Water could not be carried into effect, owing to drift ice, adverse winds, and calms. Mr. Dunér, to whom this undertaking was allotted, as well as the investigation of the practicability of the survey along the western coast of Spitzbergen, came to the conclusion that no impediments existed for carrying out the triangulation from Ross Islet to Amsterdam Island, but that the mountains surrounding Magdalena Bay are so steep and difficult or impossible of access, that the continuation of the survey southwards must be considered, if not absolutely impossible, at least so difficult and entailing such heavy expense, that its execution along that coast will probably never be carried into effect.

Mr. Chydenius, who was to explore the northern portion of the arc, presumed to be measurable from Ross Islet to Hope Island, was more fortunate in his work. During sundry boat excursions and ascensions of many mountains from the northernmost part of Spitzbergen to the termination of Hinloopen Strait, he succeeded in completely solving the problem as to that part of Spitzbergen, comprising nearly the half of the arc to be measured. The survey was carried out courageously and energetically under circumstances of frequent difficulty, as well in the drift ice as in crossing the glaciers of the interior.

The accompanying map makes a detailed description unnecessary, and I therefore confine myself to stating that all the lines of the sights in the network marked with continuous lines are, with one single exception, observed, and that Mr. Chydenius has had opportunities of convincing himself that though not all the lines of the sights in the network marked with dotted lines are observed, yet nothing prevents the angular points connected by them from being seen one from

the other. The triangles connected by continuous lines of the sights are *nine* in number. Their angles are computed by Mr. Chydenius in an accompanying Table. All the angular points are selected on moderately high and accessible mountains, situated close to, or not very far from the coasts, and the distances between them are not greater than will admit of the signals being easily seen. Mr. Chydenius found the ground, as well in Low Island as to the west of Treurenburg, to be favourable for measuring a base.

As the survey, so far as carried out, proves that, for executing the measurement of an arc of the meridian, no impediments exist which may not be overcome by courage and perseverance, there remains the question whether the part not yet explored may be expected to be equally favourable; the reply to this cannot, of course, be fully given until a similar survey has been made of the still unknown portion; there are, however, means of partially judging of the prospects of success. Mr. Chydenius considers himself almost justified in stating with certainty that the mountains marked λ and χ on the map will be found to be visible from Weide Jans Water. He is inclined to think that the easiest communication may be made through Lomme Bay. The distance from Hinloopen Strait to Weide Jans Water cannot be great. According to statements which, however, we were unable to verify, there is said to be a strait connecting these two sheets of water, and a vessel is reported to have sailed through it. Mr. Lamont is also of opinion that Weide Jans Water is open to the north. This, if found to be true, would tend greatly to facilitate the work.

It is also probable that the network can be drawn from the Waigat Islands at the southern embouchure of Hinloopen Strait to the Walter Thymen Bay, through the latter to Weide Jans Water, and further to Hope Island.

There still remains the question as to the facilities that the land on the two sides of Weide Jans Water may afford for the survey. Those walrus-hunters whom I have interrogated regarding that part of the country, are unanimous in their opinion as to the mountains on the western side being similar to those on the west coast, that is to say, as inaccessible as possible. But the country to the east of the said Water is described as a tableland, in which accessible mountains may be found in several places. There are therefore

well-founded reasons for thinking that the whole arc will be found measurable if the survey is continued. Mr. Chydenius has offered to furnish the remaining part of the exploration.

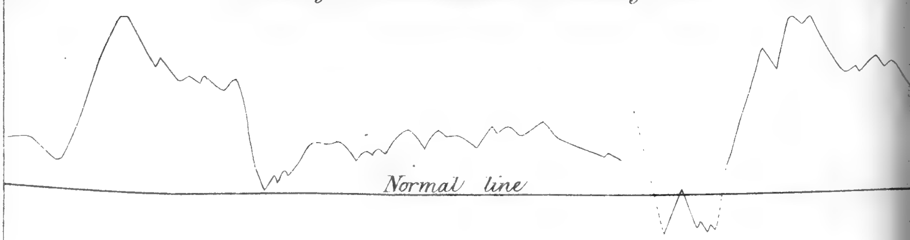
The Swedish Academy of Sciences consider the completion of the survey so important, that they have petitioned Government to supply funds for carrying it into effect during the present or next year. There is every probability that the money will be granted, and, if the result turn out as expected, that necessary steps will be taken for executing the measurement of the arc itself.

The Swedish Government has, at the instance of the Academy of Sciences, already furnished means for preliminary investigations in reference to another geodetic enterprise, namely, for the Swedish share of the proposed large middle-European triangulation from Palermo to Trondhjem, and have asked the Estates for money for executing the measurement. Should, then, the survey in Spitzbergen also be carried out, an important contribution will be made, not only to ascertain the compression of the globe in the vicinity of the North Pole, but also for the much-sought-after knowledge of the real form of the earth on different portions of its surface; and the undertaking will to a certain degree complete the results both of the projected middle-European triangulation and of the Russo-Scandinavian already effected.

If the triangulation in question be executed, it will not be the only result arising from several years' scientific labours in Spitzbergen. It is superfluous here to allude to many investigations of importance which may be made; it is sufficient to keep in mind the situation of Northern Spitzbergen, distant scarcely 10° of latitude from the North Pole. There are well-founded reasons for thinking that the execution of the measurement in question may be looked forward to. And if we seek for the origin of the whole matter, we can trace it in Captain Sabine's well-planned and lucidly explained project, which he submitted to the examination of the Royal Society in 1825.

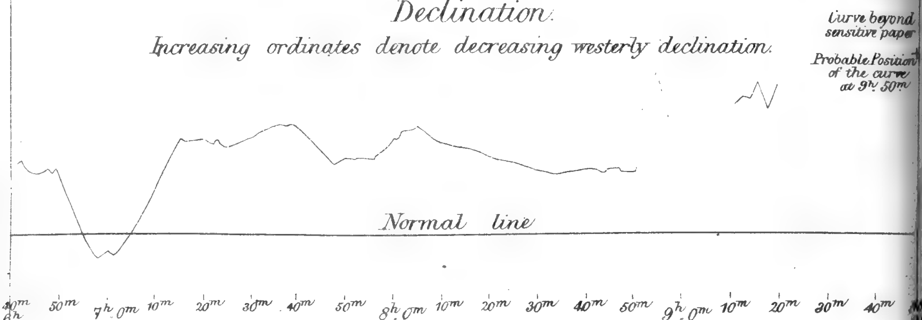
Horizontal Force.

Increasing ordinates denote decreasing force.

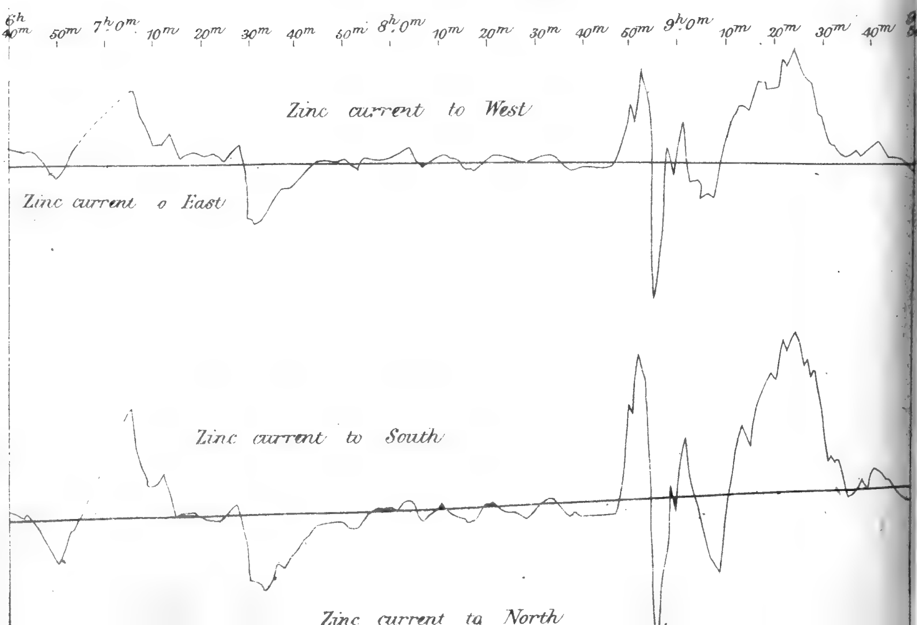


Declination.

Increasing ordinates denote decreasing westerly declination.



Earth Currents.



XIV. "On the Magnetic Disturbance which took place on the 14th of December 1862." By Balfour Stewart, M.A., F.R.S. Received May 19, 1863.

On the 14th of December 1862, a magnetic disturbance occurred about 6 o'clock in the afternoon, and was registered by means of the Kew magnetographs. As usual it was accompanied by an auroral display and by earth-currents, and the latter phenomena were observed at Greenwich by means of a system of telegraphic wires which had recently come into the possession of the Astronomer Royal.

By the kindness of Mr. Airy, the Kew Observatory has been favoured with a copy of the curves which represent the earth-currents collected at Greenwich during the progress of this disturbance; and a comparison of these with the Kew magnetograph curves will form the subject of the following paper. It will, however, first be necessary to state the arrangement adopted at Greenwich. There are two wires proceeding from the Observatory, one ending near Croydon and the other near Dartford (nearly at right angles); and these are well insulated throughout their entire length, and have good earth-connexions at their extremities.

From these, by means of galvanometers, the intensity of earth-currents is recorded for two very favourable directions; and from these again the intensity of those currents which flow in the magnetic meridian, and in a direction perpendicular to it, may be very easily deduced. The standard for direction is the current which flows through the wire of a battery from the zinc pole, and which is called the zinc-current. With this explanation the earth-current diagrams appended to this paper will be quite intelligible; and with regard to the magnetic curves, it is only needful to remark that increasing ordinates denote decreasing declination and decreasing horizontal force, and that the normal lines, which have been furnished through the kindness of General Sabine, denote the position which the curves would have occupied had no disturbance supervened. But before proceeding to compare together the two sets of curves, it will be necessary to advert to a peculiarity of disturbances which enters as an essential element into all such discussions. It has been found by General Sabine that if the disturbances of declination be divided into two categories, easterly and westerly, these obey very different laws

of daily variation, and also that this difference is not of the same description for all stations; so that we are compelled to view a magnetic disturbance as the resultant effect of two disturbances of different character, superimposed upon one another. General Sabine has likewise stated his opinion that this duality of action may perhaps be due to the disturbing force entering the earth at two or more points, one denoting magnetism of a more permanent kind, and the other magnetism of an induced description. A study of the Kew disturbance-curves tends to give confirmation to such an idea; for in these it is seldom found that the whole body of force which produces a disturbance is one which preserves the same type throughout and only varies in intensity. Even if we suppose that this type will vary with the hour of the day, we shall find, if we take disturbances which last for several days, that the type of force at a given hour of the first day is in very many cases different from that during the same hour of the second. When, however, we confine our attention to very abrupt changes of force, we find that the disturbance-type which these display retains more of the same character throughout a disturbance. In order to explain this, we may perhaps suppose that there are two sets of magnetic particles in the earth—one set being of the nature of soft iron, and the other similar to hardened steel. Now only the first of these would be acted on by any very sudden change in the disturbing force, since it would require time in order to influence the second set. We may thus perhaps account for the fact that any very sudden change is of one type, since it only influences one set of particles. Let us now consider what will take place if a disturbance of the same primary nature continues for any length of time. Here the hard-iron particles will also be influenced to an extent compounded of the time and of the average value of the disturbing force during that time. The magnetic needle, therefore, will now be acted on by the joint influence of these two sets of particles, whereas at the beginning of the disturbance it was only acted on by one of them, namely, the soft-iron ones. The type of force will therefore have changed if the hard-iron particles are differently distributed in our globe from the soft-iron ones; and if, instead of two, there are many sets of particles, we shall have a very complicated effect.

Now this duality of disturbing forces must be considered when we

attempt to ascertain the connexion between such forces and earth-currents, since we are not entitled to suppose that one of these two forces is related to earth-currents in precisely the same manner as the other. A small soft-iron force may be comparable to a large hard-iron one as far as an earth-current is concerned; or the reverse may be the case. When, however, there is a very rapid change of disturbing force, since this affects the earth through only one set of particles, we shall by its means be better able to trace the bond of connexion between a single type of magnetic force and the corresponding earth-current. Rapid changes of force are therefore of peculiar value in such an investigation.

Bearing this in mind, let us endeavour to connect together the phenomena of earth-currents and magnetic disturbances by two successive hypotheses, one of which, it would seem, must represent the truth unless there be some new link of connexion between magnetism and electricity with which we are unacquainted. The first hypothesis is that in which earth-currents are supposed to give rise to magnetic disturbances according to the laws by which a current acts upon a magnet. This may be called the theory of direct action.

By the second hypothesis, earth-currents are supposed to be induced, or secondary currents generated in the crust of the earth by those small but rapid changes in terrestrial magnetism which constitute disturbances. This may be called the theory of induced action.

Now, first, on the theory of direct action. Zinc-currents going to the south should correspond with magnetic disturbances decreasing the declination; and zinc-currents going to the east should correspond with an increase of horizontal force; but we find by the diagrams that while on this hypothesis the disturbance of the horizontal-force needle will be tolerably well accounted for by the east and west currents, the same correspondence does not hold between the north and south currents and the declination-disturbance. But I do not think that this circumstance, rightly viewed, tells either in favour of or against the hypothesis. A glance at the earth-current curves will show that the ordinates of the one bear generally a fixed proportion to those of the other, showing us that the total current has flowed backwards and forwards along one line*; and though it is equally apparent, by a glance at the magnetic curves, that the same type of force

* This had been previously observed in other cases by Mr. C. V. Walker.

has not been preserved throughout the disturbance, yet the explanation of the unity of type in the earth-currents may be that these are twisted into a line of motion, owing to the disposition of the conducting strata of the earth's surface, just as a current can only move backwards or forwards along an insulated wire. Indeed a little reflection will show us that earth-currents are not local phenomena; so that if we endeavour to estimate quantitatively their influence on the magnet according to the hypothesis of direct action, we must first extend our field of observation, and obtain their value in other countries besides our own.

But to return to our comparison of curves. We see that for the greater part of the disturbance both the horizontal force and the declination were very much above their normal lines, while on the other hand the currents were frequently crossing their zero lines,—and that both currents were simultaneously and for a long portion of their time very near zero, although during this time the magnetic disturbance was considerable.

Next, with respect to a very abrupt disturbance which commenced about $8^h\ 50^m$, the corresponding earth-current curves are exceedingly powerful, alternately passing and repassing the zero line to nearly the same distance on both sides, while the absolute disturbance of the horizontal force, and probably of the declination, was not very great.

We have thus, in the first place, a very sluggish action of earth-currents, while the magnetic disturbance was considerable, and in the next place a very violent action of the former when the absolute disturbing force was by no means excessive; and we may add that at $9^h\ 50^m$ both earth-currents were near zero, while both elements of the earth's magnetism were much disturbed.

For all these reasons this comparison of the curves is unfavourable to the hypothesis of direct action.

Let us now consider the other hypothesis, or that of induced action, and we shall find the following points in its favour.

1st. That in this disturbance, for at least one hour, both elements of the earth's magnetic force remained at a considerable distance from their normals, and that during this time the earth-currents observed were exceedingly small. Now, on the hypothesis of induced action, the earth-current effect depends not on the absolute value of the disturbing force, but on its rate of change; and if during this

period we examine the horizontal-force curve, we shall find the abruptness of change not so great as in those cases where greater earth-currents were produced, while in the declination-curve the abruptness of change during this period is exceedingly small.

2nd. A reference to the diagrams will show us that in general the most abrupt magnetic disturbances are those which are accompanied by the greatest earth-currents, and that in particular a very abrupt disturbance, which took place about $8^h\,50^m$, was accompanied by very strong earth-currents, alternately positive and negative, those of the one name being nearly as powerful as those of the other, while on the other hand the corresponding magnetic disturbances were on an average decidedly on one side of the normal lines.

On the other hand, the following fact seems at first to tell against the theory of induction. An inspection of the curves will show that we have currents remaining on one side of the zero-line for some length of time, during which the magnetic disturbances have nevertheless changed in both directions. When, however, we reflect on this circumstance, we are led to see that since we have two sets of disturbances taking place simultaneously, so we must also have two sets of earth-currents. Now one of these disturbances, which we may perhaps call the soft-iron one, reproduces those small and rapid changes which take place in the primary force, while on the other hand the hard-iron disturbance averages these small changes and presents us with a disturbance-wave of long period. Precisely, then, as in the magnetic curves we have waves of short period superimposed upon waves of long period, so will it be in the earth-current curves. Those currents due to the soft-iron disturbances will be superimposed upon those due to the hard-iron ones, with this difference, that we are not entitled to assume that the proportion in intensity between the two simultaneous earth-currents must be precisely that which exists between the rates of change of the two corresponding simultaneous disturbances. It will be apparent that this feature of duality ought also to be presented by the aurora; and here it is well known that we have at least two phenomena, one of a more fitful and the other of a more permanent character, namely, the streamers and the auroral arch. We may suppose the first of these phenomena to correspond to the soft-iron, and the second to the hard-iron disturbances. Indeed it is questionable whether the different

varieties of auroræ are confined to these two; for General Sabine has informed me that he himself, along with the late Sir Edward Parry, observed at Lerwick in the Shetland Isles in 1818, at the same instant, two auroral arches crossing one another at an angle. But, be this as it may, when we reflect that there are many kinds of particles in our earth, some of which may be affected more rapidly than others by a primary magnetic force, we shall cease to wonder that the phenomena presented are of a complicated description.

All these considerations have induced me to think that it is lost labour to attempt a quantitative comparison when our observation of the magnetic disturbances and their corresponding earth-currents is confined to one locality; and it will be seen from this paper, that while endeavouring to uphold the hypothesis of induced action, I have done so by a comparison of a general and qualitative rather than by one of a quantitative nature.

XV. "Further Observations in favour of the View that Nerve-fibres never end in Voluntary Muscle." By Lionel S. Beale, M.B., F.R.S., Fellow of the Royal College of Physicians, Professor of Physiology and of General and Morbid Anatomy in King's College, London; Physician to King's College Hospital, &c. Received June 5, 1863.

Few anatomical inquiries of late years have excited more interest than the present one. Since my paper published in the 'Philosophical Transactions' for the year 1860, several memoirs have appeared in Germany. In my paper just published in the last volume of the 'Transactions,' I have replied to the statements of Kühne and Kölliker, but I had not succeeded in actually tracing the very fine nucleated fibres I had demonstrated from one undoubted nerve-trunk to another. As a *demonstration*, therefore, my conclusions *were defective*, though the only explanation to be offered of facts I had observed was that included in the view I propounded in my first paper. The question between my opponents and myself upon this matter is not one of interpretation, but a question of simple fact. I assert that the fine nerve-fibres can be followed much further than the point where Kühne and Kölliker maintain the ends or *termina-*

tions are situated, if the specimen be so prepared as to prevent destruction of these most delicate fibres, and the refractive power of the medium be such as to enable us to see them.

I propose to present to the Royal Society next session a paper in which I shall *demonstrate* the truth of the conclusions I have arrived at; but as my specimens are already prepared, and during the last few months several drawings have been made, I hasten to give a short statement of facts, in order that those who have been led to conclusions opposed to my own may have an opportunity of studying the very same muscle.

The great width and refractive power of the large elementary fibres of the pectoral of the common frog render it impossible to follow for any great distance amongst them nerve-fibres of the $\frac{1}{60,000}$th of an inch = ·000187‴ in diameter; and I have therefore long been searching for a very thin voluntary muscle, with fine fibres, which, like the bladder of the frog, could be examined without the necessity of making thin sections, and thereby deranging the relation of all the finest and most delicate structures. Such a muscle I have found in the *extensive mylo-hyoid of the little green tree-frog* (Hyla arborea). The elementary fibres of this muscle are scarcely more than the $\frac{1}{3000}$th of an inch = ·0036‴ in diameter; and as there are but two layers, the fibres of which are at right angles to each other, all the structures in the muscle can be demonstrated most beautifully. The very long thin muscular fibres are not too close for exact observation. The vessels can be readily injected.*

These specimens have been prepared upon the same plan as others, and are preserved in glycerine, which enables me to press the thin muscle and separate the fibres further from each other, while the finest fibres of the nerves are prevented, by the viscid medium, from breaking or from being so compressed amongst the other tissues as to be destroyed or rendered invisible. The muscle

* The very thin and wide intercostal muscles of the Chameleon, after having been soaked in glycerine, may be separated into two layers, *external* and *internal intercostals*, in each of which the finest ramifications of the nerve-fibres may be followed, and their relation to the sarcolemma demonstrated. The long elementary fibres of the thin tubular part of the tongue of the same animal are also favourable for this investigation; but the Chameleon is only to be obtained occasionally, and the muscle of the green tree-frog, above referred to, possesses many advantages.

must be prepared when quite fresh, otherwise the fine nucleated fibres are completely disintegrated. The capillaries were injected as in the other cases*.

In this thin muscle, networks formed by bundles of dark-bordered fibres, consisting of from two to five or six, may be very easily shown, and with high powers (700 to 3000 diameters) the very fine nucleated fibres resulting from the division and subdivision of these in a dichotomous † manner, can be readily demonstrated.

In this thin muscle I have often followed individual fine nucleated nerve-fibres, now over, now under muscular fibres, sometimes crossing transversely, sometimes obliquely, and sometimes running for a certain distance parallel to the fine muscular fibre. The drawing accompanying this paper renders further description unnecessary. I shall enter into full detail in my communication next session; but as the summer is the period to obtain specimens of the *Hyla*, I am anxious my fellow-labourers in Germany should at once be acquainted with the advantages of the thin muscle alluded to; and I cannot too strongly recommend this beautiful little frog, which they have the advantage of procuring more readily than Englishmen, for microscopical investigation. All the tissues are beautifully distinct, and I challenge those who are interested in these questions to discuss them with me, selecting the tissues of this animal for special study.

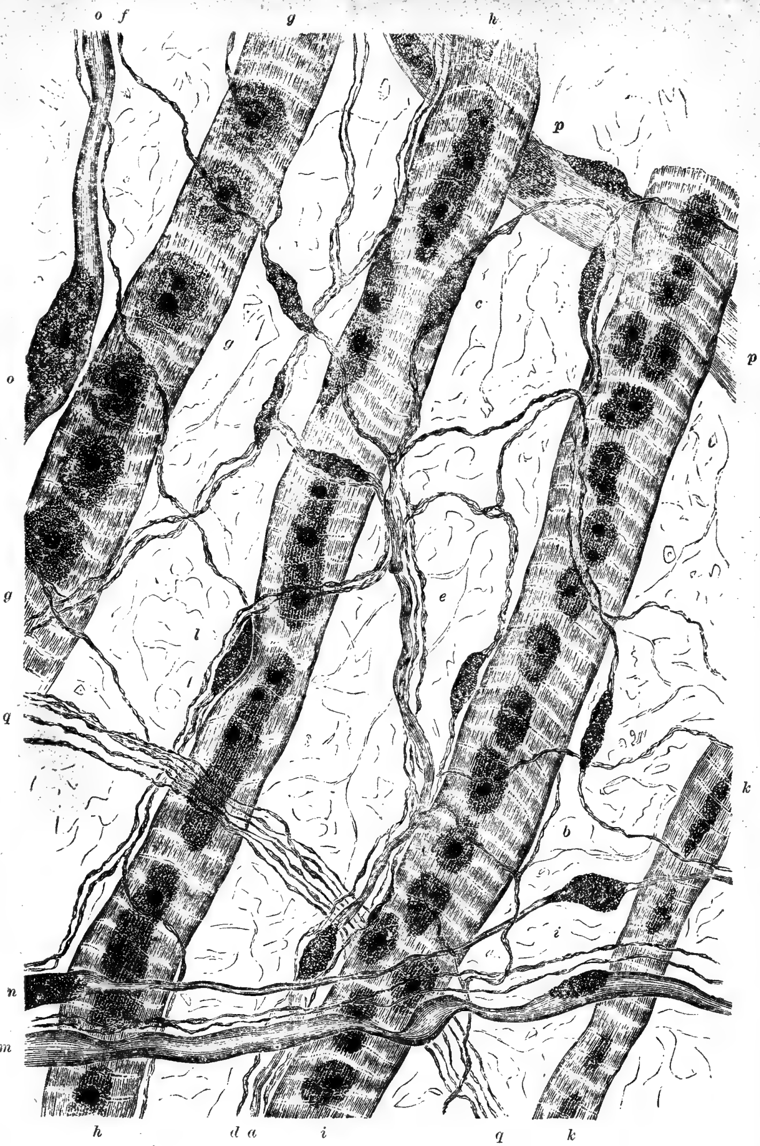
Explanation of the Plate.

Distribution of finest nucleated nerve-fibres to the very narrow elementary muscular fibres of the mylo-hyoid of the little green tree-frog (Hyla arborea), *magnified* 1700 *diameters. Drawn on the block by the author.*

The elementary muscular fibres are marked *g, h, i, k*. *k* is a very young one, slightly stretched; *i* is a fully-formed muscular fibre; *h*, another stretched in its central part. The nuclei of these fibres exhibit some differences in size and form. Nucleoli are distinct in all, and in the fibre marked *g* the nuclei, which were coloured by carmine, exhibit three different intensities of colour,—the dark central spot, "nucleolus," being most intensely coloured, as indicated by the shading in the drawing.

* As the details of the mode of preparing these specimens would occupy many pages, I must defer entering into this part of the question; and it is useless to give the outline, as success depends entirely on minutiæ.

† The dichotomous division is most common; but sometimes three, four, or even five branches result from the division of one fibre, as is well known to be the case in the common frog.



Distribution of finest nucleated-Nerve Fibres to the Elementary Muscular Fibres of the Mylo-hyoid Muscle of the little Green Tree Frog (Hyla Arborea). Drawn on the block by the Author, from a specimen magnified 1700 diameters (the first twenty-fifth made by Messrs Powell & Lealand). The diameter of each muscular fibre corresponds to that of a human red blood-corpuscle.

SCALE. $\frac{1}{10,000}$ of an English Inch × 1700 diameters. $\frac{1}{50000}$

a is a nerve-fibre which was followed over more than twenty elementary muscular fibres from a dark-bordered fibre. One of the subdivisions of this fibre is seen at *f*, where it again runs with a very fine dark-bordered fibre (*o*). The dark-bordered fibre (*o*) was some distance higher up in the specimen, but its place has been altered in order to avoid the necessity for a still larger drawing. Above *b* a nucleus of a very fine nerve-fibre is seen. Such nuclei lie upon the surface of the muscular fibres, external to the sarcolemma. The nucleus often *appears* as if it were within the sarcolemma (*c*), but the fibres proceeding from each extremity render such a position impossible. The relation of these nerve-nuclei to the sarcolemma is seen at *l* in profile. The nuclei, as well as the fibres for a certain distance, often adhere to the sarcolemma very firmly ; but in the thin mylo-hyoid muscle the course of the fibres over or under, but always *external* to the muscular fibres, may be readily traced if the muscular fibres be separated slightly from each other, as represented in the drawing.

At *d* fine nerve-fibres accompanying the fine fibre continued from the dark-bordered fibre, as described in the 'Philosophical Transactions' for 1862, are represented. Such fibres are also seen at *e* and *f*.

m, *n*, and *o* dark-bordered fibres, with nuclei near their distribution. *m* would probably pass over sixty or seventy muscular fibres, and *n* over perhaps twenty, before it divided into fibres as fine as those seen at *b*, *e*, *f*, *l*.

p a very fine capillary vessel with a nerve-fibre running close to it.

q a bundle composed of six very fine nerve-fibres near their distribution. These fibres exhibit a very distinctly beaded appearance, which is also observed in many other fine fibres in different parts of the specimen.

Traces of connective tissue are seen in all parts near the fine nerve-fibres and around the muscular fibres. Here and there some very fine connective tissue-fibres, which were not altered by acetic acid, are represented. These represent the remains of fine nerve-fibres, which existed in a state of functional activity at an earlier period.

The drawing, with the exception of the position of the nerve-fibre (*o*) above mentioned, is an actual copy from nature. The relative position of the muscular fibres, the form and general characters of the so-called nuclei, and the position and size of the nerve-fibres and their nuclei have been carefully preserved.

I have traced the very fine nerve-fibres in so many instances from one trunk to another ramifying at a very considerable distance, that I cannot believe any true terminations or ends exist.

XVI. "Note on the Minute Structure of the Grey Matter of the Convolutions of the Brain of Man, the Sheep, Cat, and Dog." By Lionel S. Beale, M.B., F.R.S., &c. Received June 18, 1863.

By a new process of investigation, I have succeeded in demonstrating the connexion between the nerve-cells and fibres in the grey matter of the convolutions and in other parts of the mammalian brain, and have followed individual fibres for a much greater distance

than can be effected in sections prepared by other processes of investigation which I have tried. In many instances one thick fibre is continuous with one or other extremity of the "cell," while from its opposite portion from three to six or eight thinner fibres diverge in a direction onwards and outwards. This arrangement is particularly distinct in the grey matter of the sheep's brain. The broad extremities of the cells for the most part are directed *towards* the white matter of the hemisphere. The cells have no cell-wall; and the matter of which the fibre is composed is continuous with that of which the outer part of the "cell" consists.

The so-called "granular matter," or "granular matrix," which is described as existing in considerable quantity in the grey matter around the cells and between the fibres, results, I think from the disintegration of the finest nerve-fibres and cells; for in the specimens I have prepared, the tissue intervening between the cells is seen to be composed *entirely of nerve-fibres.* The majority of these fibres are not more than the $\frac{1}{100,000}$th of an inch in diameter; and many fibres, whose continuity with cells can be demonstrated, are as fine as this at a distance of not more than $\frac{1}{1000}$th of an inch from their origin. The slightest displacement of the thin glass covering the specimen will often destroy these delicate fibres, and nothing but amorphous granular matter results. The apparently free nuclei in the more superficial part of the grey matter are really the nuclei of cells, *with every one of which fibres are in connexion.* There are no cells with less than two fibres in continuity with them. In the brain of a young animal it is easy to find many cells connected together by broad bands of fibres, which vary much in length and thickness; but in the adult a positive connexion between two *contiguous* cells can be demonstrated only here and there. As the cells separate further and further from each other, the fibre becomes so thin and long, and it changes the plane in which it ramifies to such an extent, that it cannot often be traced for any great distance.

The fibres are formed as the cells, originally continuous, become separated further and further from each other. In all parts of the nervous system the so-called "cells" (often termed "nuclei" in peripheral parts) are structurally continuous with each other through the fibres.

The fibres are not offsets or outgrowths which grow centrifugally from different adjacent cells and then join or anastomose with one another, but they result, on the other hand, from the moving away from each other of cells which were originally continuous; so that a very thin fibre of $\frac{1}{100,000}$th of an inch in diameter, and perhaps three or four thousandths of an inch in length, represents the broader and shorter bands or cords of communication which existed at an earlier period of development, when the cells were much closer together.

It has been stated that nerve-cells do not exist in the white matter; but numerous cells are present in this tissue for the distance of perhaps the sixth of an inch or more beneath the grey matter. I do not propose now to enter into detail; the arrangement of the cells and fibres will be accurately represented in drawings; for it is not possible for me to convey an accurate idea of the structure of this elaborate tissue by description. I may be permitted to offer the following general conclusions resulting from observations upon the grey matter of the convolutions of man, the sheep, cat, and dog.

1. The numerous nerve-cells of the grey matter are *all* connected or give origin to at least two fibres.

2. These fibres, wide near their origin, gradually diminish in thickness until they are not more than the $\frac{1}{100,000}$th of an inch in diameter.

3. The granular matter said to be so abundant in the grey matter consists of fine and very delicate nerve-fibres, which are easily broken down when thin sections of this soft tissue are submitted to microscopical examination without special preparation.

4. It is probable that the cells of the grey matter of the convolutions are connected together; but in the adult the cells are not often connected with those cells which are situated *nearest* to them.

5. There is no reason for supposing that the nerve-cells, here or elsewhere, influence any nerve-fibres save those *which are structurally continuous with them.*

XVII. "On the Change in the Elastic Force of a Constant Volume of dry Atmospheric Air, between 32° F. and 212° F., and on the Temperature of the Freezing-point of Mercury." By BALFOUR STEWART, M.A., F.R.S. Received June 18, 1863.

(Abstract.)

The author gave a detailed description of his apparatus, and of the method employed in drying the air.

The result of his experiments gave as the coefficient of increase of elasticity of air of constant volume for 1° F. 0·002040, this being slightly different from that given by Regnault, which is 0·002036.

He also finds that the temperature of the freezing-point of mercury is constant, and that its value on Fahrenheit's scale, as given by an air-thermometer, is $-37°·93$, while as recorded by a standard mercurial thermometer it is $-38°·00$. It is probable that this difference is owing to an anomalous contraction of mercury before it freezes, similar to the corresponding expansion of water; but this effect in the case of mercury seems to be very small, and it may be said that a mercurial thermometer properly graduated, will denote the true temperature, even down to the freezing-point of the mercury itself.

XVIII. "On the Degree and Weight of the Resultant of a Multipartite System of Equations." By Professor J. J SYLVESTER, F.R.S. Received May 25, 1863.

Let there be $(1+n)$ equations each homogeneous in any number of sets of variables, and suppose that the degrees of the several equations in respect to these sets are respectively

$$\begin{array}{l}
a,\ b,\ c,\ \ldots,\ l, \\
a_1,\ b_1,\ c_1,\ \ldots,\ l_1, \\
a_2,\ b_2,\ c_2,\ \ldots,\ l_2, \\
\ldots\ldots\ldots\ldots\ldots \\
\ldots\ldots\ldots\ldots\ldots \\
a_n,\ b_n,\ c_n,\ \ldots,\ l_n,
\end{array}$$

where the a, b, c, &c. are any positive integers, zero not excluded.

Let the number of variables in the several sets be respectively

$1+\alpha$, $1+\beta$, $1+\gamma$, . . . $1+\lambda$, then in order that the system may have a resultant, since the number of ratios to be eliminated is $\alpha+\beta+\gamma+\ldots+\lambda$, this sum must be equal to n.

Let

$$a_i\varrho+b_i\sigma+c_i\tau+\ldots+c_i\omega=L_2,$$

and let

$$LL_1, L_2, \ldots L_n=P, \text{ then}$$

1st, the degree of the resultant in question in regard to the coefficients of the rth equation will be the coefficient of $\varrho^\alpha . \sigma^\beta . \tau^\gamma \ldots \omega^\lambda$ in $\frac{P}{L_r}$.

2nd. As regards weight. By the weight of any letter in respect to any given variable is to be understood the exponent of that variable in the term affected with the coefficient; and by the weight of any term of the resultant in respect to such variable, the sum of the weights of its several simple factors; each term in the resultant in respect to any given variable has the same weight; and this weight may also be proved to be alike for each variable in the same set, and may be taken as the weight of the resultant in respect to such set. This being premised, we have the following theorem;—

The value of the weight of the resultant in respect to any particular set of the variables, *ex. gr.* the $(1+\alpha)$ set, will be the coefficient of

$$\varrho^{1+\alpha} . \sigma^\beta . \tau^\gamma \ldots \omega^\lambda \text{ in } P.$$

In the particular case where $\alpha=\beta=\gamma\ldots=\lambda$, the above expressions for the degree and weight evidently become polynomial coefficients. Thus, *ex. gr.*, if we suppose each equation *linear* in respect to the variables of each set, the degree of the resultant in respect to the coefficients of any equation will be

$$\frac{\pi(\alpha+\beta+\gamma\ldots+\lambda)}{\pi\alpha . \pi\beta . \pi\gamma \ldots \pi\lambda},$$

and its weight in respect to the $(1+\alpha)$ set will be

$$\frac{\pi(1+\alpha+\beta+\ldots+\lambda)}{\pi(1+\alpha)\pi\beta . \pi\gamma \ldots \pi(\lambda)}.$$

In particular if each set is binary, so that $\alpha=\beta=\gamma\ldots=\lambda=1$, the degree becomes $\pi(n)$, and the weight $\frac{\pi(1+n)}{2}$.

The above theorems are, I believe, altogether new.

It may just be noticed (as a passing remark) that the total degree in the general case is the coefficient of

$$\varrho^{\alpha} . \sigma^{\beta} . \tau^{\gamma} \ldots \omega^{\lambda} \text{ in } \mathrm{P}\left\{\frac{1}{\mathrm{L}}+\frac{1}{\mathrm{L}_1}+\ldots+\frac{1}{\mathrm{L}_n}\right\},$$

and the *total* weight the coefficient of the same argument in

$$\mathrm{P}\left\{\frac{1}{\varrho}+\frac{1}{\sigma}+\ldots+\frac{1}{\omega}\right\}.$$

XIX. "Some Remarks appended to a Report on Mr. Hopkins's Paper 'On the Theory of the Motion of Glaciers'*". By Sir JOHN F. W. HERSCHEL, Bart., F.R.S. (Referee). Received January 31, 1863.

A few remarks arising out of the perusal of this paper may perhaps not be considered as out of place on the present occasion. They are not meant as in any way impugning the author's views of the laws determining the fracture and disruption of glacier masses, or their application to glacier-phenomena in general, but in relation to the somewhat mysterious process of regelation itself, and to those generally recognized and most remarkable facts of the gradual conversion of snow into more or less transparent ice, and the reunion of blocks and fissured or broken fragments, under the joint influence of renewed pressure and of that process (whatever its nature), into continuous masses. If regelation be really a process of crystallization, it seems exceedingly difficult to imagine how the molecules forming the cementing layer between two juxtaposed surfaces can at once arrange themselves conformably to the accidentally differing axial arrangements of those of the two surfaces cemented. A macled crystal is indeed a crystallographical possibility; but then the axes of the two individuals cohering by the macle-plane have to each other a definite geometrical relation in space, as is well exemplified in the case of the interrupting film in Iceland spar. At the temperature at which "regelation" takes place (viz. the precise limit between the liquid and solid states), it seems to me very possible that the cohesive forces of the molecules of the cemented surfaces may be so nearly counteracted as to bring those surfaces into what may be so far regarded

* Read May 22, 1862.

as a viscous state as to permit (not indeed a sensible and finite change of figure of a small portion of the mass without fracture, but) a certain freedom of movement in the individual molecules, to some sensible depth within the surface, so as to allow of a *gradually progressive* deviation of *their axes* from exact parallelism, and thus to effect a transition from one crystalline arrangement to another—not by macling, but by curvilinear distortion, such as may be conceived to prevail in pearl-spar and other similar disturbed forms of crystals. Nay, I can conceive it possible that by very long continuance at this exact temperature (especially if aided by tremors short of disruption propagated through the mass, which, as we see in the crystallization of cold wrought iron in the axle-trees of railway carriages, powerfully favour the crystalline rearrangement of molecules even in the most rigid solids) the contiguous blocks may influence each other's crystallization to a greater and greater depth through the medium of the cementing film, thus tending continually to straiten the curve of the connecting chain of axes, and after a very long time to bring the two blocks into perfect conformity, so as to form an uninterrupted crystal; and this, or something like this, I take to be the process by which the snow of a *nevé* is converted into the imperfectly transparent and sometimes fully transparent ice of a glacier. Tremors of the kind here alluded to would not be wanting in a glacier in continued process of displacement, and in some part or other of which disruptions consequent on violent strain are momentarily taking place.

On the subject of the temperature of the interior of a glacier, I would observe that there will be found in the archives of the Royal Society, on the occasion of the Committee for recommending objects of inquiry to Lieut. Foster during one of his Polar expeditions, a recommendation of mine that the expedition should be furnished with a set of boring-implements for the purpose of piercing some very large and compact mass of ice, with the expectation of finding it much below the freezing-temperature. The heat of summer, it was suggested, would all be carried off in the water resulting from surface-melting; while the intense cold of a polar winter would penetrate the interior, and thus give rise to a *mean* temperature very far below that of the external climate. The implements (if I remember rightly) *were* furnished, but Lieut. Foster reported that no mass of ice sufficiently large could be met with so free from fissures as not to be

permeated by infiltering water during the summer months; and if any results were obtained, they were not striking or definite enough to be worth recording. That the lower surface of a glacier in contact with the earth is in a constant state of fusion, even in those cold regions, is proved by the phenomena recorded by Dr. Kane of the Mary Minturn River in lat. 78° 54′, and the feeder of the Kane Lake, lat. 78° 18′, which never ceases to flow, summer or winter. Admitting this as a general fact, the sliding of a glacier on its bed is an obvious necessity; and that it should be unaccelerated is no more a matter of wonder or difficulty of conception than the unaccelerated descent of the weight of a clock which is never abandoned to its own impetus, but brought to rest after every momentary descent by the action of the scapement,—or the unaccelerated fall of a body in a resisting medium when the resistance becomes equal to gravity,—or of a weight gradually and uniformly lowered by the hand. Perhaps the more general way of conceiving it would be to regard the whole glacier as a mass propped up against a support anyhow inclined, and prevented from tumbling over sideways by lateral stays. Such a mass would rest in its position, if duly supported either by a base-abutment, or by a heap of its own débris; but if these were slowly abraded, destroyed, or picked away, the whole mass would descend bodily in the exact manner of the withdrawal of support.

On the disruption of a nearly homogeneous elastic solid in a state of strain, I would add a remark which seems to me of some moment, as explanatory of the greater cohesive strength which is well known to be imparted to cements, especially those of a resinous or gummy nature, by the admixture of extraneous matter in fine powder. If in such a solid there be one portion, however small, weaker than the rest, the strain being uniform, a crack will originate in that place. Now a crack, once produced, has a tendency to run—for this plain reason, that at its momentary limit, at the point on which it has just arrived, the divellent force on the molecules there situated is counteracted only by half the cohesive force which acted when there was no crack, viz., the cohesion of the uncracked portion alone. But if a crack anywhere produced be stopped from running by encountering a solid particle of greater cohesive force, fracture will no longer be determined by the accidental deficiency of cohesion at some weak point, but by the average resistance of the whole cementing mass.

It would, I think, be interesting to determine, on the more transparent portions of glacier ice, by the simple and easily applied test of polarized light, whether a definite crystalline structure prevail in its interior, and if so, in what direction the axis lies in relation to the lines of fissure in the crevasses. Nor is there any reason why the idea above thrown out respecting the mutual modification of structure of two masses cemented by regelation, at or near their plane of junction, should not be subjected to a similar test.

XX. "On the Absorption and Radiation of Heat by Gaseous and Liquid Matter."—Fourth Memoir. By JOHN TYNDALL, F.R.S. Received June 18, 1863.

In his former researches on the absorption and radiation of heat by gaseous matter, the author compared different gases and vapours at a common thickness with each other. In the first part of the present communication he determines, in the case of several gases and vapours, the absorption effected by different thicknesses of the same gaseous body. His least thickness was 0·01 of an inch, and his greatest 49·4 inches; thus the thickness varied from 1 to nearly 5000. The apparatus employed for the smaller thicknesses consisted of a hollow cylinder, with its end closed by a plate of rock-salt; into this fitted a second hollow cylinder, with its end closed by a second plate of salt. One cylinder moved within the other as a piston, and by this means the plates of salt could be brought into flat contact with each other, or separated to any required distance. The distance between the plates was measured by means of a vernier. The cylinder was placed horizontal, being suitably connected with the front chamber used in the author's former researches, and the source of heat employed was a copper plate, against which a steady sheet of gas-flame was caused to play.

The absorptions of carbonic oxide, carbonic acid, nitrous oxide, and olefiant gas were determined by this apparatus, and such differences as might be anticipated from former experiments were found. Olefiant gas maintained its great supremacy over the others at all thicknesses. A layer of this gas, 0·01 of an inch thick, effected an absorption of about 1 per cent. of the total radiation. To show

the competence of the apparatus to measure an absorption of this magnitude, it is only necessary to state that the galvanometric deflection corresponding to this absorption was 11 degrees. Were it worth while, it might be shown that the absorption by a plate of the gas not more than $\frac{1}{1000}$th of an inch in thickness is capable of measurement. A layer of olefiant gas 2 inches in thickness intercepted nearly 30 per cent. of the entire radiation. Such a layer, encompassing the earth as a shell, permitting the passage of the solar rays, and preventing the escape of the terrestrial ones, would probably raise the surface of the earth to a stifling temperature. A layer of the gas three-tenths of an inch in thickness intercepts 11·5 per cent. of the radiation. Such a layer, if diffused through 10 feet of air, would be far more attenuated than the aqueous vapour actually existing in the air; still it would effect an absorption greater than that which the author has ascribed to the atmospheric vapour within 10 feet of the earth's surface. In the presence of such facts, arguments drawn from the smallness of quantity of the atmospheric vapours are entirely devoid of weight.

For larger thicknesses of gas and vapour, a tube was employed, which was divided into parts capable of being used separately or together. The mode of proceeding was this:—A brass cylinder 49·4 inches long had its two ends stopped with rock-salt; a source of heat was attached to it exactly as in the author's experiments described in former memoirs. The pile and the compensating cube also occupied their old positions; but instead of determining the absorption effected in a column of gas or vapour equal in length to the whole tube, the tube was now divided into two independent compartments by a third plate of rock-salt. Let us call the compartment furthest from the pile the first chamber, and that nearest to the pile the second chamber. The experiments were commenced with the first chamber very short, and the second chamber long; and the plate of salt was subsequently shifted so as to lengthen the first chamber and shorten the second one, the sum of the lengths of both chambers being preserved constant at 49·4 inches.

The absorption effected in the first chamber acting alone was first determined; then the absorption effected by the second chamber acting alone; and finally, the absorption effected when both chambers were occupied by the gas or vapour was determined. This arrange-

ment enabled the author to examine the influence of the *sifting* which occurred in the first chamber on the absorption effected by the second one. The thermal coloration of the various gases was rendered very manifest by these experiments—the heterogeneity of the obscure calorific flux being demonstrated, and the selective action of the gases on particular constituents of the flux exhibited. A stratum of carbonic oxide 8 inches thick being placed in front of a tube containing 41·4 inches of the same gas, those 8 inches intercept 6·02 per cent. of the whole radiation; the same 8 inches being placed *behind* the column 41·4 inches in length, the absorption effected is almost *nil*. So with carbonic acid: 8 inches in front absorb 6·25 per cent., while behind they absorb a scarcely measurable quantity. Similar remarks apply to the other gases, the reason manifestly being, that when the 8-inch stratum is in front, it intercepts the main portion of the rays which give it its thermal colour, while when it is *behind*, these rays have been in great part withdrawn, and to the remainder the gas is transparent.

From analogous reasoning we conclude that the sum of the absorptions of the two chambers, taken separately, must always be greater than the absorption effected by a single column of gas of a length equal to the sum of the two chambers. This conclusion is illustrated in a striking manner by the results; and it is further found that if the mean of the sums of the absorptions of the two chambers, taken separately, be divided by the absorption of the sum, the quotient is the same for all gases. It is also to be inferred from the foregoing considerations, that the sum of the absorptions must diminish as the two chambers become more unequal in length, and must be a maximum when they are equal.

In these days a special interest attaches to the radiation from any gas through itself, or through any other having the same period of radiation. The author records the results of an elaborate series of experiments on this point. The experimental tube, 49·4 inches long, was divided into two compartments by a partition of rock-salt. The compartment nearest to the pile was filled with the gas which was to act as absorber, while the chamber most distant from the pile contained the gas which was to act as radiator. This latter gas was warmed by the destruction of its own *vis viva* within the chamber. The radiation was what the author has called dynamic radiation.

The lengths of two chambers were varied, the radiating column being lengthened, and the absorbing one shortened at the same time. The experiments were carried out with a considerable number of gases and vapours.

The experiments with the vapours were thus executed. First, the chamber nearest the pile was occupied by vapour of a certain pressure; the other chamber was then occupied by the same vapour at the same pressure. The entrance of the vapour was so slow, and its quantity was so small, that the dynamic radiation due to the destruction of its own *vis viva* was almost insensible. The needle being at zero, dry air was allowed to enter the chamber most distant from the pile; the air became heated, communicated its heat to the vapour, and the latter radiated it against the pile. It is quite evident that not only does this case resemble, but it is of the same mechanical nature as that in which a vibrating tuning-fork is brought into contact with a surface of some extent. The fork, which a moment before was inaudible, becomes at once a copious source of sound; it communicates its motion to a body of sufficient dimensions to transmit it in large quantities to the air. What the sounding-board is to the tuning-fork, the vibrating compound molecule is to the elementary atom. The tuning-fork swinging alone is in the condition of the elementary atom radiating alone, the sound of the one and the heat of the other being insensible; but in association with the particles of acetic or sulphuric ether, the elementary atom is in the condition of the tuning-fork applied to its sound-board, communicating through the molecule motion to the luminiferous ether, as the fork through the board communicates it to the air.

These experiments show the great opacity of a gas to radiations from the same gas. They also show, in a very interesting manner, the influence of attenuation in the case of the vapours. The individual molecules of a vapour may be powerful absorbers and radiators, but in thin strata they may constitute an open sieve, through which a large quantity of radiant heat may pass. In such thin strata, therefore, the vapours, as used in the experiments, were generally found less energetic than the gases, while in thick strata the same vapours showed an energy greatly superior to the same gases.

A few striking results are recorded by the author in illustration of

the influence of a lining within the experimental tube on the radiation. A ring of blackened paper, for example, not more than 1½ inch in width, placed within a polished brass tube, radiated, when dry air was permitted to enter the tube, a quantity of heat sufficient to urge the needle of the galvanometer through an arc of 56°; while, when the ring was removed, the radiation from the whole surface of the tube produced a deflection of only 7°·5.

The author finally examines the diathermancy of the liquids from which the vapours made use of in his experiments were derived; and the result leaves no shadow of doubt upon the mind, that if any vapour be a strong absorber, the liquid of that vapour is also a strong absorber. The phenomenon is one in which the individual molecules are implicated, the molecule carrying its power as a radiant and an absorbent through all its states of aggregation. The order of absorption in liquids and vapours is precisely the same. These facts revive thoughts regarding the connexion between radiation and conduction, to which the author has already given expression. In a future memoir he hopes to throw additional light on this important subject.

XXI. "Account of Observations of Atmospheric Electricity taken at Windsor, Nova Scotia." By Joseph D. Everett, M.A., F.R.S.E., Professor of Mathematics, &c. in King's College, Nova Scotia. Communicated by Professor William Thomson. Received June 18, 1863.

1. The observations here described were taken at my house, which is on the College hill, Windsor, in latitude 44° 58′ 34″ N., and longitude 64° 8′ 30″ E. They were taken at a landing-window looking N.E., whose sill is 27 feet above the ground. There is a very clear view from the window, and no trees, buildings, or other obstacles to screen it from the full effect of atmospheric electricity. The ground slopes away on the N.E., E., S.E., and S., and is nearly level in other directions, rising slightly, however, for the first 20 yards on the N.W. The surrounding country is undulating, with the exception of a stretch of flat alluvial soil which runs past the base of the College hill, and to which the ground slopes away from

my house in the directions above indicated, the fall amounting to from 60 to 70 feet. The flat in question connects the two rivers Avon and St. Croix, which unite just below Windsor, and is scarcely, if at all, above high-water mark, being protected from inundation by dykes. The view from my house is bounded in the distance on all sides by ranges of thickly wooded hills, whose average height is about 500 feet, and nearest distance about three miles.

2. The apparatus used for collecting the electricity of the air was Professor Thomson's water-dropping collector, which consists of an insulated can of copper, having a brass pipe leading from it, through which the water can be discharged by turning a tap. The can stands upon a shelf just inside the window, and the pipe projects through a hole $1\frac{1}{4}$ inch in diameter in a board which is inserted under the lower sash of the window. The pipe extends nearly horizontally outside the window to the distance of 3 feet 6 inches from the sill; and the water, when turned on, flows from the end in a stream so fine, that two or three hours are required to discharge a pailful of water. The end of the pipe is about level with the sill of the window. It is assumed that the effect of the stream of water flowing from the pipe is to reduce the pipe and can to the same electrical potential as the air at the place where the water breaks into drops. The time required to reduce the can to the potential of the air is not more than a minute. In my earlier observations I observed the electricity of the air as soon as I could after turning on the water; but in looking over my observation-book, I found the electricity recorded was uniformly weaker at the first observation than at the second, taken about a minute later. In my later observations I always allowed a full minute.

3. In the commencement of winter the frost interfered with the system of water-dropping. I first tried to obviate the difficulty by leaving the water running; but a little always remained in the pipe after the can was emptied, and by freezing burst the pipe. I then shortened the pipe by removing some joints of it. This prevented the bursting of the pipe, the remaining portion being of stouter material than that removed; and I continued to take observations, with the pipe thus shortened, from Nov. 17th to Dec. 2nd; but as the weather grew colder, the water froze in that portion of the pipe which was inside the window; and after thawing it on two or three

occasions by the heat of a spirit-lamp, I determined to dispense, if possible, with water-dropping, and collect the electricity of the air by a burning match. Accordingly, from Dec. 2nd to March 30th I employed matches such as are used with Professor Thomson's Portable Electrometer (consisting of rolls of blotting-paper prepared with nitrate of lead), the burning match being fixed on the end of the water-dropping pipe. The effect of shortening the pipe is to weaken the electricity collected, in a constant ratio, which I have ascertained, by observations taken alternately with the pipe shortened and restored to its full length, to be about 1 to 3·1. This difference has been allowed for in reducing the observations, as it was very desirable to furnish my tabulated results in a form easily admitting of comparison by inspection. I have, nevertheless, inserted the letter S in the column "Remarks" against all observations taken with pipe shortened.

I have in like manner compared, by alternate observations, the results obtained respectively by water-dropping and burning match, and my observations lead to the inference that the results obtained by both methods are the same. The letters M and W, when they occur in the column "Remarks," denote respectively that the observations against which they stand were taken with burning match and water-dropping. (See Table IV.)

4. For testing and measuring the electricity thus collected I have used the "Station Electrometer," except in a few cases, when I have employed the "Portable Electrometer." Both instruments are inventions of Professor Thomson. The former consists of a Leyden jar, having within it a needle of aluminum suspended by a glass fibre, and connected (by platinum wires dipping in sulphuric acid) with the inner coating of the jar. In the neighbourhood of the needle are two brass plates, also connected with the inner coating of the jar, called the "repelling plates," because their function is to repel the two ends of the needle, causing it to rotate, and thereby twist the glass fibre. A brass cage surrounds both the needle and the repelling plates. It hangs from glass pillars, which insulate it from the jar (and therefore also from needle and repelling plates); and an arm of brass attached to it projects through a hole to the outside of the jar, furnishing the means of connecting the cage with

the object to be tested. This projecting arm is surrounded by a hollow cylinder of pumice soaked in sulphuric acid, for the purpose of drying the air which enters the jar, and thus preserving insulation. The insulation thus obtained is so good that the jar loses, on the average, only 2 or 3 per cent. per diem.

5. The force of repulsion between the needle and the repelling plates depends not on the potential of the jar absolutely, but on the difference between this and the potential of the cage, being for any given position of the needle proportional to the square of the difference of potentials. In taking an observation, the force of repulsion is ascertained by applying torsion to the glass fibre until the near end of the needle is brought into a line between two sights, one of which is on the plate of glass which covers the jar, and the other on the bottom of the cage. The amount of torsion applied is read off, and is assumed to measure the force of repulsion in the sighted position of the needle; hence the square root of the number of degrees of torsion measures the difference between the potential of the inner coating of the jar and the potential of the cage (the latter being the same as that of the body tested) in terms of a unit which is constant for any one electrometer. In accordance with a convention which has been adopted by Professor Thomson, I have always multiplied the degrees of torsion by 10 before extracting the square root. Thus, if E denote the number of degrees of torsion required to bring the needle to the sights when the cage is connected with the earth, and A the number required when the cage is connected with a conductor to be tested, the potential of the inner coating of the jar will be $\sqrt{10\,E}$, and the potential of the conductor tested will be $\sqrt{10\,A}$; hence the potential of the body tested will be $\sqrt{10\,E}-\sqrt{10\,A}$. It is by this last formula that the numbers under the heading "Electricity of Air" in my tabulated results have been calculated. If A is greater than E, the formula still holds, and the negative sign indicates that the electricity of the conductor tested is of opposite kind to that with which the jar is charged. E is called the earth-reading, and A the air-reading.

6. The portable electrometer, which is more convenient for use when the electricity of the air is very strong, also contains a needle and repelling plates, surrounded by a cage which is insulated from them, the whole being enclosed in a Leyden jar; but the needle,

instead of hanging freely by a glass fibre, is attached to the middle of a fine wire of platinum, whose two ends are secured in such a manner as to keep it always tight. The needle and repelling plates are put in connexion with the body to be tested, the cage being connected with the inner coating of the jar, an arrangement which, though opposite to that adopted in the Station Electrometer, requires the same formula to be applied in reducing the readings.

7. The best possible test of the accuracy of the method of reduction above described is to observe the potential of the same conductor with two electrometers, and try whether their indications (thus reduced) differ in a constant ratio when the charges of their jars are varied. I have applied this test, giving charges of different strength, sometimes of the same kind and sometimes of opposite kinds, the conductor tested being a short wire of copper or brass connecting the electrodes of the two instruments. These experiments were performed for the purpose of ascertaining the ratio in question, with the view of reducing all my observations to a common standard; and I have also investigated this ratio by connecting the jars of the two instruments, and taking earth-readings; but this method is less convenient because it involves the admission of undried air into the jars, thus impairing insulation, and rendering accurate observation difficult; whereas in the former method of observing, if performed in favourable weather, the insulation is as good as perfect. As it is important to establish practically the accuracy of the method of reduction pursued, I give *in extenso* all the comparisons that I have made by the methods just described.

8. In using the Station Electrometer, I have given a fresh charge to its Leyden jar about once a week: when charged too highly, it is not sufficiently retentive; when not high enough, the instrument is not sufficiently sensitive. With a charge of fifty units, which is my average working charge, a difference of 1° in the angle of torsion is equivalent to ·1 of a unit of charge, so that if the difference between air-reading and earth-reading is 1°, the strength of atmospheric electricity is ·1. The average strength of atmospheric electricity observed has been from thirty to forty times greater than this, and has in a few cases been 800 times greater. Hence it is obvious that readings to whole degrees are abundantly sufficient; and I have not thought it necessary to aim at greater minuteness, more especially

as the variation generally amounts to two or three degrees every minute.

9. Of late each observation has consisted of an earth-reading, followed by five air-readings, taken at intervals of a minute. During the first three months of observation the number of consecutive readings was irregular, and the interval between them was not always the same. The loss of charge in ten minutes is practically insensible, and is sometimes more than counterbalanced by changes in the zero of torsion produced by the strain upon the fibre during the observation. The only case in which any considerable loss occurs is when the electricity of the air is very strong negative (that of the jar being positive), causing a very high air-reading. The jar holds very well when the reading is 360° (corresponding to 60 units); and I generally give it a charge of about this amount, renewing it when it has fallen a little below 250° (or 50 units).

10. The Station Electrometer stands on the shelf at the distance of about a foot from the can. In taking earth-readings, the electrode is connected with one of the brass foot-screws which support the instrument, the connexion being made by a brass wire. In taking air-readings, the electrode is connected with the can by means of a copper wire. The square roots are generally taken on the spot by a table which serves for all whole degrees from 0° to two revolutions.

11. The Station Electrometer has never been removed from its place except during very cold weather, when the sulphuric acid at the bottom of the jar sometimes became frozen over, so that the needle was prevented from turning; and it was necessary to remove the instrument to a warm room, sometimes to thaw the ice, and sometimes to prevent it from freezing. The ice in question is, I am informed, not frozen water, but a definite compound of sulphuric acid and water. It was most liable to form when the acid had been for a long time unchanged, but I have seen a little of it when the acid had been less than a week in the jar. It never formed unless the temperature of the instrument was as low as about 20°.

12. On the first occasion of its formation, being taken by surprise, and not using proper precautions, I broke the glass fibre which supported the needle: this was on the 3rd of December. A new fibre was put in on the 6th, and the sights were adjusted to suit the new fibre on the 8th, the observations of atmospheric electricity being

taken with the Portable Electrometer in the meantime. On several subsequent occasions the Portable Electrometer was used in consequence of ice having formed in the Station Electrometer. On all other occasions the observations have been taken with the Station Electrometer, except when the electricity was too strong or too variable to be conveniently measured by it. On one occasion, in the first month of the observations (Oct. 22nd), in showery weather, the earth-reading being one revolution and 19°, the negative electricity of the air was so strong that six revolutions were not nearly sufficient to bring the needle to the sight. Electricity of such strength is very common during showers either of rain, hail, or snow, being always positive during snow, generally negative during hail, and generally negative (in some instances alternating with positive) during heavy rain. On such occasions I have generally had recourse to the "Portable," which is less sensitive.

13. On some occasions, when the electricity of the air has been strong, it has been so violent in its fluctuations from second to second, that the needle of the "Station" was quite unmanageable from the extent and rapidity of its vibrations. On such occasions, which are denoted by the word "vibration" in the column "Remarks," I had recourse to the Portable Electrometer, whose needle, in the same state of the atmosphere, frequently exhibited no vibration. Sometimes, however, the needle of the "Portable" has been violently agitated, and on these occasions, which only occurred when the electricity observed was very strong and of opposite kind to that in the jar, I have always found that the jar was discharging. On a few occasions, especially on the 6th and 7th of January, the needle of the "Station," though not trembling as in the cases above alluded to, has been remarkably unsteady, the air-reading changing by several degrees every two or three seconds.

14. In the Table of Comparisons of Electrometers, "Station I." denotes the Station Electrometer with its first fibre; "Station II." denotes the Station Electrometer with its second fibre; and "Portable" denotes the Portable Electrometer. The most probable values of the ratios of their indications, as shown by their comparisons, may be assumed to be—

Portable : Station I. : : 1 : 8·5

Portable : Station II. : : 1 : 3·1

Hence Station I. : Station II. : : 1 : ·365.

If we assume, from the comparisons of long and short pipe, that

Short pipe : Long pipe :: 1 : 3·1,

we have

Station I. with short : Station II. with long :: 1 : 1·13

Portable with short : Station II. with long :: 1 : 9·6.

All the numbers in my tabulated results are given in terms of Station II. with long pipe, and the reductions have been effected by means of a Traverse Table to single degrees on the following assumptions:—

Station I : Station II. :: Distance : Departure for 22°

Portable : Station II. :: Distance : 10 × Departure for 18°

$\left\{\begin{matrix}\text{Station I.}\\ \text{with short}\end{matrix}\right\} : \left\{\begin{matrix}\text{Station II.}\\ \text{with long}\end{matrix}\right\}$ :: Diff. Lat. : Dist. for 29°

$\left\{\begin{matrix}\text{Portable}\\ \text{with short}\end{matrix}\right\} : \left\{\begin{matrix}\text{Station II.}\\ \text{with long}\end{matrix}\right\}$ :: Dist. : 10 × Diff. Lat. for 21°.

These ratios, when stated in numbers, are respectively

1 : ·375, 1 : 3·09, 1 : 1·14, 1 : 9·34 ;

whereas those above, designated as most probable, are

1 : ·365, 1 : 3·1, 1 : 1·13, 1 : 9·6.

15. The cage of the Station Electrometer was taken out and replaced on January 13th and April 10th, an operation which involves a readjustment of the repelling plates, and may thus affect the sensitiveness of the instrument. On the latter occasion a slight change was also made in the position of the upper sight, tending to render the instrument rather less sensitive; but I do not think the effects of these changes have been sufficiently great to be worth taking into account. One at least of the electrometers seems to be affected by some uncertain disturbing cause, which renders it more sensitive on some days than on others. This is well exemplified by the comparisons of January 14th and January 19th, the mean results obtained on the two days respectively differing by 8 per cent. Even on the same day results obtained with different charges do not precisely agree, and the discrepancies are much greater than could arise from errors of reading. They do not follow any easily ascertained law; and I am inclined to attribute them to imperfect elasticity in one or both of the fibres—a cause which may also have produced discrepancies between results on different days, for the zero of torsion has not always been tested when the comparisons were made; and

as the instruments have been left standing with different amounts of torsion on the fibres at different times, the zero of torsion may have changed. This cause would principally affect the Portable Electrometer, whose zero of torsion has always been assumed to be 30°, except on May 2nd, when it was very carefully tested, and found to be 30°·1. The zero of torsion for the Station Electrometer has been so frequently tested, that no sensible correction can be needed for it beyond what has been made. That the Portable Electrometer is affected by some such disturbing cause will be seen by reference to the comparisons on February 11th, where the earth-readings of the "Portable" are 51·0, 51·8, 51·2, the increase from the first reading to the second apparently depending on the increased torsion which was applied in the interim, and the subsequent fall being assisted by the diminution which was made in the torsion between the second reading and the third. The "Portable" also seems to hang a little (see comparisons on November 4th).

16. Besides errors from want of exactness in reducing all the observations, except those taken with Station II., to units of that instrument, and those arising from changes in the Electrometers themselves, it is probable that others have been produced by circumstances affecting the collection of the electricity from the air, the place where the water-stream breaks into drops being subject to small variations, depending on the wind and the head of water in the can. In like manner, when burning matches are used, the place where the smoke breaks away depends upon the wind and the length of the match, the latter being sometimes six and sometimes only three inches at the commencement of an observation. Rain or snow falling on the pipe has also doubtless a disturbing effect, equivalen to a shortening of the pipe; but the effect cannot be great; for I have tested that during a shower of rain, when the water in the can was not allowed to flow out, a strong artificial charge given to the can and pipe, though of the opposite kind to that of the rain, was dissipated with extreme slowness; but when the water was turned on and a fresh charge given, the loss was extremely rapid—say thirty times as fast as before. This observation has been confirmed by observing that even when the electricity of the air during heavy snow or rain was excessively strong, as collected by water-dropping, very feeble indications were obtained if the water was turned off. It is

obvious that the law of distribution of electricity on the surface of a conductor gives the end of the pipe a great advantage over other portions of it as regards power of collecting electricity from the air. Notwithstanding that the tendency of rain or other downfall is to make the electricity appear weaker than it really is, by virtually shortening the pipe, the observed electricity is generally much stronger during heavy showers than at any other time.

17. The errors resulting from these various causes, though not insignificant in themselves, are very small in comparison with the variations which really occur in the electrical potential of the air, as will be seen by the most cursory glance at the tabulated observations.

18. My observations have generally been taken regularly at three stated times in the day, viz., between 8 and 9 A.M., between 2 and $2\frac{1}{2}$ P.M., and between 9 and $9\frac{1}{2}$ P.M.; and in many instances observations have been taken at other hours as well. On Sundays some of the observations have generally been omitted. Either during or immediately before or after each electrical observation, I have also observed barometer, dry- and wet-bulb thermometer, cloud, wind, and state of weather generally. The barometer used is an Aneroid of the usual size, nearly new and in good condition. I have ascertained, by experiment, that it is affected by temperature, the disturbance being in the same direction as for a mercurial barometer. It has also an index error of about ·06, reading too low by this amount; but I have not applied any correction for either cause; and it would scarcely be worth while to do so, as very little connexion appears to exist between the fluctuations of the barometer and those of atmospheric electricity.

19. My two thermometers (dry- and wet-bulb) are mercurial with unusually long degrees. They are placed outside the window of a room in the second story, in which there is never any fire; and are read through the window, from which they are about six inches distant. The window faces the north-west, and the thermometers are well protected from radiation except from the window, while at the same time exposed to a free current of air.

I have carefully tested the thermometers in melting snow, which showed that at 32° the dry-bulb read ·4 too high, and the wet-bulb ·7 too high. I have also tested them in water at various tempera-

tures beside a thermometer which has been tested at Kew Observatory; and as the errors at other points of the scale were found not to differ much from those at 32°, I have applied a uniform correction of —·4 to the readings of the dry-bulb, and of —·7 to those of the wet-bulb. In the tabulated observations I have entered the corrected reading of the dry-bulb thermometer, and the corrected difference between dry and wet.

My observations of cloud are recorded in the usual way, the figure in the column "Amount" denoting the number of tenths of the sky that are covered with cloud. In the column "Kind" the abbreviations ci., cu., st., nim., are used to denote cirrus, cumulus, stratus, nimbus.

The direction of the wind has been inferred from observations of smoke and clouds, or from other obvious sources. The force of the wind has generally been set down by estimation, 1 denoting a light breath, 2 a moderate wind, 3 a rather high wind, 4 a gale, 5 a violent gale. Where the velocity is given in inches per hour in the column "Remarks," it has been observed with a hemispherical cup-anemometer.

20. The entries of electricity are in three columns. The first contains the mean of all the observations which compose the group, these observations being generally taken at intervals of a minute; the second column contains the highest potential observed (that is, the strongest positive or the weakest negative); and the third column contains the lowest potential observed (that is, the strongest negative or weakest positive).

When a greater number of observations have been taken consecutively, they have been broken up into groups; and in grouping I have been careful, as far as practicable, to avoid including positive and negative in the same group; but sometimes, when the electricity was weak and oscillating, I have allowed them to enter the same group; and in these instances I have obtained the mean by dividing the algebraic sum by the number of observations. As regards the number of observations to be combined in one group, my arrangement has been somewhat irregular, depending generally on convenience as regards the pages of my observation-book.

The time entered corresponds nearly to the centre of each group. It is sometimes given in hours and quarters, sometimes in hours and minutes.

21. For the sake of showing the variations of atmospheric electricity from minute to minute, I subjoin all the observations taken on the 10th day of each month, from October to March. With the exception of the evening of February 10th, these are fair samples of ordinary observations. I also subjoin the observations which were taken on the 26th of November, as a specimen of very great and rapid changes in atmospheric electricity. In all these instances the electrical potential of the air is given in units of the electrometer with which it was observed. The readings of dry and wet bulb are also given without correction. (See Table I.)

22. I also subjoin Tables of all the observations (or rather the mean highest and lowest of each group) taken during rain, snow, hail, sleet, and fog. These numbers are merely copied from the complete Table of observations already given, and thus collected for greater convenience of reference. (See Table II.)

It will be observed that the electricity found is almost invariably positive during snow. Out of 25 days on which observations were taken during snow, there were 23 on which positive electricity only was observed, on the remaining 2 days both positive and negative being observed.

Of 28 days on which observations were taken during rain, there were 9 on which positive only was observed, 7 on which negative only, and 12 on which both kinds were observed.

There were only 2 days on which observations were taken during hail, and on both of these both kinds of electricity were observed, but with a great preponderance of negative.

On 2 days observations were taken during sleet. In one instance the electricity found was positive, and in the other it changed from weak positive to weak negative.

On 5 days observations have been taken during fog, and the electricity found was always positive, generally much above the average strength.

Light rain, unless accompanied by mist, has never shown strong electricity; but heavy rain, as also moderate rain with mist, is in the majority of instances marked by very strong electricity.

I have not been able to ascertain, by inspection, any connexion between the direction of the wind during rain and the accompanying electricity.

23. Altogether there are 20 days (out of about 170) on which negative electricity has been observed; but on every one of these days positive electricity was observed also. With the exceptions of December 10th and 19th, negative electricity has only been observed either during downfall (*i. e.* rain, hail, sleet, or snow) or immediately before or after it. In these two exceptional instances the sky was entirely covered with nimbi, and the negative electricity observed was weak. In the latter the observation was taken about 9 P.M.; and the next following observation, taken between 8 and 9 A.M. the next morning, showed positive electricity of unusual strength.

There are only 2 days (February 4th and March 14th) on which the strength of electricity, when there was no downfall or fog, has been as high as 10, and on these two occasions the temperature of the air was below zero. The average strength of electricity, giving equal weight to all observations, and excepting those taken during downfall or fog, has been 4·2 or 4·3,—the averages for the respective months being—Oct. 3·3, Nov. 3·1, Dec. 4·0, Jan. 4·2, Feb. 5·6, March 5·5.

24. With the view of investigating the diurnal range of atmospheric electricity, I have added, for each month, all observations taken during the same hour, and have divided the sums by the numbers of observations. By "observations" I mean, here and during the remainder of this section, the numbers representing the mean electrical potential for each group of readings, as entered in the complete tabular statement already given, except when there are two or more such entries for the same hour, in which case their arithmetical mean has been adopted and reckoned merely as one observation.

Again, dividing the day into three portions—before noon, noon to 6 P.M., and after 6 P.M.,—I have divided the sum of all observations taken in the same portion of the day, for each month, by the number of observations; and I have, in the same manner, found the mean potential "at all hours" by dividing the sum of all observations taken during a month by the number of observations.

It will be seen that for every month of the six the electricity is weaker after 6 P.M. than in either of the previous portions of the day. (See Table III.)

25. The average potential at each hour for the 6 months may be found either (1) by dividing the sum of all observations taken at the

same hour during the 6 months by the number of observations, or (2) by taking the arithmetical means of the monthly means. Whichever method be adopted, the results for some of the hours will, from paucity of observations, be liable to much uncertainty. As a check upon results obtained by these two methods, and to remove errors arising from the greater average strength of electricity in some months than in others, I have (3) divided the sum of observations at each hour for each month by the mean of observations "at all hours" for that month, and, after adding the corresponding sums for the 6 months, have divided by the number of observations. This method of reduction I conceive to be the fairest of the three, as it amounts to multiplying each observation by a factor inversely proportional to the mean electrical potential for the month in which it is taken. It gives the mean potential at each hour, supposing the general mean derived from observations at all hours to be unity. The means obtained in this way are headed "Reduced Means" in the annexed Table of Diurnal Range, those obtained by the other two methods being given in the two preceding columns.

All three methods agree in furnishing the following results:—

1. That between 7 and 8 A.M. the strength of electricity is below the mean.

2. That between 8 and 9 A.M. its strength is above the mean, and takes a very decided maximum.

3. There is apparently a minimum between 10 and 11 A.M.; but observations are few.

4. That from 1 to 7 P.M. the strength is above the mean, with the apparent exception of the hour 4–5.

5. That from 7 P.M. to midnight the strength is below the mean.

The mean here referred to is the mean of all observations, and is probably not the true mean value for the 24 hours.

[Subjoined is a selection from the Tables which accompany the Paper; the complete series is preserved in the Archives of the Royal Society.]

TABLE I.—*Extracts from the Observations.*

October 10, 1862. Station Electrometer with first fibre.

	Electricity.	Remarks.
h. m.		
9 38 A.M.	+ 7·9	
39	+ 8·8	
40	+ 9·4	
41	+10·1	Dry bulb, 56
43	+ 8·6	Wet bulb, 48
44	+12·2	Sky nearly
45	+11·0	covered with
		cirro-cumuli;
10 1 A.M.	+ 6·3	bright, but not
3	+ 7·1	sunshine.
4	+ 9·2	
5	+ 8·7	
2 5 P.M.	+12·5	Dry bulb, 57
7	+13·3	Wet bulb, 50
11	+14·0	
12	+16·1	
13	+17·7	
14	+18·1	
16	+13·4	
17	+15·5	
18	+14·9	
18½	+16·4	
19	+15·5	
20	+15·8	
21	+14·4	
23	+13·2	Dry bulb, 55·5
24	+14·1	Wet bulb, 48·5
6 55 P.M.	+ 4·1	Very cloudy
56	+ 5·2	and dark, not-
57	+ 5·7	withstanding full
59	+ 6·1	moon.
7 1	+ 5·7	Dry bulb, 53
2	+ 6·1	Wet bulb, 48½
8 47 P.M.	+ 7·6	Still very dark.
49	+ 7·7	
52	+ 7·3	
		Dry bulb, 52
10 38 P.M.	+ 4·5	Wet bulb, 48
40	+ 5·5	Sky entirely
41	+ 6·6	clouded, but
42	+ 7·5	light; moon
43	+ 8·1	just visible be-
46	+ 6·0	hind clouds.

Much rain, with wind, during the night.

November 10, 1862. Portable Electrometer.

	Electricity.	Remarks.
h. m.		
7 49 A.M.	−9·7	Bar. 29·43
49½	−6·9	Dry, 40·5
50	−4·7	Wet, 39·0
52	−5·2	Overcast,
52½	−5·9	nimbi,
53	−5·4	cumuli, and
54	−4·7	blue sky.
55	−3·1	W.S.W. 1
56	−2·2	
57	−1·9	
58	−2·0	
59	−1·3	Clearing up.
8 0	−0·5	
1	+0·2	Bar. 29·45
1½	+0·7	Dry, 39·6
2	+1·0	Wet, 37·8
8 42 A.M.	+0·8	Heavy cum.,
43	+0·8	nim., and
		blue sky.
		Bar. 29·47
		Dry, 38·9
		Wet, 36·8
2 14 P.M.	+0·2	Bar. 29·58
15	+0·6	Dry, 37·2
15½	+0·7	Wet, 34·1
3 48	+0·3	
49	+0·3	

Station Electrometer.

3 54	+2·1	Overcast.
55	+2·8	W. 2
55½	+2·7	Bar. 29·61
56	+2·8	Dry, 36·7
		Wet, 33·4
9 3 P.M.	+2·4	Snowing lightly.
		N.W. 1
		Bar. 29·72
		Dry, 33·2
		Wet, 32·3

Table I. (*continued*).

December 10, 1862.
Station Electrometer with second fibre.

	Electricity.	Remarks.
h. m.		
7 57 A.M.	+1·0	
58	−0·2	10 nim., &c.
59	−0·8	Calm.
8 0	−0·7	Bar. 29·96
1	−0·1	Dry, 28·6
		Wet, 27·2
		A little snow has fallen during the night.
2 51 P.M.	+3·0	
52	+3·5	9 nim.
53	+4·1	1 S.W.
54	+4·0	
55	+3·7	
56	+3·4	
57	+3·3	
9 1 P.M.	+2·5	
2	+2·9	
3	+2·9	
4	+2·9	
5	+2·9	
6	+3·0	Bar, 29·90
7	+3·0	Dry, 32·6
8	+2·8	Wet, 29·7
9	+2·5	
10	+2·8	

January 10, 1863.
Station Electrometer with second fibre.

	Electricity.	Remarks.
8 21 A.M.	+6·1	4 cum. and ci.-cum., calm.
22	+7·5	
23	+7·3	
24	+6·7	Bar. 30·45
25	+6·8	Dry, 10·0
26	+7·1	Wet, 9·4
27	+7·1	
28	+7·3	
29	+7·3	
11 50	+2·8	
51	+3·4	
51½	+3·5	
52	+3·7	
52½	+3·9	
53	+4·2	
53½	+3·8	

January 10, 1863.
Station Electrometer with second fibre.

	Electricity.	Remarks.
h. m.		
1 47 P.M.	+2·9	Bar. 30·38
47½	+2·9	Dry, 23·9
48	+3·2	Wet, 21·0
49	+3·1	
50	+2·8	9 str.
51	+3·3	1 W.
4 16 P.M.	+3·8	10 str. and ci.-cum.
15	+3·3	
17	+3·4	Calm.
18	+2·9	
19	+2·6	Bar. 30·36
20	+2·9	Dry, 22·0
		Wet, 20·3
9 15 P.M.	+2·7	
16	+2·9	
17	+2·8	10 cloud.
18	+2·8	1 S.
20	+2·5	Bar. 30·17
21	+2·5	Dry, 27·0
22	+2.1	Wet, 25·3
23	+2·1	

February 10, 1863.
Station Electrometer with second fibre.

	Electricity.	Remarks.
8 51 A.M.	+24·0	Snowing fast.
52	+23·5	Calm.
53	+25·7	Bar. 29·86
54	+30·1	Dry, 27·8
55	+32·1	Wet, 27·4
56	+31·4	
2 9 P.M.	+ 1·7	Not snowing.
10	+ 1·7	10 nim.
11	+ 1·3	(snow-clouds.)
12	+ 1·3	Calm.
13	+ 1·5	Bar. 29·46
14	+ 1·6	Dry, 39·1
15	+ 2·2	Wet, 38·6
16	+ 2·6	

Portable Electrometer.

About 10h. 56m. P.M. the mean of five minutes' observation was +27·0, the highest observed being above +30·0, and the lowest being +24·0.

Table I. (*continued*).

February 10, 1863.
Station Electrometer with second fibre.

Before and after the observation I drew sparks in abundance from the can.

The electricity was extremely rapid in its variations during the observation. The mean was set down from estimation, only the mean, highest, and lowest being recorded.

h. m.	Electricity.	Remarks.
11 15 P.M.	+14·3	
16	+15·0	
17	+16·2	Bar. 29·70
18	+12·8	Dry, 13·8
19	+ 8·3	Wet, 13·2
20	+ 7·6	

Snowing, with violent wind (4 N.), the whole time, from 10h. 56m. to 11h. 20m.

March 10, 1863.
Station Electrometer with second fibre.

h. m.	Electricity.	Remarks.
8 42 A.M.	+6·4	
43	+7·0	7 cum. and nim.
44	+6·7	Calm.
45	+6·3	Bar. 29·98
46	+5·4	Dry, 25·7
47	+5·3	Wet, 24·3
11 44 A.M.	+5·5	8 cum..
45	+5·3	1 W.
46	+5·2	Bar. 30·
47	+5·0	Dry, 27·4
48	+4·4	Wet, 24·2
1 26 P.M.	+4·4	8 cum.
27	+3·9	1 W.
28	+3·8	Bar. 30·
29	+3·7	Dry, 28·9
30	+3·5	Wet, 25·2
9 18 P.M.	+3·9	10 nim. at beginning of observation.
19	+4·1	Sky almost entirely clear at end of observation.
20	+4·3	Calm.
21	+4·5	Bar. 30·10
22	+4·7	Dry, 20·0
23	+4·9	Wet, 19·0
24	+4·6	

November 26, 1862.
Station Electrometer.
All these observations taken with short pipe.

h. m.	Electricity.	Remarks.
7 53 A.M.	−5·8	Raining steadily, but not heavily.
54	−5·4	10 nim.
55	−4·8	1 E.
56	−3·6	Bar. 29·88
57	−3·5	Dry, 38·0
58	−3·0	Wet, 37·4
59	−3·0	
8 32	−20·2	Rain and cloud as above.
33	−22·0	1 S.E.
34	−22·0	Bar. 29·88
35	−22·0	Dry, 38·7
		Wet, 38·1
2 19 P.M.	+12·8	Thick mist, calm.
20	+ 4·0	Bar. 29·64
20½	+ 7·9	Dry, 40·5
		Wet, 40·1
21	+14·2	Raining.
21½	+17·2	
22	+41·7	
23	+68·5	
23½	+75·6	Rain heavier.
24	+70·1	
24½	+70·1	
25	negative out of range, stronger than −28·3	Rain still heavy.

Portable Electrometer.

h. m.	Electricity.	Remarks.
2 29½ P.M.	−3·6	Raining lightly.
30	−3·8	
31	−3·1	
32	−3·3	
33	−3·3	
34	−4·0	
35	−4·0	
36	−4·0	
37	−4·0	
38	−4·1	
39	−4·1	
40	−4·1	Not raining.
41	−4·1	
42	−3·3	
43	−2·6	

Table I. (*continued*).

November 26, 1862.
Portable Electrometer.
All these observations taken with short pipe.

h. m.	Electricity.	Remarks.
2 44	−3·4	
45	+2·6	
45½	+5·0	Rain beginning.
46	+2·1	
47	+1·3	
48	+1·4	
51	+0·6	Rain heavy.
52	−0·1	Rain lighter.
53	−0·8	
54	−0·8	
55	−0·8	Bar. 29·62
56	−0·8	Dry, 41·0
57	−0·6	Wet, 40.
3 3 P.M.	−2·0	Raining.
4	−2·8	Mist clearing up.
5	−2·7	
6	+4·5	Mist gone.
7	+6·9	Very heavy rain.
7½	+6·9	,, ,,
8	+6·1	,, ,,
9	+6·9	Heavy rain.
10	−4·7	Very heavy rain.
11	−8·7	,, ,,
12	+0·8	,, ,,
13	+6·9	,, ,,
14	+5·0	Heavy rain.
15	−0·2	,, ,,
16	−0·9	Very heavy rain.
17	−1·9	,, ,,
18	−2·5	,, ,,
19	−3·5	,, ,, dark.
20	−3·5	,, ,,
21	−2·6	,, ,,
22	−3·1	,, ,,
23	−1·5	Heavy rain.

November 26, 1862.
Portable Electrometer.
All these observations taken with short pipe.

h. m.	Electricity.	Remarks.
3 24	+0·2	Wind N.W. 1
25	−0·9	Heavy rain.
26	−3·1	Very heavy rain.
27	−2·6	,, ,,
28	−3·5	,, ,,
29	−3·5	,, ,,
30	−2·6	Very heavy rain.
		Bar. 29·61
		Dry, 41·4
		Wet, 41·6
		Rather misty.
		Flash of lightning and peal of thunder at 4h. 2m. P.M.
4 4	−5·4	Needle trembling.
5	−3·9	Very heavy rain.
6	−0·4	,, ,,
7	−0·4	Heavy rain.
8	−0·1	Moderate rain.
9	−0·4	

Peal of thunder at 4h. 17m.
·6 of rain fell before 6 P.M.

Station Electrometer.

h. m.	Electricity.	Remarks.
9 47 P.M.	+0·1	10 nim.
48	+0·1	3 S.W.
49	+0·3	Bar. 29·42
52	+0·3	Dry, 50·5
53½	+0·5	Wet, 50·0
54½	+0·5	

TABLE II.—*Observations during Rain.*

		Electricity.			
		Mean.	Highest.	Lowest.	Remarks.
Oct.	h m				
11	8 11 A.M.	+ 5·0	+ 5·5	+ 4·6	Light rain.
	9 56	+ 2·4	+ 3·1	+ 1·6	Light rain.
13	4 21 P.M.	+ 2·3	+ 2·6	+ 2·1	Light rain.
	7 23	+ 0·9	+ 1·0	+ 0·8	Rain.
	8 57	+ 0·4	+ 0·7	+ 0·3	Rain.
14	7 3 A.M.	+ 3·8	......	......	Fine rain.
	9 37	+ 5·3	+ 5·3	+ 5·2	Scotch mist.
17	2 15 P.M.	+ 2·0	+ 2·7	+ 1·3	Fine rain.
	10 15	+ 2·6	+ 3·2	+ 2·1	Light rain.
20	7 30 A.M.	− 1·3	− 0·1	− 2·7	Light rain.
	7 52	+ 6·2	+ 7·4	+ 4·6	Rain and mist.
	3 15 P.M.	− 5·2	+ 0·4	−11·6	Light shower.
22	1 15	+ 0·2	+ 1·0	− 0·1	Rain.
	2 45	+ 3·6	+ 4·1	+ 3·0	Mist and fine rain.
	4 0	+ 3·9	+ 5·8	+ 2·6	Light rain.
23	2 0 P.M.	+ 2·6	+ 2·9	+ 1·9	A few drops.
27	2 25 P.M.	− 6·5	− 3·1	− 9·9	Heavy rain.
	2 32	−13·8	−10·8	−18·5	Heavy rain.
	2 43	−19·8	−13·9	−24·7	Shower at end of observation.
Nov.					[and close.
6	3 58 P.M.	− 1·8	− 1·1	− 2·0	Heavy rain, gloomy,
	5 31	− 1·0	− 0·9	− 1·2	Heavy rain.
	9 8	+ 0·9	+ 1·1	+ 0·6	Rain ceasing.
8	8 0 A.M.	+ 2·5	+ 3·0	+ 1·9	Rain. [ingtoheavy.
	11 47	+ 3·0	+ 3·9	+ 1·6	Light rain, increas-
	11 52	+ 2·5	+ 4·0	+ 0·1	Rain lighter.
	11 57	− 0·7	+ 0·5	− 1·5	Rain lighter.
12	4 8 P.M.	+ 1·9	+ 2·0	+ 1·8	Light rain.
	9 4	+ 1·4	+ 1·7	+ 1·1	Light rain.
13	7 41 A.M.	− 1·6	− 0·2	− 2·3	Light rain.
	7 48	+ 0·8	+ 1·6	+ 0·1	Light rain.
17	2 13 P.M.	−20·4	−13·2	−25·0	Rain.
	2 23	−20·1	−11·4	−29·7	Rain.
	2 30	−38·5	−31·5	−42·5	Rain.
	2 36	−41·5	−34·5	−46·1	Heavy rain.
	8 42	− 5·6	− 4·2	− 7·2	Rain.
19	9 8 P.M.	− 2·2	+ 0·3	− 5·6	Heavy rain.
					[and close).
20	7 37 A.M.	+ 4·1	+ 5·0	+ 3·4	Rain (very dark

TABLE II. (*continued*).

		Electricity.			
		Mean.	Highest.	Lowest.	Remarks.
Nov.	h m				
20	9 5 A.M.	−21·6	−21·4	−24·2	Heavy rain.
	9 22	−32·0	−28·9	−34·3	Heavy rain.
	9 51	−29·2	−27·6	−30·9	Heavy rain.
	9 5 P.M.	+ 3·3	+ 3·5	+ 3·0	Rain; very dark.
22	2 2 P.M.	+ 1·8	+ 1·8	+ 1·8	Rain.
	5 37	− 1·6	− 1·4	− 1·9	Heavy rain.
	8 36	+ 0·5	+ 0·7	+ 0·3	Heavy rain.
26	7 56 A.M.	− 4·8	− 3·4	− 6·6	Rain.
	8 34	−24·7	−23·1	−25·2	Rain.
	2 23 P.M.	+43·7	+86·4	+ 4·6	Heavy rain and thick mist, changing in a minute from +86·4 to strong negative.
	2 34	−34·7	−28·9	−38·3	Light rain and mist.
	2 42	−33·6	−24·3	−38·3	Not raining; mist.
	2 47	+20·3	+24·3	+ 5·6	Rain and mist. [up.
	2 56	−11·9	− 0·9	−26·1	Rain; mist clearing
	3 8	+58·5	+64·4	+42·0	Very heavy rain; mist gone.
	3 10	−43·9	......	......	Very heavy rain.
	3 11	−81·2	......	......	Very heavy rain.
	3 13	+39·5	+64·4	+ 7·5	Very heavy rain.
	3 18	−21·2	− 1·9	−32·7	Very heavy rain.
	3 26	−20·5	+ 1·9	−32·7	Very heavy rain; dark. A flash of lightning and peal of thunder between these observations.
	4 6	−16·5	− 0·9	−50·4	Heavy rain. A peal of thunder at 4h. 17m. P.M., being after the observation. Needle agitated during the early part of observation.
Dec. 16	1 56 P.M.	+ 1·4	+ 1·5	+ 1·2	Mist and fine rain.
Jan. 6	9 15 P.M.	+ 3·3	+ 4·2	+ 2·5	Drizzle.
7	10 30 A.M.	− 3·4	+ 0·8	−11·1	Light rain.
11	9 30 A.M.	−23·9	−11·9	−30·4	Raining.
	2 0 P.M.	−13·6	−10·7	−14·9	Raining.
16	4 15 P.M.	+ 1·2	+ 1·9	− 0·6	Raining.
	11 30	− 7·7	− 6·5	− 8·6	Raining.

TABLE II. (*continued*).

		Electricity.			
		Mean.	Highest.	Lowest.	Remarks.
Jan.	h m				
29	8 15 A.M.	− 6·7	− 4·8	− 9·8	Very heavy rain.
	2 15 P.M.	+ 2·7	+ 4·7	− 0·9	Rain.
	9 15	− 5·7	− 5·4	− 6·0	Light rain.
Feb.					
6	6 0 P.M.	−30·8	−27·5	−36·5	Pouring rain.
	7 15	−32·6	−31·2	−35·2	Pouring rain.
	9 15	−21·8	−18·9	−24·4	Pouring rain.
20	8 40 A.M.	− 3·1	− 1·2	− 3·9	Rain.
27	2 41 P.M.	−20·9	−19·1	−24·3	Rain.
	2 46	− 8·9	− 2·8	−12·0	Rain.
	2 57	+16·4	+21·5	+12·1	Rain.
April					
5	5 11 P.M.	+ 4·3	+ 4·6	+ 3·7	Drizzle, rather misty.
6	8 45 A.M.	−25·7	−21·0	−33·2	Rain, rather misty.

Observations during Snow.

Nov.	h m				
10	9 3 P.M.	+ 0·9	......	......	Snowing lightly.
27	1 0 P.M.	− 0·5	− 0·1	− 0·7	Snowing lightly.
	1 7	+ 0.5	+ 0·6	+ 0·3	Snowing heavily.
	2 14	+ 1·9	+ 2·3	+ 1·7	Snowing lightly.
Dec.					
1	2 16 P.M.	+ 0·3	+ 0·6	+ 0·2	Snowing.
4	8 44 A.M.	+ 2·7	+ 2·8	+ 2·5	Snowing lightly.
	1 53 P.M.	+ 3·8	+ 4·0	+ 3·4	Snowing lightly.
6	8 7 A.M.	+24·8	+26·0	+22·6	Snowing fast.
	9 25	+31·2	+35·8	+30·0	Snowing fast.
9	8 13 A.M.	+ 4·0	+ 5·1	+ 3·4	Snowing.
20	9 5 P.M.	+ 6·9	+ 8·2	+ 6·1	Snowing lightly.
	9 13	+13·1	+21·5	+ 6·8	Snowing lightly.
Jan.					
7	2 30 P.M.	+19·3	+31·2	+14·8	A little snow.
	3 0	+22·1	+34·3	+15·6	A little snow.
12	3 0 P.M.	+ 2·6	+ 3·0	+ 2·1	Snowing lightly.
14	9 15 P.M.	+18·9	+20·1	+18·4	Snowing lightly.

TABLE II. (*continued*).

Electricity.					
		Mean	Highest.	Lowest.	Remarks.
Jan.	h m				
27	8 30 A.M.	+ 7·6	+ 7·8	+ 7·4	Snowing.
	2 15 P.M.	+16·4	+17·3	+15·6	Snowing.
	4 45	+11·0	+12·1	+ 9·9	Snowing.
	9 30	+ 2·7	+ 2·7	+ 2·6	Snowing.
Feb.					
3	9 30 A.M.	+35·0	+49·3	+27·5	Snowing lightly.
6	2 15 P.M.	+26·4	+30·6	+23·2	Snowing.
10	9 0 A.M.	+27·8	+31·4	+23·5	Snowing fast.
	11 0 P.M.	+83·4	+92·7	+74·2	Snowing, with violent N. wind.
	11 15	+38·3	+46·4	+23·5	
12	2 13 P.M.	+10·1	+10·7	+ 9·5	Snowing without intermission.
	4 24	+ 3·6	+ 3·7	+ 3·5	
13	8 56 A.M.	+13·3	+17·6	+ 9·6	Snowing & drifting.
21	8 44 A.M.	+ 8·2	+11·2	+ 5·1	Snowing lightly.
	2 38 P.M.	+35·7	+48·6	+27·6	Snow directly after.
28	9 2 A.M.	+ 4·9	+ 5·7	+ 4·1	Snowing lightly.
	2 26 P.M.	+ 5·2	+ 7·2	+ 3·6	Snowing lightly.
March					
3	2 29 P.M.	+ 8·7	+10·0	+ 7·9	Snowing.
	9 28	+17·5	+30·6	+ 7·6	Snowing lightly.
	9 41	+15·6	+21·3	+12·7	Snowing lightly.
8	5 12 P.M.	+11·1	+13·2	+ 7·2	Snowing.
	8 46	+ 7·5	+ 8·1	+ 6·6	Snowing.
11	9 6 A.M.	+ 8·8	+ 9·7	+ 8·1	Snowing.
	2 33 P.M.	+ 7·0	+ 7·9	+ 6·4	Snowing.
	9 35	+ 5·4	+ 6·3	+ 4·7	Snowing lightly.
31	9 35 P.M.	Positive, out of range, violent agitation of needle, sparks passing spontaneously.			Snowing, with stiff breeze from S.E.
	9 43	+63·3	+85·3	+24·1	
April					
2	2 7 P.M.	+35·0	+42·2	+28·5	Snowing.
	4 9	+26·5	+31·7	+24·8	Snowing.
	4 21	+28·7	+31·7	+26·5	Snowing.
	4 25	+20·1	+20·8	+18·6	Snowing.
	4 30	+29·8	+30·6	+29·0	Snowing.
	4 35	+37·0	+40·3	+34·5	Snowing.
	4 40	+41·3	+43·2	+39·5	Snowing.
	4 46	+41·5	+45·0	+37·0	Snowing.

TABLE II. (*continued*).

		Electricity.			
		Mean.	Highest.	Lowest.	Remarks.
April	h m				
7	8 19 A.M.	−(29·9)	−10·9	−(65·2)	Snowing.
	9 6	+57·5	+65·7	+53·5	Snowing.
	9 32	+22·6	+28·7	+ 6·5	Snowing.
	10 5	+ 1·5	+ 1·6	+ 1·5	Snowing very lightly.
	2 42 P.M.	− 1·1	− 0·9	− 1·3	Snowing lightly.
	2 53	+ 3·1	+ 4·7	+ 1·4	Snowing.
	9 25	+ 8·6	+ 9·3	+ 7·9	Snowing lightly.
		Observations during Hail.			
Nov. 7	2 28 P.M.	+ 4·1	+ 4·5	+ 3·5	Hail.
	4 30	strong negative.			Hail.
	4 56	+ 3·9	+ 4·0	+ 3·7	Hail.
	6 1	−13·3	0·0	−20·4	Hail.
	6 10	− 6·2	− 3·4	− 9·9	Hail.
	6 25	−10·5	− 2·2	−21·0	Hail.
	6 41	−13·3	− 9·9	−19·8	Hail.
	6 50	− 7·4	− 5·6	− 9·6	Hail.
	7 1	−16·1	− 9·9	−19·8	Hail.
	7 8	−28·4	−25·3	−30·0	Hail lighter.
	11 12	+ 1·8	+ 1·9	+ 1·6	Hail light.
March 9	8 47 A.M.	−22·1	−10·2	−33·0	Hail.
April 8	8 47 A.M.	− 3·2	− 1·2	−11·1	Hail immediately [after.
		Observations during Sleet.			
Feb. 6	3 45 P.M.	+ 5·4	+ 7·1	+ 4·3	Sleet.
March 9	1 8 P.M.	+ 1·0	+ 1·1	+ 0·2	Sleet.
	1 11	− 0·5	− 0·4	− 0·6	Sleet.
		Observations during Fog.			
Nov. 28	11 7 P.M.	+ 7·8	+ 7·9	+ 7·7	Fog.
29	8 28 A.M.	+10·6	+13·8	+ 8·1	Dense fog.
	10 22	+ 7·1	+ 7·1	+ 7·1	Dense fog.
Dec. 27	8 45 P.M.	+ 7·1	+ 8·3	+ 6·1	Dense fog.
Jan. 3	9 45 A.M.	+13·1	+15·1	+10·7	Fog.
	12 45 P.M.	+18·4	+19·7	+16·2	Fog.
	2 0	+ 1·2	+ 2·0	+ 0·6	Mist.
	5 0	+12·8	+18·6	+ 5·3	Fog.
	9 15	+20·0	+23·2	+16·7	Very dense fog.
4	9 0 A.M.	+10·7	+11·3	+ 9·5	Fog.
	2 15 P.M.	+18·8	+22·0	+17·0	Fog.
	5 0	+24·8	+26·2	+22·3	Dense fog.

TABLE III.—*Diurnal Range.*

Hour.	October.		November.		December.		January.		February.		March.		All six months.			
	Number of Observations.	Mean Potential.	Number of Observations.	Mean Potential.	Number of Observations.	Mean Potential.	Number of Observations.	Mean Potential.	Number of Observations.	Mean Potential.	Number of Observations.	Mean Potential.	Whole Number of Observations.	Mean of all Observations.	Mean of Means.	Reduced Mean.
6 to 7 A.M.	1	+2·1	...	...	...	...	...	...	...	...	...	...	1	+2·1	+2·1	+ ·63
7 to 8	13	+2·7	12	+3·1	5	+2·5	...	...	...	...	3	+4·1	33	+3·0	+3·1	+ ·85
8 to 9	5	+5·0	7	+3·8	15	+4·9	15	+ 4·7	16	+7·0	13	+5·9	71	+5·4	+5·2	+1·20
9 to 10	11	+3·6	6	+3·8	3	+3·6	9	+ 3·6	9	+4·9	10	+7·8	48	+4·8	+4·6	+1·09
10 to 11	3	+3·5	1	+3·1	1	+2·0	5	+ 3·3	1	+4·1	...	...	11	+3·3	+3·2	+ ·85
11 to 12	...	...	3	+4·2	1	+2·6	1	+ 4·0	2	+5·3	3	+4·9	10	+4·4	+4·2	+1·03
12 to 1 P.M.	...	...	1	+2·5	...	...	3	+ 3·8	1	+6·3	1	+4·8	6	+4·2	+4·4	+ ·92
1 to 2	2	+4·2	5	+2·7	6	+6·4	7	+ 5·2	2	+6·0	3	+4·4	25	+4·9	+4·8	+1·19
2 to 3	13	+4·1	19	+3·1	15	+4·5	19	+ 4·1	19	+5·6	18	+5·2	103	+4·5	+4·4	+1·04
3 to 4	1	+3·1	5	+2·2	1	+3·8	2	+12·4	...	...	...	...	9	+4·7	+5·4	+1·27
4 to 5	6	+3·0	1	+2·8	...	...	3	+ 4·2	2	+5·4	4	+5·3	16	+4·1	+4·1	+ ·94
5 to 6	5	+3·3	...	...	...	...	2	+ 5·9	2	+7·2	1	+4·0	10	+4·7	+5·1	+1·12
6 to 7	2	+4·4	1	+5·6	...	...	4	+ 4·2	...	...	1	+4·7	8	+4·5	+4·7	+1·18
7 to 8	3	+2·3	...	...	...	...	1	+ 2·9	1	+6·0	...	...	5	+3·2	+3·7	+ ·78
8 to 9	6	+3·0	6	+3·0	7	+3·5	...	...	1	+5·1	...	...	20	+3·3	+3·6	+ ·91
9 to 10	12	+2·4	9	+2·1	13	+2·3	20	+ 3·2	22	+5·1	13	+5·4	89	+3·6	+3·4	+ ·79
10 to 11	4	+2·8	2	+2·0	...	...	1	+ 2·5	1	+3·8	4	+4·3	12	+3·2	+3·1	+ ·77
11 to 12	1	+1·9	...	...	1	+4·4	3	+ 4·7	2	+3·6	2	+4·0	9	+3·9	+3·7	+ ·87
Before noon.	33	+3·4	29	+3·5	25	+4·1	30	+ 4·1	28	+6·1	29	+6·3	174	+4·3		
Noon to 6 P.M.	27	+3·7	31	+2·9	22	+5·0	36	+ 4·9	26	+5·8	27	+5·1	169	+4·5		
After 6 P.M.	28	+2·7	18	+2·6	21	+2·8	29	+ 3·4	27	+5·0	20	+5·0	143	+3·6		
At all hours.	88	+3·3	78	+3·1	68	+4·0	95	+ 4·2	81	+5·6	76	+5·5	486	+4·3		

Table IV.—*Comparisons of Match and Water.*

April 2nd, 1863. Snowing. 2 st. 10 nim. Barom. 29·49. Dry, 30·7; Wet, 29·4.

Match.			Water.		
h m			h m		
4 20 P.M.	+31·7		4 24 P.M.	+20·7	
20½	28·7		24½	18·6	
21	28·7	28·72	25	19·9	20·08
21½	28·0		25½	20·8	
22	26·5		26	20·4	
4 29 P.M.	+29·0		4 33½	+38·3	
29½	29·7		34	40·3	
30	29·6	29·80	34½	37·2	37·00
30½	30·6		35	34·7	
31	30·1		35½	34·5	
4 39 P.M.	+40·7		4 45	+42·5	
39½	43·2		45½	45·0	
40	40·5	41·28	46	44·5	41·54
40½	39·5		46½	37·0	
41	42·5		47	38·7	

$$\text{Mean for Match} = \frac{99{\cdot}80}{3} = 33{\cdot}27.$$

$$\text{Mean for Water} = \frac{98{\cdot}62}{3} = 32{\cdot}87.$$

April 28th, 1863. Calm. 2 ci.-cu. Barom. 29·74. Dry, 36·6. Wet, 34·6.

Water.			Match.		
h m			h m		
7 47 A.M.	+7·3		7 55 A.M.	+6·9	
48	7·4		55½	6·8	
49	7·2	7·28	56	6·8	6·82
50	7·2		56½	6·8	
51	7·3		57	6·8	
8 0 A.M.	+6·7		8 6 A.M.	+7·7	
0½	6·6		6½	7·8	
1	6·3	6·44	7	7·9	7·96
1½	6·3		7½	8·1	
2	6·3		8	8·3	
8 11 A.M.	+8·4				
11½	8·4				
12	8·3	8·34			
12½	8·4				
13	8·2				

$$\text{Mean for Match} = \frac{14{\cdot}78}{2} = 7{\cdot}39.$$

$$\text{Mean for Water} = \frac{22{\cdot}06}{3} = 7{\cdot}35.$$

XXII. "On the Brain of a Bushwoman; and on the Brains of two Idiots of European Descent." By JOHN MARSHALL, F.R.S., Surgeon to University College Hospital. Received June 18, 1863.

(Abstract.)

The author having described the mode of preparation and dissection of the three brains, divides his paper into two parts, one relating to the Bushwoman and her brain, and the other to the idiots and their brains.

1. *The Bushwoman's brain.*

The Bushwoman was aged, and about 5 feet high—unusual for her race.

The form of the cranium is a long narrow ovoid—less dolichocephalic, however, than the Negro skull; the face is high-cheeked, and the nose very small and flattened. The frontal sinuses are absent, and the walls of the cranium are thick—so thick that its internal capacity is less than would be expected from its outward form and size, being equal to 35 oz. av. of water, or 60·64 cubic inches, which, for the height of the Bushwoman's body, is decidedly, but not very small.

The actual weight of the preserved encephalon proved to be 21·77 oz. av., which would probably represent, as the author shows, 31·5 oz. for the weight of the recent brain enclosed in its membranes. Allowance being made for the height of the body, this is less by 8·5 oz. than the average weight of the brains of European females of the same age, as estimated from the Tables of Dr. Boyd, published in the Philosophical Transactions for 1861.

The cerebrum proper probably weighed, in its recent state, 27·25 oz., the cerebellum 3·45 oz., and the pons with the medulla oblongata ·8 oz.

The ratio of the cerebrum to the cerebellum was as usual, 7·7 to 1; that of the cerebrum to the body was probably as 1 to 52, and that of the cerebellum to the body as 1 to 418, instead of the usual ratios of 1 to 41, and 1 to 328.

An examination of the general form of the cerebrum shows that it is small, but long—defective in width, and especially in height. Its outlines and surfaces are angular and flat instead of rounded and

full. The frontal region is very narrow, shallow, much excavated below, and compressed laterally near the entrance of the Sylvian fissure. The parietal region is low, but prominent laterally; the occipital region is long, but defective in height; and the temporal region is long, but narrow.

The cerebrum overlaps the cerebellum by ·5 inch, which is as great an absolute overlap as is usual in European brains, but less relatively to the length of the brain, which is very long in the Bushwoman.

The fissures, lobes, and convolutions are then described at length, and compared with those of the ordinary European brain, with those of the Hottentot Venus's brain figured by Gratiolet, and with those of the young Chimpanzee. It is impossible to give in an abstract even an outline of the facts recorded in this part of the paper.

The general result of the inquiry is to show that the fissures are rather more complex than in the brain of the Hottentot Venus, but much less so than in the European. They are rather more complex on the left than on the right side of the brain. They are widely separated from those of the Ape's brain.

The author concludes—1. That all the convolutions proper to man are present, but, as compared with the European brain, are much more simple, and less marked with secondary sulci. The greatest deficiency is in the occipital and orbital convolutions.

2. That the convolutions, taken generally, are rather more complex than those represented in Gratiolet's figure of the Hottentot Venus's brain, which may be partly due to the obliteration of details in the latter during its long period of preservation.

3 & 4. That the resemblance between the Bushwoman's brain and the Hottentot Venus's brain is sufficient to justify the conclusion that the latter was not an idiot, or a defectively developed individual; but both brains, as compared with the European, have an infantile simplicity, characteristic partly of sex, but chiefly of race.

5. That the convolutions being more simple, can be more easily traced and compared on the two sides than usual, but still show abundant evidences of the asymmetry characteristic of man.

6. That there is a greater difference between the Bushwoman's cerebrum and the highest Ape's cerebrum than between it and the European cerebrum; but a less specific difference between it and the

European than between the Chimpanzee and the Orang; and, of course, much less than between the highest and lowest Quadrumanous brains. There is, however, less difference between the Bushwoman and the highest Ape than between the latter and the lowest Quadrumanous animal.

7. The general results, the author thinks, justify the expectation that characteristic differences of degree of cerebral development may hereafter be found in the several leading races of mankind.

The author then proceeds to describe the colour and relative proportions of the grey and white substance, the commissures, ventricles, and ganglionic masses.

The commissural fibres of the corpus callosum are very deficient in the Bushwoman; and the other commissures are also small. The body and anterior cornu of the lateral ventricle are also small; but the posterior cornu and its contained parts are very large.

In the cerebellum, the median parts appear to be somewhat less developed than the hemispheres. Its transverse commissural fibres are more largely developed than the same system of fibres in the European brain; the Chimpanzee standing, in this respect, still lower. The laminæ of the cerebellum are even more numerous than in the European specimen with which the Bushwoman's brain was compared. The cerebellum seems to be more perfectly developed than the cerebrum.

2. *The Idiots' brains.*

Some account is first given of the age, height, and bodily and mental condition of these idiots, one of whom was a woman, aged forty-two years, and the other a boy of twelve. The former was able to walk, though badly, to nurse a doll, and to say a few words; whilst the latter could not walk, nor handle anything, nor articulate a single word.

In the idiot woman, the weight of the recent encephalon was 10 oz. 5 grs., of which the cerebrum weighed 7·6 oz., the cerebellum 1·95 oz., and the pons with the medulla oblongata ·42 oz. In the idiot boy, the recent encephalon weighed 8·5 oz., the cerebrum 5·85 oz., the cerebellum 2·25 oz., and the pons with the medulla oblongata ·4 oz. These are the two smallest idiots' brains the weights of which have been recorded.

Calculations are then entered upon by the author to show the pro-

bable ratios, in the two cases, of the weight of the encephalon, the cerebrum, and the cerebellum to that of the body, and of the relative weight of the cerebrum to the cerebellum. The result of this inquiry is to prove that the entire encephalon was, in each case, about one-fourth of its normal proportional weight. The cerebrum was much more defective than the cerebellum. The idiot boy had relatively more cerebellum, and the idiot woman more cerebrum.

On studying the general form, dimensions, and relative position of the parts of the encephalon, it appears that the entire brain in the idiot woman resembled very closely, at first sight, both in its general mass and in the form of its anterior part, the brain of the Chimpanzee; but a closer comparison shows great differences. The cerebellum especially is of very great size, forming about one-fourth of the entire mass, and, instead of being covered by the cerebrum, has about ·35 inch of it exposed posteriorly.

A detailed description is then given of the fissures, lobes, and convolutions, which are compared with those of the healthy brain and with those of the Chimpanzee. Only the most general conclusions arrived at can here be given.

Of the lobes, the temporal are remarkably large; the parietal seem to be next highly developed; whilst the occipital and frontal are the smallest. According to the author—

1. The idiots' cerebra are not merely diminutive organs, having all the proper parts on a smaller scale, but these parts are fewer in number, less complex, and different in relative proportion and position.

2. Nevertheless all the primary and connecting convolutions proper to the human cerebrum are represented in the idiots, but are very remarkably simplified.

3. The degree to which the convolutions of those parts are developed follows the order observed in the lobes themselves.

4. The convolutions of the idiot woman are more developed than those of the idiot boy, except those of the parietal region.

5. The peculiarities in the idiots' cerebra are due to arrest of development occurring at some period of fœtal existence.

6. Judging from external appearances generally, it might be supposed that this period was about the latter half of the seventh month, and somewhat earlier in the boy than in the woman. But a closer

examination shows that the malformation is not due to a simple arrest occurring so late in fœtal life, but commences much earlier in the parts at the base of the cerebrum, and then influences the evolution of the superficial parts of the hemispheres. The corpora striata appear to be specially affected, and through these the whole hemispheres, but the frontal lobe especially. The interest of this observation in a general physiological view, and especially in regard to the mental condition of idiots, is pointed out.

7. It is not certain whether the idiots' brains had undergone any local evolutional change as the result of education or training.

8. It is certain that they had increased somewhat in size after the general cessation of evolutional changes in their form.

9. The idiots' brains differ—the woman's being more developed on the whole, especially in the temporal regions. Her mental powers were also greater.

10. These idiot brains are somewhat less developed than the two microcephalic cerebra figured by Leuret and Gratiolet.

11. The convolutions in the idiots' brains are more simple than those of the higher Apes, and approach, in this respect, those of still lower Quadrumana. But the points of difference between the idiots' brains and those of the Quadrumana are very decided. They are human cerebra, although so imperfectly developed. They show a general conformity to the cerebral plan of the Primates generally; but already they manifest special human characters.

In regard to the internal structure of the cerebrum and cerebellum, many facts are noticed. The commissural fibres of the corpus callosum are very imperfectly developed. The lateral ventricles and their contents generally are fairly developed; but the corpora striata are very small. The cerebellum is well developed in all its parts in both idiots, but is not perfectly so in either. It is larger in the idiot boy; but the transverse commissural fibres are much less developed in him than in the woman. In accordance with Malacarne's statements, the laminæ are fewer in number in both idiots' brains than in the perfect brain. The cerebellum is not merely larger, but much more developed in its form than the cerebrum, and it certainly continued to be developed to a much later period.

In a postscript-note, dated August 6th, 1863, the author gives an

account of the examination of two idiots' brains preserved in the museum of St. Bartholomew's Hospital, and also of a series of wax models of fœtal brains in the museum of Guy's Hospital.

The result of the additional information so obtained is entirely to confirm the descriptions and explanations given of the structure and mode of formation of the idiot brain.

XXIII. "On Fermat's Theorem of the Polygonal Numbers." By the Right Hon. Sir FREDERICK POLLOCK, F.R.S., Lord Chief Baron. Received June 18, 1863.

[An abstract will be given in a future Number.]

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

I. "On Mauve or Aniline-Purple." By W. H. PERKIN, Esq., F.C.S. Communicated by J. STENHOUSE, LL.D., F.R.S. Received August 19, 1863.

(Abstract.)

The discovery of this colouring matter in 1856, and its introduction as a commercial article, have originated that remarkable series of compounds known as Coal-tar colours, which have now become so numerous, and, in consequence of their adaptability to the arts and manufactures, are of such great and increasing importance. The chemistry of mauve may appear to have been rather neglected, its composition not having been established, although it has formed the subject of several papers by continental chemists. Its chemical nature also has not been generally understood; and it is to this fact that many of the discrepancies between the results of the different experimentalists who have worked on this subject are to be attributed.

On adding a solution of hydrate of potassium to a boiling solution of commercial crystallized mauve, it immediately changes in colour from purple to a blue violet, and, on standing, deposits a crystalline body, which, after being washed with alcohol and then with water, presents itself as a nearly black glistening body, not unlike pulverized specular iron ore.

This substance is a base which I propose to call *Mauveine*: it

dissolves in alcohol, forming a violet solution, which immediately assumes a purple colour on the addition of acids. It is insoluble, or nearly so, in ether and benzole. It is also a very stable body, and decomposes ammoniacal salts readily. When heated strongly it decomposes, yielding a basic oil. Its analysis has led to the formula

$$C_{27}{}^{*} H_{24} N_4.$$

Hydrochlorate of Mauveine.—This salt is prepared by the direct combination of mauveine with hydrochloric acid. From its boiling alcoholic solution it is deposited in small prisms, sometimes arranged in tufts, possessing a brilliant green metallic lustre. It is moderately soluble in alcohol. Carbon, hydrogen, nitrogen, and chlorine determinations have led to the formula

$$C_{27} H_{24} N_4 HCl.$$

I have endeavoured to obtain a second hydrochlorate, but up to the present have not succeeded.

Platinum-Salt.—Mauveine forms a perfectly definite and beautifully crystalline compound with bichloride of platinum, which, if prepared with warm solutions, separates in the form of crystals of considerable dimensions. It possesses the green metallic lustre of the hydrochlorate, but on being dried assumes a more golden colour. It is very sparingly soluble in alcohol. The analysis of this salt has led to the following formula,

$$C_{27} H_{24} N_4, HPt Cl_3.$$

Gold-Salt.—This substance separates as a crystalline precipitate, which, when moist, presents a much less brilliant aspect than the platinum derivative; it is also more soluble in alcohol than that salt, and when recrystallized appears to lose a small quantity of gold. Its analysis has given numbers agreeing with the formula

$$C_{27} H_{24} N_4, H Au Cl_4.$$

Hydrobromate of Mauveine.—This salt is prepared in a similar manner to the hydrochlorate, which it very much resembles, except that it is less soluble. Carbon, hydrogen, and bromine determinations give results agreeing with the formula

$$C_{27} H_{24} N_4, H Br.$$

Hydriodate of Mauveine.—In preparing this salt from the base, it

* C=12.

is necessary to use hydriodic acid which is colourless, otherwise the free iodine will slowly act upon the new product. It crystallizes in prisms, having a green metallic lustre. It is more insoluble than the hydrobromate. Its analysis has led to the formula

$$C_{27}H_{24}N_4, HI.$$

Acetate of Mauveine.—This salt is best obtained by dissolving the base in boiling alcohol and acetic acid. It is a beautiful salt, crystallizing in prisms possessing the green metallic lustre common to most of the salts of mauveine. Combinations of this substance gave numbers agreeing with the formula

$$C_{24}H_{24}N_4, C_2H_4O_2.$$

Carbonate of Mauveine.—The tendency of mauveine to combine with carbonic acid is rather remarkable. If a quantity of its alcoholic solution be thrown up into a tube containing carbonic acid over mercury, the carbonic acid will be quickly absorbed. To prepare the carbonate, it is necessary to pass carbonic acid gas through boiling alcohol containing a quantity of mauveine in suspension; it is then filtered quickly, and carbonic acid passed through the filtrate until cold; on standing, the carbonate will be deposited as prisms having a green metallic lustre. This salt, on being dried, gradually loses carbonic acid. From experiments that have been made with this salt, it would appear to have the composition of an acid carbonate, viz.

$$C_{27}H_{24}N_4, H_2CO_3.$$

In the analysis of salts of mauveine great care has to be taken in drying them thoroughly, as most of them are highly hygroscopic.

I am now engaged with the study of the replaceable hydrogen in mauveine, which I hope will throw some light upon its constitution. From its formula, I believe it to be a tetramine, although up to the present I have not obtained any definite salts with more than one equivalent of acid. Mauveine, when heated with aniline, produces a blue colouring matter, which is now under investigation. A salt of mauveine, when heated alone, also produces a violet or blue compound.

II. "Notes of Researches on the Intimate Structure of the Brain."—Third Series. By J. Lockhart Clarke, F.R.S. Received September 1, 1863.

Structure of the Valve of Vieussens.—The valve of Vieussens consists of four different kinds of layers. The most inferior layer is composed of epithelium, which is continuous with that of the fourth ventricle. The *second* layer is a stratum of longitudinal nerve-fibres, continuous with the white substance of the inferior vermiform process of the cerebellum. The *third* layer consists of a multitude of round, granular nuclei of about the 3500th of an inch in diameter, traversed by fibres derived from the subjacent layer. These nerve-fibres, in my preparations, may be seen in the most unequivocal manner to divide and subdivide into small branches, to which the nuclei are attached as by stalks. The *fourth* or *uppermost* layer is chiefly granular, but is also interspersed with nuclei of the same kind. Along its under side, where it joins the preceding layer, is a row of large multipolar cells, which are connected by their processes with the nuclei of both layers*.

Structure of the Cerebral Convolutions.—In the human brain most of the convolutions, when properly examined, may be seen to consist of no less than *eight* distinct and concentric layers. This laminated structure is most marked at the end of the posterior lobe. On cutting off the rounded point of this lobe in the human brain, by a transverse section, at about the distance of an inch, measured along the side of the longitudinal fissure, I found, at this part, that the stratified appearance was very indistinct in the *upper* and *outer* convolutions, while it was still clearly observable in the *inner* and *lower* convolutions which rest on the cerebellum. It was most conspicuous in the convolution that lies over the bottom of the posterior notch of the cerebellum, and which runs outward and upward, and then winds inward, to reach the surface at the side of the longitudinal fissure.

In vertical sections of convolutions taken from the end of the posterior lobe, where the laminated structure is most marked, the

* This description of the valve of Vieussens formed part of the manuscript of a Paper published in the 'Proceedings of the Royal Society' for June 20, 1861, but was accidentally omitted in the printing.

first or superficial layer is a comparatively thin stratum of fine and closely-packed fibres, intimately connected externally with the pia mater—with which they are very liable to be torn away—while internally they are continuous with fibres radiating from the grey substance.

The second layer is of a pale or whitish colour, and several times the thickness of the one just described. It consists, first, of fibres running parallel with the surface, both around the convolution and longitudinally; secondly, of fibres radiating across them from the grey substance beneath, and crossing each other with different degrees of obliquity; and thirdly, of a small number of scattered nuclei, which are round, oval, fusiform, or angular, and have their longer axes in different directions, but mostly within-outward.

The third layer is of a grey colour, from two to four times as thick as the one above it. It is densely crowded with cells of small size, but of different shapes, in company with nuclei like those of the preceding layer. The cells are more or less pyriform, pyramidal, triangular, round and oval, or fusiform. The pyriform and pyramidal cells—especially in the outer portions of the layer—lie for the most part with their tapering ends toward the surface; and the oval and fusiform cells have generally their longer axes and their processes in a similar direction. In the deeper portions of the layer, however, their position is more irregular, many of them lying with their longer axes parallel with the surface, and in connexion with a multitude of fibres which run in the same direction and in great number *along* the layer. They contain each a comparatively large granular nucleus, which frequently nearly fills the cell. Two, three, four, or more processes spring from the broader ends of the pyramidal cells, and run partly toward the central white substance, and partly in the plane of the layer, to be continuous with nerve-fibres in different directions.

The fourth layer is of a much paler colour. It is crossed, however, at right angles to its plane, by narrow long and vertical groups of small cells and nuclei of the same general appearance as those of the preceding lamina. These groups are separated from each other by bundles of fibres radiating toward the surface from the central white substance, and, together with them, form a beautiful and fan-like structure. This layer is distinguishable from the

one immediately above it by a tolerably sharp outline, but internally it gradually passes into, or blends with, the next one below it, or the fifth lamina.

This fifth layer consists of the same kind of vertical and radiating groups of small cells and nuclei; but the groups are broader, more regular, and, together with the bundles of fibres between them, present a more distinctly fan-like arrangement.

The sixth layer is again paler, and somewhat whitish, but contains some cells and nuclei which have a general resemblance to those of the preceding layers and are arranged only in a faintly radiating manner.

The seventh layer is of a reddish-grey colour, of about the same depth as the preceding, and contains the same kind of cells and nuclei, but in much greater numbers, and mixed with some others of *rather larger* size: only here and there they are gathered into the small elongated groups which give the appearance of radiations. On its under side it gradually blends with the central white layer, into which its cells are scattered for some distance. Both this and the preceding lamina are traversed by nerve-fibres which run *along* their planes, or parallel with the surface of the convolution.

The eighth layer is the central white stem or axis of the convolution. As just stated, it contains, for some distance below its summit, a gradually diminishing number of scattered cells and nuclei, extending from the lower side of the next *upper* layer. The cells are all separate, and disposed with their longer axes at right angles to the curved surface of the convolution, and therefore in the direction of the fibres radiating from the central white stem, with which some, at least, are continuous*.

Course of the Fibres of the Central White Substance through the Convolutions.—From the central white stem bundles of fibres diverge in all directions, in a fan-like manner, toward the surface of the convolutions. As they pass between the long and vertical groups of cells (already mentioned) in the inner grey layers, some of them become continuous with the processes of the cells, and others turn round to become *horizontal*, both in a transverse and longitudinal direction

* The presence of small cells and nuclei in the white substance of the cerebrum and cerebellum, as well as of the spinal cord, was before pointed out by myself. See Phil. Trans. 1859, p. 442 (note).

as regards the convolution, and with different degrees of obliquity. While the *bundles* themselves are by this means reduced in size, their component *fibres* become finer as they approach the surface, in consequence, *apparently*, of branches which they give off, to be connected with cells in their course. When they arrive at the outer grey layer, they are reduced to the finest dimensions, and form a close network, with which the nuclei and cells are in connexion*. *Through* this layer, however, many of them pass in straight lines, and, in company with processes from some of the cells, traverse the next outer and white layer, in which part of them turn round the circumference of the convolution—part run *longitudinally* and with various degrees of obliquity, but parallel with the surface, decussating with the former—others *appear* to form loops by returning to the grey lamina from which they proceed—while the rest continue their vertical course, crossing each other at different angles, and reaching the surface, where they become continuous with the compact and thin stratum of fibres which forms the first layer of the convolution, and is in immediate connexion with the pia mater.

While the bundles of fibres diverge on all sides from the central stem of white substance, another system of fibres, springing from each side of the base of the stem, *curve inward* and form a beautiful arch over its summit, where they decussate each other, and partly constitute the *innermost* pale layer. The fibres of the stem itself are crossed transversely and obliquely by a variable number of others of different diameters; and in longitudinal sections (that is, in sections made in the length of the convolutions) these transverse and oblique fibres are frequently seen to increase in number toward the base of the white substance, where they decussate each other at every possible angle.

Such is the structure of the convolutions at the extremity of the posterior lobe, in which the laminated appearance is most marked. In almost all other convolutions, however, *eight* laminæ, although sometimes indistinct, may be brought into view by means of solution

* This network in the grey substance between the cells and fibres was, I believe, first noticed by myself in my article on the Structure of the Olfactory Bulb, &c., in Siebold and Kölliker's Zeitschrift, 1861, Bd. xi. Heft 1, plate v. fig. 6; and subsequently in my memoir "On the Development of the Spinal Cord," Phil. Trans. 1862, p. 925, *note*.

of potash or soda. Sometimes, as in certain parts of the posterior lobe itself, one can scarcely make out more than seven layers, there being only one broad layer of arciform fibres running *along* the grey layer outside the white central stem. It is an error to call the layers containing these *arciform* fibres (for I shall so name them) the white layers of the convolution, for they are always interspersed with numerous cells, with processes of which they are continuous. In some parts of the brain (on the vertex for instance) the second (from the centre) of the arciform bands of fibres is very broad and strong, and thickly interspersed with large and small cells of different shapes. These arciform fibres of the convolutions run in different planes, transversely, obliquely, and longitudinally. Where a convolution bends round upon itself at a right angle, a section made at the angle contains them in abundance; but here the separate fibres forming the arciform bands are very short, being cut in their passage. The curved arciform fibres, then, establish an infinite number of communications in all directions between different parts of each convolution, between different convolutions, and between these and the central white substance. I have already shown that the more superficial layer of grey substance contains numerous arciform fibres, but finer and less strongly marked.

But the convolutions at the extremity of the posterior lobe differ from the rest, not only in the greater distinctness of their several laminæ, but also in the appearance of some of their cells. On advancing forward, the convolutions contain a great number of cells of a *much larger* kind. In a section, for instance, taken from a convolution at the vertex, and in a vertical line passing through the optic thalamus, the *greater number* of the cells differ but little from those at the extremity of the posterior lobe; but amongst these cells, in the two inner bands of arciform fibres, and the grey layer between them, I found a number of *much larger*, triangular, oval, and pyramidal cells scattered about at variable intervals. The pyramidal cells are very peculiar. Their bases are quadrangular, directed toward the central white substance, and give off four or more processes, which run partly toward the centre to be continuous with fibres radiating from the central stem, and partly parallel with the surface of the convolution, to be continuous with *arciform* fibres. The processes may frequently be seen to subdivide into minute

branches which form part of the intervening network, as I have described on former occasions. The opposite end of each pyramidal cell tapers gradually into a straight process which runs directly towards the surface of the convolution, and may be traced to a surprising distance, giving off minute branches in its course, and becoming lost in the surrounding network. Many of these cells, as well as those of a triangular, oval, and pyriform shape, are as large as those of the anterior grey substance of the spinal cord.

In other convolutions I again found the vesicular structure somewhat modified. In the surface convolution, for instance, at the side of the longitudinal fissure, on a level with the *anterior* extremity of the corpus callosum, all the three inner laminæ are *thronged* with pyramidal, triangular, and oval cells, of considerable size, and in much greater number than in the situation last mentioned. Between these, as usual, is a multitude of the smaller cells.

The cells of the convolutions in man certainly differ in some respects from those of the larger mammalia—from those, for instance, of the ox, sheep, and cat.

In the early fœtal brain of mammalia and man the structure consists of one uninterrupted nucleated network. As development advances, separate layers may be distinguished. In a fœtal sheep $2\frac{1}{2}$ inches long, for instance, I distinguished six layers in a transverse section of the brain, extending from the vertex to the interior of the lateral ventricle. The first, second, and third corresponded to those which I have described in the convolutions of the adult human brain, and still consisted of roundish nuclei connected by a network of fibres. The third of these layers consisted chiefly of a dark and dense stratum of nuclei, exactly similar to that which the caput cornu posterioris of the spinal cord presents at the same period of development. The fourth layer consisted chiefly of elongated and radiating groups of nuclei. The fifth layer was dark, containing nuclei and a dense stratum of transverse fibres. The sixth layer was composed of epithelium, uninterruptedly connected with the network of the preceding layers, and having precisely the same appearance as the epithelium of the cord at the same period of development.

On the Structure of the Cerebellum.—The observations of Gerlach on the minute structure of the cerebellum are in the main confirmed by my own. I must state, however, that the outer grey layer con-

sists of an exceedingly fine *network* of fibres interspersed and connected with nuclei. This network is partly formed by the minute ramifications of the processes which proceed from the large nucleated cells along its inner border, and which completely reach the surface, communicating with each other in their course. In my preparations this arrangement is very distinctly seen.

The facts contained in these notes will be illustrated, as soon as possible, by appropriate drawings.

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END OF THE TWELFTH VOLUME.

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OBITUARY NOTICES OF FELLOWS DECEASED

BETWEEN 30TH NOV. 1860 AND 30TH NOV. 1861.

WILLIAM BALY was born at Lynn, in Norfolk, in 1814, of parents distinguished for their intellectual culture and literary tastes. He was educated in the Lynn Grammar School, and was apprenticed to Mr. Ingle (now Dr. Ingle, of Emsworth), an active and esteemed practitioner of that town.

In 1831 he entered as a pupil of University College, London, and in 1832 at St. Bartholomew's Hospital. At the former he attended the lectures, and at the latter the practice, necessary for the usual diplomas of the College of Surgeons and the Apothecaries' Hall. At both schools alike he distinguished himself by earnest and successful work; and at the end of his pupilage he attached himself to St. Bartholomew's, where he devoted himself zealously to the study of clinical medicine, chiefly under the guidance of Dr. Latham and Dr. Burrows, who even then observed so much of good promise in him that they advised him to prepare to venture on the life of a physician in London. Accordingly in 1834, after obtaining the Diploma of the College of Surgeons and the Licence of the Society of Apothecaries, he went to Paris with a view to the further prosecution of his studies, and, after a winter spent there, to Heidelberg, and thence to Berlin, where he graduated as Doctor of Medicine in 1836.

On his return to England he settled in London, with the view of establishing himself in practice. During the first four years of this period of his career he was occupied with the translation of Müller's 'Handbuch der Physiologie,' a task which he executed with the same scrupulous care as he gave to all his later works; for he not only rendered the German into English of a better style, but he thoroughly studied and worked through the book, repeating many of the observations it described, and examining many of its doctrines. His annotations to that work, if published separately, would have gained for him the reputation of being an expert and original physiologist.

In 1840, through the recommendation of Dr. Latham, Dr. Baly was appointed to visit and report on the state of the Milbank Penitentiary, where dysentery was very prevalent. This led in the next

year to his appointment as Physician to that establishment. The post was of immense advantage to him. It gave him sufficient means of living, while he had very little private practice. The number of sick under his care was often large; their diseases had peculiar interest; and he was brought into contact with Government officers, many of whom could appreciate his trustworthiness and rare ability. For nearly twenty years during which he held this appointment, among all the changes to which the prison was subjected in its discipline and purpose, and in all the varieties of administration under successive Home Secretaries, inspectors, and governors, he was always well esteemed, always trusted, and very generally referred to as a principal medical adviser of Government on questions of the hygiene of prisons. The chief results of his studies at the prison are comprised in his numerous Reports; but more especially in a most elaborate paper on the "Diseases of Prisons," in the twenty-eighth volume of the 'Medico-Chirurgical Transactions,' and in his 'Gulstonian Lectures on Dysentery,' published in 1847. To the same studies also may be referred much of the knowledge displayed in the Report on Cholera, drawn up in conjunction with Dr. Gull, at the desire of the College of Physicians.

In 1841 Dr. Baly became Lecturer on Forensic Medicine at St. Bartholomew's Hospital. He held that Lectureship for fourteen years; and though, as his simultaneous work at the prison showed, he never forgot that the real business of his life was in Practical Medicine, yet he worked assiduously and conscientiously at the duties of this subsidiary appointment.

In 1846 Dr. Baly was admitted a Fellow of the College of Physicians; in 1847 a Fellow of the Royal Society; in 1854 he became Assistant-Physician to St. Bartholomew's Hospital; and in 1855, in conjunction with Dr. Burrows, Lecturer on Medicine there. He was now fairly in the tide of practice, with every prospect of attaining high reputation as a hospital physician, and of multiplying a hundredfold the value of his knowledge by diffusing it among his pupils.

But his social position was to be yet more eminent, and his influence yet wider. In 1859 some one of adequate fitness was required who might at first share with Sir James Clark, and then hold alone, the office of Physician in immediate attendance on the Queen and

the Royal Family. Those who were charged to make a just selection for this high office wisely fulfilled their responsible task in the choice of Dr. Baly, whose pre-eminent qualifications for the duty were unanimously admitted by the whole medical profession. To himself the appointment is said to have been a surprise; but how well he justified the selection was shown by the proofs of confidence which he received from the Queen and the Prince Consort, and the esteem in which he was held by the whole of the royal household.

With the highest honours of his profession within his reach, trusted by his Sovereign, esteemed by his brethren, and held in affection by his many friends,—the pride of his two sisters, who had worked lovingly with him in his laborious days,—his prosperous career was sadly terminated and his valuable life in a moment cut off, on the 28th of January, 1861, by one of those fatal chances to which railway travelling is still but too liable, but in this unhappy case apparently baffling human foresight.

Dr. Baly's early death occasioned a wide-spread feeling of grief. Literally he was mourned from the palace to the prison. With the sense of bereavement was mingled that of disappointed hope; for with his clear and vigorous intellect, his well-balanced and cultivated mind, his devotion to the profession of his choice, his severe sense of duty, his assiduous habits, and his freedom from all controversial tendencies, there was sure promise that, had he been vouchsafed a longer life, he would have yet done much for the advancement of knowledge and the good of mankind.

George Bishop was born August 21, 1785, at Leicester. He was well known in the commercial world as the head of the largest manufacture of British wines in the kingdom. Having a taste for astronomy, he erected an observatory in 1836 at his residence, South Villa, in the Regent's Park. He received the services of such observers, among others, as Mr. Dawes and Mr. Hind, who soon gave his observatory a European name. Without entering into details on double stars, nebulæ, &c., we shall but say that the South Villa observatory claims eleven of the small planets, ten discovered by Mr. Hind and one by Mr. Marth. It is now removed to Twickenham by Mr. George Bishop, Jun.

Mr. Bishop was successively Secretary, Treasurer, and President

of the Astronomical Society; he was elected a Fellow of the Royal Society in 1848. He died June 14, 1861. He will stand high among those of his day (no small number) who have devoted commercial wealth to the increase of knowledge; and will be remembered with all the additional honour due to uprightness and benevolence.

Sir William Cubitt was the son of a miller of Dilham, in Norfolk, and at an early age was apprenticed to a joiner. After some years spent in the exercise of his trade, and in the works required for repairing the mills of the district, he entered the factory of Messrs. Ransome of Ipswich. In their employment Sir William became practically acquainted with the details of Civil Engineering; and about this period of his life he invented the self-winding apparatus of windmills, and the now well-known instrument of prison discipline, the tread-wheel. About 1826 he removed to London and began business on his own account as a civil engineer, and in time attained the foremost rank in his profession. The works executed by Sir William Cubitt on the Norfolk and Lowestoft Navigation, on the Severn Navigation, the South-Eastern and the Great Northern Railways, the landing-stages at Liverpool, the new Rochester Town Bridge, the Berlin Water-works, &c., may be referred to as illustrations of his practical skill; and it is not too much to say that the manner in which the South-Eastern line is carried between Folkestone and Dover is one of the boldest pieces of engineering of which we have examples in England. In 1851 Sir William was charged with the superintendence of the working details of the Great Exhibition building, and for his exertions on that occasion he received the honour of knighthood.

Sir William Cubitt was born in 1785, and died October 13th, 1861. The date of his election into the Royal Society is April 1, 1830.

Dr. William Henry Fitton, who died in London on the 13th of May, 1861, was born in Dublin in January 1780. His family was originally of Cheshire, but had long been settled in Ireland. After passing through his school education, he entered Trinity College, Dublin, and in 1798, through his proficiency in classics, obtained the

Senior Scholarship. He took his Degree of Bachelor of Arts in 1799.

Although originally intended for the Church, Mr. Fitton chose a medical career, and with that view pursued his studies in the University of Edinburgh, where he also attended the Lectures of Professor Jameson on Natural History, and made the acquaintance of various young men, zealous in study, who afterwards attained to distinction in science and literature. After taking his Doctor's degree, he passed some time in London, studying medicine and chemistry, and in 1812 removed with his widowed mother and his three sisters to Northampton, where he began practice as a physician.

After eight years' stay in Northampton, he married a lady who brought him the means of living independently of his profession, and he accordingly withdrew from practice, and took up his abode in London, where he dwelt for the rest of his life, occupied chiefly with his favourite pursuit of geology, and contributing, by his personal qualities and accomplishments, and by his open hospitality, to promote useful and agreeable social intercourse among the scientific men of the metropolis.

From his youth up Fitton was devoted to geology. Before he left Ireland he collected fossils, determined barometrically the height of the chief mountains, and made excursions into Wales to study its mineral structure. His first publication on the science was a memoir "On the Geological Structure of the Vicinity of Dublin," communicated to the Geological Society in 1811, and printed in the first volume of its 'Transactions.' From 1817 to 1841 he contributed to the 'Edinburgh Review' a series of articles which present a just and enlightened commentary on the progress of geological science for the eventful thirty years of which they treat. But the researches on which the reputation of Dr. Fitton as a geologist will most deservedly and most enduringly rest, "are those by which, during twelve active years of his life (from 1824 to 1836), he laboriously developed the true descending order of succession from the Chalk downwards into the Oolitic Formations, as exhibited in the south-east of England and in the adjoining parts of France. Before these labours commenced geologists had only confused notions as to the order of the strata beneath the Chalk, as well as of the imbedded fossil remains of each stratum. It was Fitton who made the Greensand Formations his

own, by clearly defining the position and character of the Upper and the Lower Greensands, as separated by the Gault*." This statement is from a recent notice of Dr. Fitton, containing further interesting information on his life and labours, and proceeding from an authority unquestionable, both as regards personal knowledge of the man, and just appreciation of his work.

Dr. Fitton was elected into the Royal Society in 1815. He belonged also to the Linnean, Astronomical, and Geographical Societies. Of the Geological Society he was one of the most active and distinguished Fellows; he served for some years as Secretary, and eventually attained to the honour of the Presidency; and in 1852, when he had for some years ceased from active labour, the Society "conferred on their veteran associate the highest honour in their gift, the Medal founded by his dear friend Wollaston."

Sir John Forbes was born in December 1787, at Cuttlebrae, in the parish of Ruthven, Banffshire. In 1799 he went to the Academy of Fordyce, where he formed that friendship with Sir James Clark which remained a source of life-long pleasure to both. Obtaining a Bursary (founded by an ancestor of his mother's) to the Grammar School at Aberdeen, he proceeded thither in 1802; and in the following year he entered at Marischal College in Aberdeen, where he remained until 1806†. From Aberdeen he went to Edinburgh, where he obtained a surgical qualification; and in 1807 he entered the medical service of the Navy, in which he served, chiefly in the North Sea and in the West Indies (where he was present at the taking of Guadaloupe by Sir P. H. Durham, to whom he acted not only as flag-surgeon but as secretary), until 1816. Being placed on half-pay at the general reduction which took place at the conclusion of the war, he returned to Edinburgh, where he spent a year and then graduated. On the recommendation of Professor Jameson he settled at Penzance as the successor of Dr. Paris, and there he remained until 1822, giving his attention not merely to

* Address delivered at the Anniversary Meeting of the Geological Society, 1862.

† Among other Professors whose lectures he attended there was Dr. Robert Hamilton, who gained a high reputation by his published works, but who was locally noted for his extraordinary "absence of mind," of which Sir J. Forbes was accustomed to relate some most amusing illustrations.

professional but also to scientific pursuits, especially meteorological and geological investigations. His "Observations on the Climate of Penzance" and his papers on the "Temperature of Mines" are still quoted as of standard value; and two papers on the "Geology of the Land's End" give further evidence of his zeal and sagacity as a student of Nature. It was during the last year of his residence at Penzance that he published his translation of Laennec's great work on 'Auscultation,' which was at that time but little known and still less appreciated in this country, but which impressed Dr. Forbes's mind with a sense of its value that was soon justified by the general voice of the more enlightened part of the profession. In 1822 he removed to Chichester as successor to Sir William Burnett; and there he continued for twenty years, obtaining the principal practice in the town and in the neighbouring district of Sussex; while there too he formed that friendship with Dr. Conolly which led to their subsequent association in two medical works of great importance. The first of these was the 'Cyclopædia of Practical Medicine,' the publication of which was commenced in 1832 and completed in 1835, under the joint editorship of Dr. Tweedie (to whom the original idea of the work is due), Dr. Forbes, and Dr. Conolly. Besides undertaking a large share of the editorial labour, Dr. Forbes furnished to this 'Cyclopædia' several articles of high excellence, which contributed in no small degree to establish its reputation. Previously to its completion he projected the 'British and Foreign Medical Review,' associating Dr. Conolly with himself as editor; the publication of this journal, which commenced in January 1836, was carried on under their joint superintendence for four years, Dr. Forbes performing nearly all the editorial labour; and on Dr. Conolly's removal to Hanwell in 1840, which occasioned his relinquishment of his connexion with the 'Review,' Dr. Forbes became its sole editor, and continued to discharge that duty until 1847. It was chiefly with the object of improving the 'Review' that he removed to London in 1840, giving up a lucrative practice and a high social position at Chichester, under the full consciousness that he could not expect to attain a corresponding *status* in the metropolis. In the next year he was appointed Physician to the Prince Consort and to the Queen's Household, and he continued to hold these appointments until compelled to relinquish them

by the failure of his health in 1859. He was elected into the Royal Society in 1829.

Although the 'Review' never attained a commercial success, yet there cannot be two opinions as to the importance of the benefits it conferred on the medical profession. Previously to its commencement there had been nothing that deserved to be called full and fair criticism in medical journalism; the so-called 'Reviews' being either mere analyses of the books which they professed to criticise, or confined to a general expression of the opinion formed as to their merits or demerits by writers who were too frequently incapacitated by ignorance or prejudice, or by both combined, to pronounce a trustworthy verdict. It was Dr. Forbes's constant object to secure the services of the best-informed and most impartial contributors whom he could succeed in enlisting; and such was the estimation which the 'Review' soon acquired, not only for its truthful appreciation of the works it criticised, but for the original information contained in many of its articles, that he had no difficulty in assembling around him a staff of able and zealous assistants, over whose productions he exercised a judicious editorial supervision, stamping upon them everywhere his own peculiar marks of justice, accuracy, and vigour. It was his constant object to give an account of the progress of every department of medical science, wherever and by whomsoever made; and by this means he largely diffused an acquaintance with the best foreign medical literature among the profession in this country. Constantly seeking to infuse fresh blood into the organism of which he was the life, he was always glad to avail himself of the assistance of young men who could give the requisite evidence of ability and probity, to whom on his part he afforded the benefit of his wise counsel and kindly aid; and it would not be difficult to point to several men now holding positions more or less distinguished, who would gladly testify how much of their subsequent success they owe to their early association with the 'Review' and with its editor. It was very seldom that he himself wrote more than short 'Notices' of books, or paragraphs interpolated in the longer articles of his contributors; but he departed from his usual course in 1846, putting forth (avowedly as his own) a remarkable article entitled "Homœopathy, Allopathy, and Young Physic;" the purpose of which was in the first place to expose the

errors and absurdities of Homœopathy, whilst bringing into prominence the "vis medicatrix naturæ" as the real agent in its reputed cures,—next to point out that the ordinary routine of medical practice, as carried on by a large proportion of the profession, is scarcely less erroneous in principle and even more mischievous in result,—and thirdly, to assert the doctrine that Rational Medicine should be based on the recognition of the curative powers of Nature as the foundation of treatment, and that it should place its chief reliance on those methods which carry out the indications afforded by the "natural history" of each form of disease, that is, the course it would run if uninterfered with by Art. These views, which he subsequently expanded in a small treatise entitled "Nature and Art in the Cure of Disease," were put forth in the first instance with an incautious *brusquerie* which raised a storm of indignation against their author, and damaged the reputation of the 'Review.' But although what was injudicious in form and manner for a time prevented what was really just and true from obtaining a fair hearing, yet much of the effect which the author strove to produce has gradually developed itself; for there can be no doubt that the practice of the better-educated portion of the profession is now essentially based on the principles which he enunciated; and although various influences have cooperated to bring about this reform, yet no small share of its merit must be assigned to the honesty and vigour with which truths were spoken out in 'Young Physic,' which conservative timidity would have continued to keep in reserve.

The advance of years and other circumstances determined Dr. Forbes in 1847 to relinquish the editorship of the 'British and Foreign Medical Review,' and to transfer his property in it to its publisher, who has made it his constant aim to keep up the high tone impressed on it by its originator, and to maintain the position he acquired for it as the "leading medical journal," not only of this country, but of the world.

With the exception of the small treatise just referred to, Dr. Forbes did not make any further additions to professional or scientific literature; but he published, under the name of 'A Physician's Holiday,' an account of a summer excursion in Switzerland, which acquired a popularity that led him to two further ventures in the same line, respectively entitled 'Memorandums made in Ireland,'

and an 'Excursion in the Tyrol.' In 1852 the University of Oxford conferred upon him the Degree of D.C.L., and in 1853 the honour of knighthood was bestowed upon him. At the end of 1854, having been requested by Government to organize and superintend a large hospital at Smyrna for the sick of the Crimean war, he accepted the post with alacrity under the promptings of that earnest desire to make himself useful in his day and generation which had shown itself in his previous undertakings, and actively commenced the necessary arrangements; but with more time for deliberation and consultation with friends he began to question whether his physical powers would be equal to the post, and finally determined to resign it. Not long subsequently he had the first warnings of that failure of nervous power which progressively increased, until in 1859 he found it necessary to withdraw altogether from active life, and to remove to the residence of his only son at Whitchurch near Reading, where he gradually and tranquilly sank, his death occurring on the 13th of November, 1861.

Although Sir John Forbes cannot be ranked among those who have advanced the science of medicine by the discovery of new facts or the promulgation of new principles, he must be regarded as having done most essential service to the cause of progress, on the one hand by his ready recognition and zealous diffusion of every novelty of sterling value, on the other by the determined onslaught which he made upon prevalent errors, and the vigorous earnestness with which he pleaded for generally-neglected truths. In the depth and extent of his knowledge, in his sagacity as a reasoner, in the earnestness of his search for truth, in his fearless courage in proclaiming it, in his single-minded devotion to right and justice, and in the disinterestedness with which he sacrificed all personal considerations to promote the general good, Sir John Forbes combined all the best qualities of a Reformer. When we add to this estimate his ardent love and extensive knowledge of literature, the general liberality of his sentiments, the wide range of his sympathies, the geniality of his disposition, and that active benevolence which ceaselessly urged him to employ every means in his power for the promotion of objects of public philanthropy, and for the individual benefit of those who had acquired a peculiar claim to his regard, we have such a combination of admirable qualities as could not but command for him

the general respect and esteem of his contemporaries and the warm attachment of a large circle of private friends.

The Royal Society has lost a young and promising associate in Mr. HENRY GRAY, who was cut off by an attack of small-pox on the 8th of June, 1861, at the early age of thirty-six.

Mr. Gray was Lecturer on Anatomy at St. George's Hospital, and had been nominated to the office of Assistant Surgeon to the Institution. During the brief career vouchsafed to him, Mr. Gray laboured assiduously and with much success in Anatomy and Physiology. In 1849 he gained the triennial prize of the Royal College of Surgeons for an Essay on the "Anatomy and Physiology of the Nerves of the Human Eye," and soon afterwards he presented a paper to the Royal Society "On the Development of the Optic and Auditory Nerves," which was published in the 'Philosophical Transactions' for 1850. Another contribution, entitled "On the Development of the Ductless Glands of the Chick," appeared in the volume for 1852. He then undertook an important research into the Anatomy and Physiology of the Spleen, in the prosecution of which he was aided by an allotment from the annual grant placed at the disposal of the Royal Society by Parliament for the promotion of science; and his labours were rewarded by the triennial "Astley Cooper Prize" of £300 in 1853. Two papers on more strictly professional subjects appeared in the 'Medico-Chirurgical Transactions.' His last work was a 'Systematic Treatise on Anatomy,' which was published in 1858, and has rapidly gone through two editions. Mr. Gray was, moreover, an accomplished and lucid teacher of anatomy, and much esteemed in private life, so that his early death was very widely lamented by his professional brethren. His election into the Royal Society took place in 1852.

EATON HODGKINSON was the son of a farmer at Anderton, in the parish of Great Budworth, Cheshire, where he was born on the 26th of February, 1789. When but six years old he lost his father; and in compliance with the wish of his uncle, the Rev. Henry Hodgkinson, Rector of Arberfield, Berkshire, he was sent to a classical school, in order to fit him for a university course, with a view to his entering the Church. The youth, however, had little

turn for languages, and it was determined to send him to the private school in Northwich to learn mathematics, to which he had shown a strong inclination. He seems to have profited greatly by the instruction he received there, for in after-life he often expressed his gratitude to his early master, Mr. Shaw, for laying the foundation of his future mathematical acquirements.

In 1811 his mother and family removed to Manchester, where Mr. Hodgkinson assisted his mother in carrying on business, by which she eventually earned a competency. In Manchester he had full scope to follow the bent of his mind for mathematical and physical pursuits. Here also he made the acquaintance of various eminent persons distinguished for their scientific attainments or manufacturing and engineering skill; and, following the example of some other young men of his acquaintance who were desirous of improvement, he became a pupil of Dr. Dalton, then a private teacher of mathematics in Manchester, and read with him the works of Lagrange, Laplace, Euler, and Bernoulli. The friendship thus begun continued uninterruptedly until Dr. Dalton's death.

The erection of the factory of Phillips and Lee first gave occasion to Mr. Hodgkinson's experimental inquiries into the strength of materials used in construction, which, amongst other results, led him to propose a new form of cast-iron girder. He discovered that cast iron resists compression with an energy nearly six times as great as that with which it resists extension; and he accordingly recommended a form of cross section, in which the upper and lower flanges present sectional areas corresponding with the power of resistance to compression and extension respectively; and this form has now been universally adopted. At the works of Messrs. Fairbairn and Lillie, then rising engineers, Mr. Hodgkinson obtained the requisite means and facilities for making his experiments.

In 1840 Mr. Hodgkinson communicated to the Royal Society a memoir, entitled "Experimental Researches on the Strength of Pillars of Cast Iron and other Materials," which was published in the 'Philosophical Transactions' for 1840, and obtained for its author the award of the Royal Medal for the year 1841. The results of further inquiries were given in a later paper, published in the 'Philosophical Transactions' for 1857. He was elected a Fellow in 1841. The formulæ he deduced for calculating the

strength and deflexion of pillars and beams have been accepted with implicit confidence, and now have a place in all engineering text-books.

Mr. Hodgkinson became a Member of the Manchester Philosophical Society in 1826, and from 1822 to 1844 contributed seven papers to its 'Memoirs,' chiefly on the mechanical principles of engineering. He was also an active Member of the British Association for the Advancement of Science, and contributed valuable matter to the 'Transactions' and 'Reports' of that Association.

The combination of experimental skill with mathematical knowledge which characterized Mr. Hodgkinson was turned to account on the occasion of the construction of the Conway and Britannia tubular bridges. He was engaged by Mr. Fairbairn to assist in the experimental inquiry which it was deemed advisable to institute before commencing those great and novel undertakings; and he contributed valuable formulæ to Mr. Stephenson for calculating the true results of the experiments. It is to the results which were then obtained that we owe the application of wrought-iron plain and boxed girders in the art of construction. For a similar reason he was in 1847 appointed on the Royal Commission to inquire into the properties of wrought and cast iron, and their application to railway structures.

In 1847 Mr. Hodgkinson was appointed Professor of the Mechanical Principles of Engineering in University College, London, and delivered several courses of lectures, although in later years delicate health interrupted his labours.

Mr. Hodgkinson married in 1841 Catharine, daughter of the Rev. William Johns, of Manchester, an intimate friend of Dalton. She died childless in little more than a year after her marriage; and, after remaining a widower till within eight years of his death, he married for his second wife (who still survives him) Miss Holditch, daughter of Henry Holditch, Esq., Captain in the Cheshire Militia. He died at Eaglesfield House, near Manchester, on the 18th of June, 1861.

Francis Palgrave, K.H., author of 'The Rise and Progress of the English Commonwealth,' 'The Merchant and Friar,' 'The History of England and Normandy:'—born July 1788; died 6th July, 1861; the only son of Mr. Meyer Cohen; assumed the name of Palgrave

on his marriage, in 1823, to Elizabeth daughter of Mr. Dawson Turner of Great Yarmouth.

The bankruptcy of his father, at the beginning of this century, compelled Palgrave to exchange the dream of foreign travel and the expectation of a life of competence for the necessity of working for his living. But he neither shrank from the duties thus unexpectedly laid upon him, nor neglected that mental cultivation which his father's unstinted care had commenced. In his sixteenth year he entered a lawyer's office, and continued there, on the expiry of his articles, as managing clerk till the year 1822.

A home education gave early maturity to his abilities. When but eight years old he translated the 'Battle of the Frogs and Mice,' attributed to Homer, from Latin into French. Before he had attained his twentieth year he had contributed many articles to the minor periodicals of the day; and not many years later he became a regular contributor to the 'Edinburgh' and 'Quarterly Reviews.' Writing for the press and writing anonymously were alike distasteful to him; but as he devoted the whole of his only certain income, that derived from the lawyer's office, to his father, he was compelled to this means of support. His early promise of talent did not fail of fulfilment. He was endowed with a mind quick to acquire languages,—to grasp the laws of physical science,—to appreciate the beauties of poetry and art. He was also gifted with a bright imagination, a thirst for knowledge, and the power of patient industry. Honesty and simplicity of nature ennobled all he said or did, and true humility made him unwilling to trust his own researches, and ready to receive suggestions from minds however different in stamp from his own.

In 1821 Palgrave submitted to Lord Spencer a scheme for the publication of the national records, which was unanimously approved by the Commission of Records, "many glorious things," according to Mr. Hudson Gurney's friendly report, being said of him by all. This took place in 1822, and from that time till 1838 he was occupied in the publication of the 'Parliamentary Writs,' 'Exchequer Calendars,' and other works of great magnitude and historical importance connected with the Commission. He was also engaged on his own literary undertakings, and in practice as a barrister, chiefly in pedigree cases. Much labour also devolved on him as one

of the Municipal Corporation Commissioners, though he withheld his signature from their report. He was knighted in 1831, as an acknowledgment of his contributions to constitutional and parliamentary history; and was appointed Deputy Keeper of the Public Records in 1838, a post which he held up to his death. Previous to his appointment, the national muniments were scattered over fifty-six different repositories, many of them but little fitted for the safe custody of the public archives. A different system of management, a different scale of charges for searches and copies, prevailed in each. By the exertion of great activity and perseverance he brought these various establishments under one system, and finally united their contents at the Rolls Estate. His 'Annual Reports,' twenty-two in number, afford ample proof of the extent of his official labours. He was elected a Fellow of the Royal Society in 1821.

The dates of Palgrave's writings are as follows:—He brought out in 1831 a short history of English affairs from the acquisition of Britain by the Romans until the Norman Conquest; and in the year following, the 'Rise and Progress of the English Commonwealth.' The 'Merchant and Friar' was his next publication. In 1841, after the labour of several years, he furnished Murray with the first edition of the 'Handbook to Northern Italy.' The first two volumes of the 'History of England and Normandy' appeared in 1851 and 1857. These volumes treat of the Carlovingian empire, the rise of the Capetian dynasty, and the foundation of the Duchy of Normandy. Materials are left that carry the narrative to the time of Henry I. He also contributed, principally between the years 1815–21 and 1840–45, upwards of forty articles to the 'Edinburgh' and 'Quarterly Reviews.'

With the exception of the *handbook*, one purpose, the elucidation of our national history, runs through his works. The 'Commonwealth' represents the national life of England before the Conquest. The character of the people and general aspect of the realm is exhibited by an examination of those legal and social institutions which regulated the daily life of the community, as he felt that the attention of historians had hitherto been too exclusively confined to the political action of the times. The little history of the Anglo-Saxons was designed to supply that biographical portraiture and narrative detail necessarily excluded from a constitutional history.

In the 'Merchant and Friar,' Roger Bacon is employed as the expounder of mediæval philosophy. Intimate acquaintance with the curious arts of the middle ages, astrology and alchemy, with physical science, both ancient and modern,—the archæological incidents disclosed by study of the city archives,—a sensibility to the beauties of architecture and nature, supplied the author with ample means towards a just comparison of the arts and customs of the past and present. The illustration of important constitutional principles, as shown, for instance, in the development of trial by jury and of the parliamentary representation of the English counties, even more than a picture of mediæval society and manners, was his object in this story. Especially did he wish to impress upon his readers that essential truth, that our "constitution is based, not upon liberty, but upon law,"—that Parliament is not only an assembly of the political estates of the realm, but a judicial tribunal, that High Court to which even the poorest in the middle ages could apply for justice. In spite of the "wit and wisdom" contained in the 'Merchant and Friar,' the animated pictures of past times and varied display of knowledge, the outpourings of one who loved study for its own sake, it is but an unknown book.

Further investigation of Anglo-Norman history, and that passion for his subject which springs from long-continued research, led Palgrave to abandon his intention of continuing, in one volume, the 'Commonwealth' to the accession of the Stuarts, and he devoted himself to investigate the times that lay nearest to the Conquest. This was, however, to him no brief undertaking. To the right understanding of the process by which our constitution arose after the Conquest, he felt it necessary to treat fully of the Norman dynasty from its first establishment, and to exhibit the parallel between France under the Capetians, and the German empire after the extinction of the Carlovingian dynasty. But he was not enabled to complete this great project before declining powers impeded his progress.

Palgrave sought throughout his writings to enforce certain leading historical principles. Independent study convinced him, at the commencement of his career, "that the states composing Western Christendom were to be considered as carrying on the succession of the imperial authority of Rome," a doctrine upon which, as he be-

lieved, all real conception of mediæval and modern history depended. The insight which led him to grasp this important theory, the "great key of mediæval history," and trace its influence upon the general system of human affairs, has entitled him, in the language of one of his reviewers, to a place in the very highest rank of historical inquirers.

He held an opinion of his own upon that vexed question of our history, the position assumed by William and his Normans towards conquered England. He convinced himself that the idea, upon which Thierry laid such stress, of a bitter war of race against race being waged against the Anglo-Saxons, was greatly overcharged. To him it seemed that the remarkable fact in English history is the practical union of interests, that the continuity of English national life was never broken by the Normans: hence the vigorous and uninterrupted progress of national power. The lessons, however, from past times which he sought to enforce were not exclusively historical.

Political economy was a study of great interest to him. But he never missed an opportunity of pointing out a source of error which, in his opinion, pervaded the whole school—the "considering the science of political economy as being entirely subject to calculation, wholly a matter of figures; whereas in fact the "wealth of nations," even in the narrowest sense of the term, is quite as much rated by passion and imagination, the imponderable elements which evaporate during the analysis, and leave no residuum in the crucible."

Imbued with reverence and deeply stored with the learning of the past, he shrank from that tendency, perhaps more general thirty years ago than now, to contrast triumphantly the progress of modern science with mediæval credulity. He inclined the rather to regret the wisdom that still lingered than to boast of the knowledge that had come. That appeal to "civilization," so common with French historians, as the highest standard of human perfection, was specially distasteful to him. On the contrary, he maintained that all the elements which are really beneficial in nationality are directly at variance with the French idea of civilization—that with national language, national institutions, and national religion it cannot amalgamate.

Art was a subject to which he gave but casual attention; yet in this his appreciation of what is real, and of true taste, gave him an

insight beyond his time. He early claimed for gothic architecture the place it now holds in popular estimation. Years ago he suggested that principle in design, since enforced by Mr. Ruskin, that in the physical world the curve is the token of life or organized matter, as the straight line indicates death or inorganized matter. His article on the "Fine Arts in Florence" (Quarterly Review, June 1840) is a fine example of his range of mind, and contains passages eloquent with picturesque description, and stored with sound historical and artistic knowledge.

Few living men have equalled Palgrave in the extent of his reading, still fewer have surpassed him in sincere and independent inquiry. His language was vigorous and often pointedly descriptive. He was capable of vivid biographical portraiture, and of tracing acutely the original development and meaning of laws and titles. Still he lacked some qualifications towards a great historian. His habit of mind was rather that of an advocate than a judge, which diverted him from that perfect judgment which characterizes Hallam and Thirlwall. His feeling for the importance of the laws and social institutions that influenced the daily life of the people prevented him in some degree from grasping the history of the nation as a whole. The habit of dictating his writings tempted him to a diffuseness and redundancy of style; and, not unlike Southey, he has left passages in his writings which are fanciful rather than humorous, and hardly worthy of his powers or of history. This estimate of Palgrave's literary position has been mainly adopted from a criticism on his last volume in the Edinburgh Review of April 1859. Notwithstanding failings such as these, the writer assigns to him a place among the highest rank of historical inquirers, and states that the new light which he has thrown upon the ancient institutions of our land, and his share in enabling us to realize the grand picture of mediæval Europe, should secure to him the deep gratitude of every historical student.

He passed, like Scott and Southey, a life of unremitting industry. As his years were continued from occupation to occupation, without the chasm of a single day, so his hours led him on from work to work—the page of history before breakfast, the office in mid-day, his library and books in the evening. Though he could not rest content save in a round of unremitting activity, friendship and home love were still the salt that made life precious

to him. A man so gifted and true-hearted could hardly fail of meeting with worthy love and honourable friendship; and in these blessings he amply shared. The mutual devoted love and noble companionship that was given to him in his wife formed indeed the keystone of his happiness; yet friendship was to him no empty word. Deservedly so, for he could claim the affectionate regard of those that really knew him, above all of Henry Hallam and Sir Robert Inglis, and of Hudson and Anna Gurney. Nor did he escape the notice of distinguished men. While quite young, Byron had remarked of him, that he would be at the tip-top of whatever pursuit he embarked in. Describing an ideal translator of ancient German poetry, Scott mentioned Palgrave's name. Later in life we find Dr. Arnold proposing to put questions to him on our history, as the person who could answer them better than any one else.

An animated talker in society, at home he was not less ready to amuse and to instruct. He was so bright and playful, so exceeding in kindness and indulgence, that he was there as a boy among his children, sharing in their talk, joining in their arguments, or telling stories, allegories of his own invention or scenes from history. He ever glowed with kindness and sympathy; but latterly animation was dimmed by sorrow from which there was no recovery, and by the commencement of that gradual decay of mind and body of which he died. In 1847 he parted with a very dear son for the East; about five years after that, the best part of his life was buried in his wife's grave. The remaining years were chiefly marked by declining powers and the death of friends, and did but speak to the truth of Hallam's sad words, "time can never reinstate us to the position of domestic happiness." The latter portion of his official life was, however, smoothed to him by the most considerate kindness of Sir John Romilly, a comparative stranger till his appointment as Master of the Rolls.

A mind such as Palgrave's, that adhered instinctively to truth for its own sake, however antagonistic to popular fancy, that turned rather towards the oppressed and humble than to the successful, that was naturally averse to party feeling, and distrustful of the idol of the day,—such a mind, so endowed with wisdom not of this world, so many-sided and imaginative, could not expect to be generally appreciated, or to cause any immediate effect, or to reap the fruit of

its labours. Still, even in this life, he anticipated, in great measure, the reward which is given to those that follow after true knowledge, and strive to guide others in the right way.

General Sir CHARLES WILLIAM PASLEY, K.C.B., of the Royal Engineers, was born at Eskdale-Muir, Dumfries, on the 8th of September 1780. In his early years he displayed the impetuosity and high courage which distinguished him in after life, as well as the perseverance, ability, and liberality for which he became no less remarkable.

Having received a solid preliminary education in Scotland, he joined the Royal Military Academy at Woolwich in August 1796, and obtained a commission in the Royal Artillery on the 1st of December, 1797. He was transferred to the Royal Engineers on the 1st of April 1798, and on the 2nd of August 1799 he was gazetted as first Lieutenant in that corps.

Between 1799 and 1807 he served in Minorca, Malta, Naples, and Sicily, and was employed on various important services and confidential missions. He was sent by General Villettes to communicate with Lord Nelson in 1804; and after having been promoted to the rank of second captain on the 1st of March 1805, he served under the Prince of Hesse-Philippsthal in the defence of Gaeta against the French in 1806, and under Sir John Stuart at the battle of Maida (in Calabria) in the same year. The experience of that battle confirmed the strong opinion which he had always maintained—in opposition at that time to many in the British army—that the English generals would beat the French marshals as soon as they got a chance of doing so.

Captain Pasley took part in the siege of Copenhagen under Lord Cathcart in 1807, and joined Major-General Leith at Oviedo in the north of Spain in September 1808. He was employed to reconnoitre the Asturian frontier, and then to communicate with General Blake at Reynosa in November, and he left Soto on the 15th of that month at night as the French entered it. After joining Colonel Robert Crawford's Brigade, he was retained by Sir David Baird as his extra aide-de-camp, in consequence of his general attainments and knowledge of the Spanish language. He soon after joined Sir John Moore's staff in a similar capacity, and was attached to it during the

retreat upon, and at the battle of Corunna. Though a great admirer of Sir John Moore, he was much annoyed at this retreat, and could never afterwards speak of it with patience—his conviction having been that the army ought to have turned round upon its pursuers whilst it was strong, and to have maintained a footing in the Peninsula, instead of waiting to fight at Corunna after it had been seriously weakened by retreat, and then quitting the country. From his intercourse with the Marquis of Romagna and others, he was also convinced that the Spanish troops might under improved arrangements have been made more useful.

Captain Pasley next accompanied the expedition to Walcheren; he was employed in reconnoitring the coasts of Cadsand and Walcheren under the fire of the enemy's batteries; and he was present at the siege of Flushing in 1809. Leading a storming party of 100 men under Colonel Pack, to obtain possession of a French battery on the dyke according to his own proposal, he was first wounded (though not disabled) by a bayonet in the thigh, and then, after reaching the top of the dyke, shot through the body by a French soldier from below, belonging to a fresh party of about sixty whom he challenged to surrender to twenty men. The bullet passed in at one side and out at the other, injuring the spine in its progress, and it was hardly expected at first that he could recover. Portions of bone, sash, and clothes came out of the wound afterwards by degrees, and it rendered him incapable of duty for more than a year*.

In November 1810 Captain Pasley published the first edition of his 'Essay on the Military Policy and Institutions of the British Empire.' This work appeared in a time of great national despondency; and its principal objects were to advocate greater energy and perseverance in prosecuting the war with France, judicious offensive action in the conduct of that war, and especially a more vigorous policy in Spain, and to demonstrate that Great Britain had "sufficient force and a favourable opportunity for destroying the French empire." It attracted great attention, and was highly approved on account of the manly and patriotic spirit which it displayed, though the doctrines of political economy which it contained were disputed. It ran rapidly through four editions, and was favour-

* He took advantage of this opportunity to teach himself German amongst other things.

ably noticed (by Mr. Canning as was supposed) in the 'Quarterly Review' of May 1811, in which it is characterized as one of the most important political works that had ever fallen under the observation of the reviewer. The opinions it expressed were contrasted with the humiliating language then to be found in the pages of the English press, and with the principle of *husbanding* resources which was alike the watchword and the fatal error of the despondents.

Whilst in command of the Plymouth Company of Royal Military Artificers in 1811, Captain Pasley set himself to consider how improvements could best be made in the practice of Military Engineering. He had found on active service the serious disadvantage under which the Royal Engineers laboured, of having no properly educated men at their disposal, and no good system for regulating their operations; and the remainder of his life was chiefly devoted to the supply of these wants. Finding that the ordinary modes of instruction were unsuited to his object, he composed an elaborate treatise intended to enable the noncommissioned officers to teach themselves and their men without the assistance of mathematical masters, on a method similar to that of Dr. Bell and Mr. Lancaster, and to go through their courses of geometry in the same manner as their company drills or their small-arms exercises. The system thus organized was found so successful at Plymouth, that it was introduced on an extended scale into the schools at Chatham in spite of some objections—one critic fearing that the men would become better educated than their officers, and might be consulted by the Generals commanding! His energy and success, backed by the representations of the Duke of Wellington from the Peninsula as to the defective condition of the Engineer Department in the Field, led to the formation of the Establishment for Field Instruction at Chatham, and to his appointment to the office of Director of that establishment, with the rank of Brevet-Major. He was promoted to the rank of Brevet Lieutenant-Colonel in May 1813, and he became a Lieutenant-Colonel in the Royal Engineers in December 1814. Following up his designs, he completed a work on 'Military Instruction' in three volumes, of which the first was published in 1814, and the second and third in 1817. The former contained the course of practical geometry before referred to; the two latter a complete treatise on elementary fortification, including the principles of

the science, and rules for construction, many of which apply to civil as well as to military works.

Finding, in 1817, that his men had been "most grossly ill-treated by the Army Bread Contractor," he was led to inquire into the system under which the army was supplied with provisions; and in 1825 he printed and circulated, but abstained from publishing, a volume containing the result of his investigations. The exposure which he thus afforded of abuses that were prejudicial to the soldier, and the improvements that he suggested and was partly the means of introducing, were in themselves services of great value. In 1818, he published a volume of "Standing Orders," containing a perfect code of military rules for the duties of all ranks in the army.

Colonel Pasley organized, during his residence at Chatham*, improved systems of telegraphing, sapping, mining, pontooning, and exploding gunpowder on land and in water, and laid down rules which, being founded on careful experiment, will always endure, besides preparing pamphlets and courses of instruction on these and other subjects. The volume which contained his 'Course of Practical Architecture' was especially valuable. His work on the 'Practical Operations of a Siege,' of which the first part was published in 1829, and the second in 1832, is still a text-book, and the best that has been written in any language on that subject. Every operation in it was treated as a separate study; and it exposed various mistakes into which the French and German authors had fallen. It was translated into French, and published in Paris in 1847.

Early in 1831 Colonel Pasley prepared a pamphlet, and in May 1834 he completed a volume of 320 pages, entitled 'Observations on the Expediency and Practicability of simplifying and improving the Measures, Weights, and Money used in this country, without materially altering the present standards.' He strongly advocated the adoption of the decimal principle of division in all its simplicity for our coinage, as well as for our weights and measures, and opposed with equal ardour the introduction of the French units into this country.

He sent to the press in May 1836 the first sheets of a work containing 'Observations on Limes, Calcareous Cements, Mortars,

* In addition to these various occupations, he employed privates of Sappers to teach him the native Welsh and Irish languages.

Stuccos, and Concrete, and on Puzzolannas, natural and artificial,' of which the first edition was published in September 1838. It contained considerable discoveries, the results of experiments at Chatham, and led at once to the manufacture in large quantities of artificial cements, under the different names of "Portland Cement," "Patent Lithic Cement," and "Blue Lias Cement."

In connexion with experiments on the explosion of gunpowder under water, Colonel Pasley was led to undertake, and successfully to carry out, the removal of two sunken vessels from the bed of the Thames near Gravesend, in the year 1838. He received for this service the thanks of the municipal authorities, and was presented with the freedom of the City of London in a gold box. Emboldened by the success of these operations, he proceeded to execute the more formidable task of clearing away the wreck of the 'Royal George' from the anchorage at Spithead, and that of the 'Edgar' from St. Helen's. The value of the materials recovered from these vessels was more than equal to the expense incurred in their removal. Portions of six successive summers, from 1839 to 1844 inclusive, were devoted by him to this work; but he never asked for nor received from the Admiralty any remuneration for the important services that he rendered in this manner to the navy and the nation.

Colonel Pasley remained at Chatham till the end of the year 1841, when he was appointed, at the age of 61, to the office of Inspector-General of Railways. During the twenty-nine years and a half that he was at the head of the Royal Engineer Establishment, there was hardly any subject connected with his profession as a military man and an engineer—of instruction, construction, or destruction—that did not benefit by his attention. His presence there was of the greatest advantage to his country as well as to his corps. The corps of Royal Engineers owes, in fact, its existence in its present condition, as well as its high state of efficiency, to his energy, his example, and his exertions; and the success of the British army in many a field has been due in no small degree to the system of instruction at which he laboured so devotedly, and which he rendered so perfect. As the latest example of the advantage of that system we may refer to the recent war in New Zealand, which was brought to a close mainly through the employment of Pasley's methods, by officers (one of them his own son) who had been trained by him at Chat-

ham. The easy and bloodless capture of the native pahs, which resulted from a systematic employment of the spade, proved at once to their defenders the hopelessness of further resistance.

He became a Brevet-Colonel in 1830, a Colonel of Engineers in 1831, and a Major-General in the Army in 1841. He received the honorary distinction of D.C.L. at Oxford in 1844; and in 1846, on relinquishing the appointment of Inspector-General of Railways, he was made a K.C.B. for general services. He held the appointment of Public Examiner at the East India Company's Military Seminary at Addiscombe for sixteen years, up to the year 1855, and took an active part in its management, contributing materially to the high standard which it reached and at which it was maintained. He was elected a Fellow of the Royal Society as far back as 1816; he was also of old standing in the Astronomical, the Geological, the Geographical, the Statistical, and other societies; and he lost no opportunity of contributing to the advancement of practical science. He was also a liberal subscriber to a great number of charitable institutions.

He held no public office after 1855, but occupied himself chiefly in re-editing his works, superintending the construction of pontoon equipages, and in other matters connected with his profession, as well as in advocating the introduction of decimal coinage, devoting a large proportion of his time to the benefit or advancement of his friends and relations. He was promoted to the rank of Lieutenant-General on the 11th of November 1851, and to that of General on the 20th of September 1860.

He was twice married. His first wife died of consumption in a few months, his second died in 1848. Of six children, three survive him. He was well and hearty up to within a week of his death; but his long life of labour was brought to a close at his residence at 12 Norfolk Crescent, Hyde Park, from congestion of the lungs, on the 19th of April 1861.

John T. Quekett was born at Langport, Somersetshire, in the year 1815; he was the fourth son of the late Mr. Quekett, head master of the Langport Grammar School, and received his early education in that establishment. Being intended for the medical profession, he was sent to London and apprenticed to his brother,

the late Edwin Quekett, lecturer on botany at the London Hospital, at which institution he pursued his medical studies. After passing the usual examinations, he, in June 1840, obtained by competition the appointment of Student of Human and Comparative Anatomy in the Museum of the College of Surgeons, and at the expiration of the term of his appointment became Assistant-Conservator. While holding these appointments, Mr. Quekett was enabled freely to follow his strong inclination for microscopical research, which had very decidedly shown itself in early youth; and after having formed a most elaborate and valuable collection of specimens of the tissues of plants and animals, including numerous fine injections of the vessels, in preparing which he was remarkably skilful and successful, he was in 1844 appointed by the Council of the College to deliver annually a Course of Demonstrations, with a view to the exhibition and connected description of the collection, and the explanation of the method and resources of microscopical study. This collection, numbering 2500 preparations, was purchased by the College. On the retirement of Professor Owen in 1856, Mr. Quekett was appointed Conservator of the Hunterian Museum, and also Professor of Histology, which appointments he held till his death, which took place at Pangbourne, in Berkshire, on the 20th of August, 1861, at the early age of forty-six.

Mr. Quekett was the author of an elaborate treatise 'On the Use of the Microscope,' published in 1848, and speedily again in a second edition. He also prepared the 'Descriptive and Illustrated Catalogue of the Histological Series contained in the Museum of the Royal College of Surgeons of England,' the first volume of which appeared in 1850, and the second in 1855. In 1852 he published a volume of 'Lectures on Histology,' which was followed by a second in 1854. Besides these separate works, Mr. Quekett contributed numerous papers to the 'Transactions of the Microscopical Society,' of which he was one of the founders, and, after labouring zealously in its service for nineteen years as Honorary Secretary, was elected President in 1860. He was a fellow of the Linnean Society, and was elected into the Royal Society in 1860.

Although best known for his microscopical pursuits, Professor Quekett had devoted himself very successfully to the observation of facts throughout the whole field of natural history. In original

research the character of his mind led him less towards speculation than to the determination of facts; and accordingly he has left behind him a rich store of trustworthy materials, the result of his acute and careful observation and faithful record. His knowledge on all subjects of microscopic investigation was extensive and accurate, and he was ever ready to give the benefit of it to others, and especially to his numerous medical brethren, who continually sought his aid on questions determinable by means of the microscope.

Professor Quekett left a widow and four children, to whom the Council of the College of Surgeons, in consideration of his merits and services, have kindly and considerately granted a liberal pension.

Friedrich Tiedemann, Foreign Member of the Royal Society, was born at Cassel, on the 23rd of August 1781. In his father, Dietrich Tiedemann, a Teacher in the Caroline College in Cassel, and afterwards Professor of Philosophy in the University of Marburg, he was not only blessed with an affectionate and watchful parent, but enjoyed the advantage of an accomplished and painstaking preceptor,—to whose private tuition, indeed, much more than to the teaching of the school and gymnasium, he was indebted for his early educational training, and for a thorough grounding in classical studies.

While yet a boy at the gymnasium of Marburg, to which town his father and family had removed, the young Tiedemann showed a decided turn for those pursuits in which he was destined in after life to become so eminent. He delighted in dissecting and preparing such animals as he could procure, and was thus naturally led to the pursuit of Zoology and Medicine. In 1798 he accordingly entered the University of Marburg, where he remained until 1802, diligently studying medicine and the auxiliary sciences. The progress he made was, however, owing more to his own ardent love of the work he had undertaken than to the professorial teaching of the University, which, one or two chairs excepted, appears to have been at that time conducted in an irregular and slovenly way. For better means of studying practical medicine he therefore went to Bamberg, where he made the acquaintance of Döllinger—and thence to Würzburg; and having in the mean time returned to Marburg, he took his Doctor's degree in that University in 1804.

Tiedemann's first essay as a teacher of anatomy was made in 1803.

In the winter of that year he delivered the lectures and superintended the practical studies in the anatomical school of Marburg, as substitute for Professor Brühl, who was incapacitated by ill health; and next year, being invited by the Professor, and prompted by his own inclination, he established himself as "Privatdocent" in the University, and gave lectures on Physiology, Comparative Osteology, and on Gall's Craniology, which then attracted much attention. Feeling, however, the need of further preparation, he went again to Würzburg to work at dissection under Hesselbach, and afterwards to Paris, where he studied hard in the great museum of the Garden of Plants, and attended the lectures of Cuvier, Geoffroy St.-Hilaire, Lamarck, Duméril, and Haüy.

On his way to Paris, Tiedemann had made the acquaintance of Soemmering at Frankfort, and gained his favourable opinion; and now, through the recommendation of that great anatomist, who had in the mean time removed to Munich, he was offered the Professorship of Zoology and of Human and Comparative Anatomy in the University of Landshut. This offer, flattering as it was to so young a man, he naturally accepted, and to Landshut he accordingly went in 1805. There he found a new and handsome anatomical theatre, but no collection of preparations or other appliances for teaching. During the first few years of his incumbency therefore he had to labour hard to supply these wants, besides discharging his professorial duties, even to the temporary injury of his health.

The war between France and Austria having broken out afresh in the spring of 1809, Landshut and its neighbourhood became the scene of active military operations, and Tiedemann, in addition to his regular duties, took charge of one of the temporary hospitals established in the place. But in spite of the distractions of these troubled times he steadily pursued his scientific work. In that same year appeared the first volume of his 'Zoologie,' and in 1810 and 1814 the two parts of the second volume. The author's object in this work was to combine zoology with comparative anatomy, and to found the classification of animals upon their organization. On the part which comprehends Birds he bestowed especial pains, and with a view to the preparation of it made numerous dissections, measurements, and observations of various kinds; so that it is still valued as a rich store of information on the class to which it refers.

Two of his minor works, the 'Anatomie des Fischherzens' and the 'Anatomie und Naturgeschichte des Drachens,' having in the mean time appeared, Tiedemann in 1811 made a journey to the shores of the Adriatic to study the organization of the Echinoderms, a subject which had been proposed for a prize-question by the French Institute. The issue of his labours was his great work on the Holothuria, Asterias, and Echinus, which gained the prize, established his reputation as a Zootomist, and brought him a nomination as Corresponding Member from the Academies of Paris, Berlin, and Munich.

The study of the brain—in its structure and development, its differences among animals, its characters in different races of mankind, its defects and deformities—was a subject on which Tiedemann bestowed much labour, both mental and manual. In 1813 he published the 'Anatomie der Kopflosen Missgeburten,' and in 1816 his well-known description of the anatomy and development of the Fœtal Brain. In this work he described the successive stages of development of the human brain, and showed the correspondence of these transitory conditions with its permanent conditions in animals lower in the scale; and notwithstanding the great advances of this as well as other departments of embryology in later times, Tiedemann's work is still held in high estimation.

These researches on the brain, so happily begun, were continued through many years of Tiedemann's life. The breaking up of the Grand-Ducal Menagerie at Carlsruhe afforded him the opportunity of dissecting several rare animals, and of publishing in 1821 the 'Icones Cerebri Simiarum.' In 1825 he gave, in two memoirs published in the 'Zeitschrift für die Physiologie,' a comparison of the brains of the Orang Outang and Dolphin with that of Man; and he has left behind him a large collection of unpublished figures of the brains of animals, which, in the hands of his distinguished son-in-law Prof. Bischoff of Munich, may yet prove serviceable to science.

The comparison of the brain of the Negro with that of the European, to which he next directed his attention, became for Tiedemann a subject of keen interest. After collecting all the materials to which he could find access on the Continent, he made a visit to England in 1835, mainly for the purpose of making examinations and measurements of the brains and crania of different races to be found in

British collections. The fruits of these labours he presented to the Royal Society, of which he had a few years before been elected a Foreign Member, in a memoir entitled "On the Brain of the Negro, compared with that of the European and the Orang Outang," which was published in the 'Philosophical Transactions' for 1836, and appeared afterwards in German as a separate work in 1837. From his extensive researches, Tiedemann arrived at the conclusion that, whilst in the majority of cases the Negro's brain is undoubtedly less than that of the European, there are nevertheless individuals of the negro race in whom the brain is as large as in the Caucasian; and coupling this result with the fact, shown as he conceived by careful historical and literary inquiries, that there is no province of intellectual activity in which individuals of pure negro race have not distinguished themselves, he draws the inference that there is no impassable limit between Caucasian and Negro which should unconditionally denote the one as the master of the other.

In 1816 Tiedemann accepted a call to Heidelberg, where he undertook the Professorship of Physiology, as well as that of Anatomy. His physiology was, like Haller's, founded on anatomy, observation, and experiment. It is true that, while a student at Würzburg, he was for a time an ardent hearer of Schelling, in the hope of a new illumination of biological science through the German "Naturphilosophie;" but his solid sense and the positive scientific progress he had already made soon led him to distrust the allurements of a vain system. It was in this practical spirit that, at a riper age, he joined with the accomplished chemist Leopold Gmelin in those elaborate researches which ended in the celebrated experimental Essay on Digestion (Die Verdauung, nach Versuchen, 1825), in which the names of the anatomist and chemist are so honourably associated. Tiedemann also entered on the laborious task of preparing a systematic work on physiology on an extended plan, somewhat after the manner of the 'Elementa' of Haller; and the first volume, embracing general physiology, was published in 1830. The issue of the third volume was prematurely forced upon him by the unauthorized publication of the corresponding part of his lectures; but the work was not further proceeded with, and the part published, although full of learning, and displaying other excellencies characteristic of the author, and although it was translated both into French and into English, at-

tracted comparatively little attention. At that time, indeed, the revived and improved use of the microscope in physiology, the more exact application of physical experiment, and other influential causes were about to introduce a mass of new materials into the science, and to involve a fresh handling of the old in any systematic treatise that should be up to the actual state of knowledge; and it is probable that advancing years and other cares indisposed the author to persevere in what had become an arduous task.

Tiedemann meanwhile did not intermit his labours in human and comparative anatomy. He had, in conjunction with Oppel and Liboschitz, projected a work on the anatomy and natural history of Reptiles; but, in consequence of the death of his collaborators, it went no further than the part on Crocodiles, which appeared in 1817. In 1820 he published a monography of the Ursine Sloth; in 1822 his 'Tabulæ Nervorum Uteri,' which he dedicated to the Royal Society, and in the same year came out the 'Tabulæ Arteriarum Corporis Humani.' This grand work, though not exactly faultless in every point, has, more than any other, contributed to spread abroad the name of Tiedemann; for the figures it contains have been reduced and copied and disseminated, in collections of anatomical plates and in anatomical systems and hand-books, in every part of the world where anatomy is taught. A supplement, containing additional figures of varieties in the distribution of the arteries, was published in 1846.

Besides the works noticed in this brief sketch, Tiedemann was the author of various minor essays and memoirs, published separately, or contributed to Transactions of Societies and Journals. He was also associated with the two brothers Treviranus in conducting the 'Zeitschrift für Physiologie.' A complete list of his writings is appended to a memoir of his life read before the Bavarian Academy of Sciences, by his son-in-law Professor Bischoff, from whence the facts in the present notice have been mainly derived.

In 1807 Tiedemann married the daughter of the Obervoght of Rastatt, whose family name was von Holzing; and with her he lived in happy union to the end of his days. She bore him seven children, of whom three survive him. Unhappily three of his sons were drawn into the revolutionary movement in Baden in 1848, for which the eldest paid the forfeit of his life, and the other two had to exile them-

selves from their fatherland. These deplorable events weighed all the more heavily on the old man, as his own political tendencies were strongly conservative. Added to these family afflictions, a growing infirmity of his eyesight induced Tiedemann to withdraw from public duty, and to retire in 1849 to Bremen, and thence soon after to Frankfort. There he met with a kind reception from numerous attached friends, and in 1854 he was honoured by the celebration of his "Doctor-Jubilæum," on the fiftieth anniversary of his promotion to the Degree of Doctor. On this occasion not only his immediate friends and countrymen, but men of science in all parts of the world joined in the general testimony of respect; and a medal, bearing his likeness, was struck in commemoration of the event. Having recovered a very serviceable degree of vision by a successful operation for cataract, he continued to enjoy his retirement, and to gratify his passion for natural scenery in summer excursions among the Bavarian Alps; but after a time he became subject to attacks of bronchitis, under which he finally sank on the 22nd of January, 1861.

Tiedemann, besides a thorough acquaintance with the literature of his special department, possessed a large stock of accurate general information—the fruit of diligent reading and personal observation, favoured by a singularly exact and capacious memory. His private as well as his public life was characterized throughout by elevated sentiment, manly independence, and strict integrity. We have already mentioned the special honour conferred upon him in his old age; and as a further evidence of the esteem in which he was held, we may add that he was elected a member of no less than sixty-two learned Academies and Societies. He was also raised to the rank of Privy Councillor by the Grand Duke of Baden, and received orders of Knighthood from Baden, Bavaria, Greece, and Prussia. His election to the Foreign Membership of the Royal Society took place in 1832.

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